BRITISH ASSOCIATION
FOR THE ADVANCEMENT
OF SCIENCE

REPORT

OF THE

CENTENARY MEETING

LONDON—1931
SEPTEMBER 23—30

LONDON
OFFICE OF THE BRITISH ASSOCIATION
BURLINGTON HOUSE, LONDON, W. 1

1932

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1831, Sept. 27	York	Viscount Milton, D.C.L., F.R.S.	—	—
1832, June 19	Oxford	The Rev. W. Buckland, F.R.S.	—	—
1833, June 25	Cambridge	The Rev. A. Sedgwick, F.R.S.	—	—
1834, Sept. 8	Edinburgh	Sir T. M. Brisbane, D.C.L., F.R.S.	—	—
1835, Aug. 10	Dublin	The Rev. Provost Lloyd, LL.D., F.R.S.	—	—
1836, Aug. 22	Bristol	The Marquis of Lansdowne, F.R.S.	—	—
1837, Sept. 11	Liverpool	The Earl of Burlington, F.R.S.	—	—
1838, Aug. 10	Newcastle-on-Tyne	The Duke of Northumberland, F.R.S.	—	—
1839, Aug. 26	Birmingham	The Rev. W. Vernon Harcourt, F.R.S.	—	—
1840, Sept. 17	Glasgow	The Marquis of Breadalbane, F.R.S.	—	—
1841, July 20	Plymouth	The Rev. W. Whewell, F.R.S.	169	65
1842, June 23	Manchester	The Lord Francis Egerton, F.G.S.	303	169
1843, Aug. 17	Cork	The Earl of Rosse, F.R.S.	109	28
1844, Sept. 26	York	The Rev. G. Peacock, D.D., F.R.S.	226	150
1845, June 19	Cambridge	Sir John F. W. Herschel, Bart., F.R.S.	313	36
1846, Sept. 10	Southampton	Sir Roderick I. Murchison, Bart., F.R.S.	241	10
1847, June 23	Oxford	Sir Robert H. Inglis, Bart., F.R.S.	314	18
1848, Aug. 9	Swansea	The Marquis of Northampton, Pres. R.S.	149	3
1849, Sept. 12	Birmingham	The Rev. T. R. Robinson, D.D., F.R.S.	227	12
1850, July 21	Edinburgh	Sir David Brewster, K.H., F.R.S.	235	9
1851, July 2	Ipswich	G. B. Airy, Astronomer Royal, F.R.S.	172	8
1852, Sept. 1	Belfast	Lieut.-General Sabine, F.R.S.	164	10
1853, Sept. 3	Hull	William Hopkins, F.R.S.	141	13
1854, Sept. 20	Liverpool	The Earl of Harrowby, F.R.S.	238	23
1855, Sept. 12	Glasgow	The Duke of Argyll, F.R.S.	194	33
1856, Aug. 6	Cheltenham	Prof. C. G. B. Daubeny, M.D., F.R.S.	182	14
1857, Aug. 26	Dublin	The Rev. H. Lloyd, D.D., F.R.S.	236	15
1858, Sept. 22	Leeds	Richard Owen, M.D., D.C.L., F.R.S.	222	42
1859, Sept. 14	Aberdeen	H.R.H. The Prince Consort	184	27
1860, June 27	Oxford	The Lord Wrottesley, M.A., F.R.S.	286	21
1861, Sept. 4	Manchester	William Fairbairn, LL.D., F.R.S.	321	113
1862, Oct. 1	Cambridge	The Rev. Professor Willis, M.A., F.R.S.	239	15
1863, Aug. 26	Newcastle-on-Tyne	Sir William G. Armstrong, C.B., F.R.S.	203	36
1864, Sept. 13	Bath	Sir Charles Lyell, Bart., M.A., F.R.S.	287	40
1865, Sept. 6	Birmingham	Prof. J. Phillips, M.A., LL.D., F.R.S.	292	44
1866, Aug. 22	Nottingham	William R. Grove, Q.C., F.R.S.	207	31
1867, Sept. 4	Dundee	The Duke of Buccleuch, K.C.B., F.R.S.	167	25
1868, Aug. 19	Norwich	Dr. Joseph D. Hooker, F.R.S.	196	18
1869, Aug. 18	Exeter	Prof. G. G. Stokes, D.C.L., F.R.S.	204	21
1870, Sept. 14	Liverpool	Prof. T. H. Huxley, LL.D., F.R.S.	314	39
1871, Aug. 2	Edinburgh	Prof. Sir W. Thomson, LL.D., F.R.S.	246	28
1872, Aug. 14	Brighton	Dr. W. B. Carpenter, F.R.S.	245	36
1873, Sept. 17	Bradford	Prof. A. W. Williamson, F.R.S.	212	27
1874, Aug. 19	Belfast	Prof. J. Tyndall, LL.D., F.R.S.	162	13
1875, Aug. 25	Bristol	Sir John Hawkshaw, F.R.S.	239	36
1876, Sept. 6	Glasgow	Prof. T. Andrews, M.D., F.R.S.	221	35
1877, Aug. 15	Plymouth	Prof. A. Thomson, M.D., F.R.S.	173	19
1878, Aug. 14	Dublin	W. Spottiswoode, M.A., F.R.S.	201	18
1879, Aug. 20	Sheffield	Prof. G. J. Allman, M.D., F.R.S.	184	16
1880, Aug. 25	Swansea	A. C. Ramsay, LL.D., F.R.S.	144	11
1881, Aug. 31	York	Sir John Lubbock, Bart., F.R.S.	272	28
1882, Aug. 23	Southampton	Dr. C. W. Siemens, F.R.S.	178	17
1883, Sept. 19	Southport	Prof. A. Cayley, D.C.L., F.R.S.	203	60
1884, Aug. 27	Montreal	Prof. Lord Rayleigh, F.R.S.	235	20
1885, Sept. 9	Aberdeen	Sir Lyon Playfair, K.C.B., F.R.S.	225	18
1886, Sept. 1	Birmingham	Sir J. W. Dawson, C.M.G., F.R.S.	314	25
1887, Aug. 31	Manchester	Sir H. E. Roscoe, D.C.L., F.R.S.	428	86
1888, Sept. 5	Bath	Sir F. J. Bramwell, F.R.S.	266	36
1889, Sept. 11	Newcastle-on-Tyne	Prof. W. H. Flower, C.B., F.R.S.	277	20
1890, Sept. 3	Leeds	Sir F. A. Abel, C.B., F.R.S.	259	21
1891, Aug. 19	Cardiff	Dr. W. Huggins, F.R.S.	189	24
1892, Aug. 3	Edinburgh	Sir A. Geikie, LL.D., F.R.S.	280	14
1893, Sept. 13	Nottingham	Prof. J. S. Burdon Sanderson, F.R.S.	201	17
1894, Aug. 8	Oxford	The Marquis of Salisbury, K.G., F.R.S.	327	21
1895, Sept. 11	Ipswich	Sir Douglas Galton, K.C.B., F.R.S.	214	13
1896, Sept. 16	Liverpool	Sir Joseph Lister, Bart., Pres. R.S.	330	31
1897, Aug. 18	Toronto	Sir John Evans, K.C.B., F.R.S.	120	8
1898, Sept. 7	Bristol	Sir W. Crookes, F.R.S.	281	19
1899, Sept. 13	Dover	Sir Michael Foster, K.C.B., Sec.R.S.	296	20

* Ladies were not admitted by purchased tickets until 1843. † Tickets of Admission to Sections only.

[*Continued on p.* xiv.

ANNUAL MEETINGS.

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
—	—	—	—	—	353	—	—	1831
—	—	—	—	—	—	—	—	1832
—	—	—	—	—	900	—	—	1833
—	—	—	—	—	1298	—	£20 0 0	1834
—	—	—	—	—	—	—	167 0 0	1835
—	—	—	—	—	1350	—	435 0 0	1836
—	—	—	—	—	1840	—	922 12 6	1837
—	—	—	1100*	—	2400	—	932 2 2	1838
—	—	—	—	34	1438	—	1595 11 0	1839
—	—	—	—	40	1353	—	1546 16 4	1840
46	317	—	60*	—	891	—	1235 10 11	1841
75	376	33†	331*	28	1315	—	1449 17 8	1842
71	185	—	160	—	—	—	1565 10 2	1843
45	190	9†	260	—	—	—	981 12 8	1844
94	22	407	172	35	1079	—	831 9 9	1845
65	39	270	196	36	857	—	685 16 0	1846
197	40	495	203	53	1320	—	208 5 4	1847
54	25	376	197	15	819	£707 0 0	275 1 8	1848
93	33	447	237	22	1071	963 0 0	159 19 6	1849
128	42	510	273	44	1241	1085 0 0	345 18 0	1850
61	47	244	141	37	710	620 0 0	391 9 7	1851
63	60	510	292	9	1108	1085 0 0	304 6 7	1852
56	57	367	236	6	876	903 0 0	205 0 0	1853
121	121	765	524	10	1802	1882 0 0	380 19 7	1854
142	101	1094	543	26	2133	2311 0 0	480 16 4	1855
104	48	412	346	9	1115	1098 0 0	734 13 9	1856
156	120	900	569	26	2022	2015 0 0	507 15 4	1857
111	91	710	509	13	1698	1931 0 0	618 18 2	1858
125	179	1206	821	22	2564	2782 0 0	684 11 1	1859
177	59	636	463	47	1689	1604 0 0	766 19 6	1860
184	125	1589	791	15	3138	3944 0 0	1111 5 10	1861
150	57	433	242	25	1161	1089 0 0	1293 16 6	1862
154	209	1704	1004	25	3335	3640 0 0	1608 3 10	1863
182	103	1119	1058	13	2802	2965 0 0	1289 15 8	1864
215	149	766	508	23	1997	2227 0 0	1591 7 10	1865
218	105	960	771	11	2303	2469 0 0	1750 13 4	1866
193	118	1163	771	7	2444	2613 0 0	1739 4 0	1867
226	117	720	682	45‡	2004	2042 0 0	1940 0 0	1868
229	107	678	600	17	1856	1931 0 0	1622 0 0	1869
303	195	1103	910	14	2878	3096 0 0	1572 0 0	1870
311	127	976	754	21	2463	2575 0 0	1472 2 6	1871
280	80	937	912	43	2533	2649 0 0	1285 0 0	1872
237	99	796	601	11	1983	2120 0 0	1685 0 0	1873
232	85	817	630	12	1951	1979 0 0	1151 16 0	1874
307	93	884	672	17	2248	2397 0 0	960 0 0	1875
331	185	1265	712	25	2774	3023 0 0	1092 4 2	1876
238	59	446	283	11	1229	1268 0 0	1128 9 7	1877
290	93	1285	674	17	2578	2615 0 0	725 16 6	1878
239	74	529	349	13	1404	1425 0 0	1080 11 11	1879
171	41	389	147	12	915	899 0 0	731 7 7	1880
313	176	1230	514	24	2557	2689 0 0	476 8 1	1881
253	79	516	189	21	1253	1286 0 0	1126 1 11	1882
330	323	952	841	5	2714	3369 0 0	1083 3 3	1883
317	219	826	74	26 & 60 H.§	1777	1855 0 0	1173 4 0	1884
332	122	1053	447	6	2203	2256 0 0	1385 0 0	1885
428	179	1067	429	11	2453	2532 0 0	995 0 6	1886
510	244	1985	493	92	3838	4336 0 0	1186 18 0	1887
399	100	639	509	12	1984	2107 0 0	1511 0 5	1888
412	113	1024	579	21	2437	2441 0 0	1417 0 11	1889
368	92	680	334	12	1775	1776 0 0	789 16 8	1890
341	152	672	107	35	1497	1664 0 0	1029 10 0	1891
413	141	733	439	50	2070	2007 0 0	864 10 0	1892
328	57	773	268	17	1661	1653 0 0	907 15 6	1893
435	69	941	451	77	2321	2175 0 0	583 15 6	1894
290	31	493	261	22	1324	1236 0 0	977 15 5	1895
383	139	1384	873	41	3181	3228 0 0	1104 6 1	1896
286	125	682	100	41	1362	1398 0 0	1059 10 8	1897
327	96	1051	639	33	2446	2399 0 0	1212 0 0	1898
324	68	548	120	27	1403	1328 0 0	1430 14 2	1899

‡ Including Ladies. § Fellows of the American Association were admitted as Hon. Members for this Meeting.

[*Continued on p.* xv.

Table of

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1900, Sept. 5	Bradford	Sir William Turner, D.C.L., F.R.S. ...	267	13
1901, Sept. 11	Glasgow...................	Prof. A. W. Rücker, D.Sc., Sec.R.S. ...	310	37
1902, Sept. 10	Belfast	Prof. J. Dewar, LL.D., F.R.S.	243	21
1903, Sept. 9	Southport	Sir Norman Lockyer, K.C.B., F.R.S.	250	21
1904, Aug. 17......	Cambridge...............	Rt. Hon. A. J. Balfour, M.P., F.R.S.	419	32
1905, Aug. 15......	South Africa	Prof. G. H. Darwin, LL.D., F.R.S. ...	115	40
1906, Aug. 1	York	Prof. E. Ray Lankester, LL.D., F.R.S.	322	10
1907, July 31	Leicester	Sir David Gill, K.C.B., F.R.S.	276	19
1908, Sept. 2	Dublin	Dr. Francis Darwin, F.R.S.	294	24
1909, Aug. 25......	Winnipeg	Prof. Sir J. J. Thomson, F.R.S.	117	13
1910, Aug. 31	Sheffield..................	Rev. Prof. T. G. Bonney, F.R.S.	293	26
1911, Aug. 30	Portsmouth	Prof. Sir W. Ramsay, K.C.B., F.R.S.	284	21
1912, Sept. 4	Dundee	Prof. E. A. Schäfer, F.R.S..............	288	14
1913, Sept. 10	Birmingham	Sir Oliver J. Lodge, F.R.S...............	376	40
1914, July-Sept....	Australia	Prof. W. Bateson, F.R.S.	172	13
1915, Sept. 7	Manchester	Prof. A. Schuster, F.R.S.	242	19
1916, Sept. 5	Newcastle-on-Tyne...	Sir Arthur Evans, F.R.S.	164	12
1917	(No Meeting)		—	—
1918	(No Meeting)		—	—
1919, Sept. 9	Bournemouth	Hon. Sir C. Parsons, K.C.B., F.R.S....	235	47
1920, Aug. 24......	Cardiff	Prof. W. A. Herdman, C.B.E., F.R.S.	288	11
1921, Sept. 7	Edinburgh	Sir T. E. Thorpe, C.B., F.R.S.	336	9
1922, Sept. 6	Hull	Sir C. S. Sherrington, G.B.E., Pres. R.S.	228	13
1923, Sept. 12	Liverpool	Sir Ernest Rutherford, F.R.S.	326	12
1924, Aug. 6	Toronto	Sir David Bruce, K.C.B., F.R.S.......	119	7
1925, Aug. 26	Southampton	Prof. Horace Lamb, F.R.S.	280	8
1926, Aug. 4	Oxford	H.R.H. The Prince of Wales, K.G., F.R.S.	358	9
1927, Aug. 31	Leeds	Sir Arthur Keith, F.R.S.	249	9
1928, Sept. 5	Glasgow..................	Sir William Bragg, K.B.E., F.R.S. ...	260	10
1929, July 22......	South Africa	Sir Thomas Holland, K.C.S.I., K.C.I E., F.R.S.	81	1
1930, Sept. 3	Bristol	Prof. F. O. Bower, F.R.S.	221	5
1931, Sept. 23......	London	Gen. the Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S.	487	14

[1] Including 848 Members of the South African Association.
[2] Including 137 Members of the American Association.
[3] Special arrangements were made for Members and Associates joining locally in Australia, see Report, 1914, p. 686. The numbers include 80 Members who joined in order to attend the Meeting of L'Association Française at Le Havre.
[4] Including Students' Tickets, 10*s*.
[5] Including Exhibitioners granted tickets without charge.

Annual Meetings—(continued).

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
297	45	801	482	9	1915	£1801 0	£1072 10 0	1900
374	131	794	246	20	1912	2046 0	920 9 11	1901
314	86	647	305	6	1620	1644 0	947 0 0	1902
319	90	688	365	21	1754	1762 0	845 13 2	1903
449	113	1338	317	121	2789	2650 0	887 18 11	1904
937[1]	411	430	181	16	2130	2422 0	928 2 2	1905
356	93	817	352	22	1972	1811 0	882 0 9	1906
339	61	659	251	42	1647	1561 0	757 12 10	1907
465	112	1166	222	14	2297	2317 0	1157 18 8	1908
290[2]	162	789	90	7	1468	1623 0	1014 9 9	1909
379	57	563	123	8	1449	1439 0	963 17 0	1910
349	61	414	81	31	1241	1176 0	922 0 0	1911
368	95	1292	359	88	2504	2349 0	845 7 6	1912
480	149	1287	291	20	2643	2756 0	978 17 1	1913
139	4160[3]	539[3]	—	21	5044[3]	4873 0	1861 16 4[6]	1914
287	116	628[4]	141	8	1441	1406 0	1569 2 8	1915
250	76	251[4]	73	—	826	821 0	985 18 10	1916
—	—	—	—	—	—	—	677 17 2	1917
—	—	—	—	—	—	—	326 13 3	1918
254	102	688[4]	153	3	1482	1736 0	410 0 0	1919

Old Annual Regular Members	Annual Members: Meeting and Report	Annual Members: Meeting only	Transferable Tickets	Students Tickets	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
136	192	571	42	120	20	1380	1272 10	1251 13 0[8]	1920
133	410	1394	121	343	22	2768	2599 15	518 1 10	1921
90	294	757	89	235[5]	24	1730	1699 5	772 0 7	1922
					Complimentary[7]				
123	380	1434	163	550	308	3296	2735 15	777 18 6[9]	1923
37	520	1866	41	89	139	2818	3165 19[10]	1197 5 9	1924
97	264	878	62	119	74	1782	1630 5	1231 0 0	1925
101	453	2338	169	225	69	3722	3542 0	917 1 6	1926
84	334	1487	82	264	161	2670	2414 5	761 10 0	1927
76	554	1835	64	201	74	3074	3072 10	1259 10 0	1928
24	177	1227[11]	—	161	83	1754	1477 15	1838 2 1	1929
68	310	1617	97	267	54	2639	2481 15	683 5 7	1930
78	656	2994	157	454	449	5702[12]	4792 10	1146 7 6	1931

[6] Including grants from the Caird Fund in this and subsequent years.
[7] Including Foreign Guests, Exhibitioners, and others.
[8] The Bournemouth Fund for Research, initiated by Sir C. Parsons, enabled grants on account of scientific purposes to be maintained.
[9] Including grants from the Caird Gift for research in radioactivity in this and subsequent years to 1926.
[10] Subscriptions paid in Canada were $5 for Meeting only and others pro rata ; there was some gain on exchange.
[11] Including 450 Members of the South African Association.
[12] Including 413 tickets for certain meetings, issued at 5*s*. to London County Council school-teachers.

NARRATIVE OF THE CENTENARY MEETING.

The Reception Room and Offices were established in the University of London, Imperial Institute Road.

The President's Installation and Address.

On Wednesday afternoon, September 23, in the Albert Hall (Faraday Centenary Exhibition[1]), at 3.0 p.m., Gen. the Right Hon. J. C. Smuts, P.C., C.H., F.R.S., assumed the Presidency of the Association in succession to Prof. F. O. Bower, F.R.S., and received the invited Delegates of Societies and Institutions, and of Universities, Colleges, and Cities in which the Association had held meetings in the past.

A humble Address was forwarded to His Majesty The King, Patron of the Association :—

'On the occasion of the centenary of the foundation of the British Association for the Advancement of Science, we, the Members assembled at the first meeting held in London, desire to express our humble respects and loyal devotion to Your Majesty.

'To this Centenary Meeting have also come representatives of Science from the Dominions and other parts of the Empire, who join with their British colleagues in giving an Imperial character to this great occasion.

'We recall with gratitude the words of encouragement which Your Majesty has repeatedly addressed to our Association, as well as the Royal Charter of Incorporation with which you have honoured us. We also bear in grateful memory the services rendered to our Association by the Prince Consort as the President of its Meeting at Aberdeen in 1859, and by the Prince of Wales as the President of its Meeting at Oxford in 1926.

'And we desire to express to Your Majesty our loyal determination to devote our future efforts to the advancement of Science and to the application of its teachings to the welfare and prosperity of Your Majesty's peoples and the world at large.'

The following gracious Reply was received, and was communicated to the Members at the evening meeting referred to below :—

Balmoral Castle.

General The Right Honble. J. C. Smuts, F.R.S.,
President, Centenary Meeting, British Association
for the Advancement of Science.

'At the Centenary Meeting, under your Presidency, of the British Association for the Advancement of Science, I warmly thank all those present for their loyal message. It gives me much pleasure to learn that many representatives from all parts of my Empire are with you on this great occasion.

[1] The Faraday Centenary Exhibition was open from 10 a.m. to 2.30 p.m. on Wednesday, September 23, for private view by Members of the Association, thanks to the kind co-operation of the authorities concerned.

'A hundred years ago your first meeting was held in York. Ever since that memorable September evening the British Association has steadily advanced, and you can truly say that the roll of its members is bright with names that the world will never forget. Although we live in times fraught with difficulties, scientific progress does not slacken, and I know that the contributions to all branches of Science made by your world-renowned members of the past are continued to-day by many distinguished men.'

George R.I.

At the Inauguration of the President the following speeches were delivered :—

Prof. F. O. Bower, Sc.D., D.Sc., LL.D., F.R.S.

'It falls to me, as President in the hundredth year of the British Association, to perform the last act of a vanishing century. It is usual for an outgoing President on vacating office, in the name of the Council and of the General Committee to invite his successor to take the Chair. To-day we stand on the border-line not merely of two official years, but between two centuries of scientific activity. Naturally, we shall be looking forward, but for a moment we should also look back. During the past century the Association has done a great work for Science and for Mankind. In its earlier days it moved only within the narrow circle of the British Isles: but as the years passed, and the evolution of the Empire progressed, it began to cultivate a wider field, by meeting occasionally in one or another of the great Dominions. In this the Association was gradually assuming an Imperial rather than an insular function. In preparing for this Centenary Meeting the General Committee and the Council desired to mark their sense of widening aspirations by looking overseas for their new President. By a general consensus of opinion the choice fell upon one who represents the Dominion of South Africa, and whose name carries great weight in varied fields. I have now the honour of asking the Right Honourable Jan Christiaan Smuts—lawyer, soldier, statesman, and philosophical exponent of scientific theory—to take the Chair: and in so doing to inaugurate a new century of existence of the British Association.'

Gen. The Rt. Hon. J. C. Smuts, P.C., C.H., D.Sc., F.R.S.

'I very much appreciate the extreme kindness with which Professor Bower has inducted me to this Chair. I feel a special pleasure and pride in following him here. For half a lifetime I have followed him as a foremost exponent of the science which we both love above all others; and though it is not botany that has brought me here, there is this bond between us, which will be strengthened by the events of to-day.

'But how could I adequately thank this Association for the honour they have conferred on me of presiding over this historic Meeting? The Presidency of the British Association is justly one of the most highly coveted distinctions of British science, and it has been held by the greatest scientists of the last hundred years. The uniqueness of this occasion only adds to the distinction. I am consoled by the thought that I did not covet this lofty eminence, and as Lord Melbourne said of another form of

honour, there was no personal merit implied in my election. I stand here in a representative capacity. My being thus singled out can stir no feeling of envy, and only calls for sympathy on the part of those who know the difficulties confronting an amateur in this exalted position.

'May I be personal a moment longer? This day, forty years ago, I sailed from South Africa to continue my studies at a British University. What floods of history have since poured over the world! Much has happened in my own personal life. But nothing can equal this occasion where, in your desire to mark the Imperial character of this Centenary Meeting, you have chosen me as a Dominion representative to preside over it. It is the crowning honour of my life. South Africa looks upon this as an honour done to herself and as part of the romance of her own story. The sister Dominions appreciate this act of courtesy in which they all share. The British Empire has given yet another instance of that breadth of conception and human sympathy which has made it the greatest and most beneficent political society that has ever existed.

'Your Association has, during the century now passed, run a course not unlike that of the Empire on a smaller scale. The sapling, which was planted at York a hundred years ago, has grown into the great tree Yggdrasil whose roots penetrate all the continents, whose branches cover the Empire overseas. Your Association has become the parent of similar science associations in most of the Dominions, and this Centenary Meeting has truly become a great family reunion. The parent Association has spared no effort or expense in making this a representative gathering of British Science from all parts of the world. This wonderful Meeting is a proof of its success. I extend a special welcome to my fellow-citizens from the Dominions and the outer marches of the Empire. For them as for me, this will be one of the golden memories of our lives.

'I also extend a cordial and warm welcome to the many distinguished foreign scientists from Europe, Asia and America, who have honoured us with their presence. Science knows no political boundaries. More and more it is becoming a collective collaboration among the scientific workers of all nations for the common good of mankind. Science is universal and recognises no frontiers except those of reason and fact. And it is destined, perhaps more than any other form of human activity, to draw the nations together, to reconstitute their broken unity, and to give form and substance to that ideal of mankind as one human family, which science has itself done most to reveal as a fact.

'What shall I say on this introductory occasion? My main address will be given to-night. The circumstances of the inception of this Association a century ago I shall more appropriately refer to at York next Saturday--the place and date of the first Meeting. Time does not permit of a statement to-day of the great advances of science during the century that has elapsed since. I may, however, be permitted to say a few words here on the general situation out of which the Association arose a hundred years ago, and contrast it with that of to-day. And it is also fitting for me to refer to the special function which this Association has performed in the advancement of science during that century.

'Like to-day, 1831 was a time of grave economic and political confusion and unsettlement. The situation sixteen years after the Napoleonic Wars

was not very different from the situation now, thirteen years after the Great War, although the mischiefs of to-day reach farther and go much deeper. It was, like ours, a time of transition from one era, from one order of things to another, with all the dislocation and unrest and social suffering which such a transition implies. Men could not then foresee the great Victorian era on whose threshold they were already standing, and they were filled with fears and dark forebodings of the future. The agitation which led to the great Reform Act the following year had reached its climax. The prospects of British science no less than of British industry appeared sombre even to those best informed. Sir David Brewster, perhaps the leading figure in the British science of that day, painted in that year a gloomy picture—of England alone among the nations lagging behind in the race of recovery and reconstruction, of artisans quitting her shores and service, of machinery and inventions being exported to distant markets, and of the best arts and industries of England being lost to other nations.

'But the situation was not really as black as it was painted: it seldom is. In the first place, in the Reform Act of 1832 was found the master key of democracy, which was to open and render possible the orderly political evolution of the Victorian age. In the second place, science was definitely coming on the scene as a force to be reckoned with. At the very time of Brewster's jeremiad, Michael Faraday was successfully solving the principle of the dynamo, and thereby opening up a source of power which was to revolutionise the industries of the future. In that year also, and largely on the initiative of Brewster himself, this Association for the Advancement of Science was founded. At the darkest hour before the dawn Science thus came forward as the new reinforcement, the great new factor in the economic reconstruction of the world, and the immense material progress of the nineteenth century has been largely due to it. Drawing the parallel for to-day, one may hesitate about the next step in political development, but there is no room for doubt that science, now even more than in 1831, is the dominant factor in the industrial and economic revival, and that in the penetration of science into all avenues of human activity lies our main hope of future advance.

'The British Association was founded on a successful German model and had for its two main objects the fostering of intercourse among workers in science, and the creation of a platform for propaganda purposes from which the progress and discoveries in science could be brought to the notice of the public. Several of our great expert scientific societies were already in existence, but something more was wanted, which would bring their members together in a common association and co-operation, and would at the same time link them up with the large lay public which was more and more becoming interested in science and its discoveries. It was in a sense a realisation of Bacon's prophetic vision in the New Atlantis, of some future philosophical academy of the pioneers and researchers in knowledge, which would foster the spread of science by arranging 'circuits or visits of the divers cities of the Kingdom.' Like all novel ideas, it met with a good deal of active or covert opposition; the press laughed at it, and famous writers like Dickens poured ridicule on it. But the idea was sound and met a real need, and it could count from the first

on the active support of many of the leading men in British science. It was, therefore, bound to succeed. Men like Brewster, Dalton, Robert Brown, Faraday, Lyell, Murchison, Whewell, Sedgwick and Forbes, took a prominent part in its proceedings and gave prestige to the infant Association. At a later date, most of the great figures in British science took an active part in its work; and with a few exceptions, the list of Presidents reads like a roll-call of all that is most distinguished in British science. The Association has served its purpose admirably as an effective sounding board of the scientific advance. Here were fought out great controversies, like that over Darwin's theory of descent, and the din of battle helped to give impetus to the new views. Here great discoveries were announced; here Joule explained his epoch-making researches into the mechanical equivalent of heat; here Rayleigh and Ramsay announced the discovery of argon which led to the discovery of the other inert elements; here Fitzgerald first announced Hertz's verification of Clerk Maxwell's theory of electro-magnetic waves; here Sir Oliver Lodge gave the first public demonstration of wireless; most epoch-making of all, here Sir Joseph Thomson announced his discovery of the electron. I could greatly extend this list of famous discoveries announced or proclaimed at meetings of the Association, but the foregoing will suffice.

'Nor has the Association been content to cater for science in these islands only. From the eighties of last century onward, it has extended its mission to the Dominions, having visited Canada four times, South Africa twice, and Australia once. These Dominion visits have not been among the least fruitful and valuable meetings of the Association. Dominion workers have thereby been inspired and stimulated in a way which would not have been possible otherwise; lines of research in the Dominions have been suggested and started which have led to fruitful results. British men of science, again, have come into contact with the wider problems presented by Dominion conditions, and have had to adjust their views to new and larger situations. The exchanges of science at these Dominion meetings have thus been mutually helpful, and apart from the purely scientific results, these meetings have served a useful purpose in stimulating the sense of fellow-feeling and comradeship in the Empire as a whole. The response is seen in this great gathering, which in a sense represents a return visit of the Dominion Associations to the Mother Association, and a symposium of Empire science in the widest sense.

'Is it too much to hope that from this great gathering of science will go forth a new message of hope to this Empire and to a world distracted and labouring in unprecedented troubles? Science has come to represent the growing point of the human advance. It stands for the new forces which are reshaping this world of ours. It faces the future with a bold and confident spirit. It has an invincible faith in truth at all costs; and in that faith it is embarked on the endless adventure which carries the future of the human race. May its confident spirit and sublime faith bring new inspiration to the peoples, and give them courage and strength for the grave tasks ahead.'

Delegates were invited and received from the following Universities, Cities and Institutions :—

The University of Aberdeen.
The University of Adelaide.
The University of Alberta.
The Queen's University, Belfast.
The University of Birmingham.
The University of Bristol.
The University of Cambridge.
*The University of Cape Town.
The University of Edinburgh.
The University of Glasgow.
The University of Leeds.
The University of Liverpool.
The University of London.
The University of Manchester.
The University of Manitoba.
The University of Melbourne.
*The McGill University, Montreal.
The University of Oxford.
The University of Reading.
*The University of St. Andrews.
The University of Saskatchewan.
The University of Sheffield.
*The University of South Africa.
The University of Stellenbosch.
The University of Sydney.
The University of Toronto.
The University of Wales.
*The University of the Witwatersrand.
Armstrong College, Newcastle-on-Tyne.
The University College, Hull.
The University College, Leicester.
The University College, Nottingham.
The University College, Southampton.
The University of Durham, College of Medicine, Newcastle-upon-Tyne.
Trinity College, Dublin.
The City of Aberdeen.
The City of Adelaide.
The City of Bath.
The City of Belfast.
The City of Birmingham.
The Town of Bournemouth.
The Town of Brighton.
The City of Bristol.
The City of Cape Town.
The Town of Cambridge.
The City of Cardiff.
The Town of Cheltenham.
The City of Edinburgh.
The City of Glasgow.
The City of Hull.
The Town of Ipswich.
*The City of Johannesburg.
The City of Leeds.
The City of Leicester.
The City of Liverpool.
The City of Manchester.
The City of Newcastle.
The City of Nottingham.
The City of Oxford.
The City of Plymouth.
The City of Portsmouth.
The Town of Southampton.
The Town of Swansea.
The City of Toronto.
The City of York.

Academia Română.
Associação Portuguesa para o Progresso das Sciências.
Česká Akademie věd a Umění.
Det Kongelige Danske Videnskabernes Selskab.
Det Norske Videnskaps-Akademi.
Die Akademie der Wissenschaften in Wien.
Die Bayerische Akademie der Wissenschaften.
*Die Gesellschaft Deutscher Naturforscher und Aertze.
Die Sächsische Akademie der Wissenschaften.
*Finska Vetenskaps-Societeten.
Jugoslavenska Akademija Znanosti i Umjetnosti.
Koninklijke Akademie van Wettenschappen te Amsterdam.
*Kungl. Svenska Vetenskapsakademien.
L'Académie des Sciences (Institut de France).
L'Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique.
L'Association Français pour l'Avancement des Sciences.
La Reale Accademia D'Italia.
La Societa Italiana per il Progresso Delle Scienze.
Magyar Tudományos Akadémia.
The Academy of Athens.
The African Society.
The American Association for the Advancement of Science.
The Anatomical Society of Great Britain and Ireland.

The Association of Economic Biologists.
The Australian and New Zealand Association for the Advancement of Science.
The Biochemical Society.
The British Academy.
The British Astronomical Association.
The British Medical Association.
The British Ornithologists' Union.
The British Science Guild.
The Chemical Society.
The Entomological Society of London.
The Eugenics Society.
The Faraday Society.
The Folk-lore Society.
The Geographical Association.
The Geological Society of London.
The Imperial Academy of Japan.
The Incorporated Association of Headmasters in Secondary Schools.
The Incorporated Association of Headmistresses in Secondary Schools.
The Incorporated Association of Assistant Mistresses in Secondary Schools.
*The Institute of Chemistry of Great Britain and Ireland.
The Institute of Fuel.
The Institute of Metals.
The Institute of Physics.
*The Institution of Civil Engineers.
The Institution of Electrical Engineers.
The Institution of Engineers and Shipbuilders in Scotland.
The Institution of Mechanical Engineers.
The Institution of Mining and Metallurgy.
The International Research Council.
The Iron and Steel Institute.
The Japanese Association for the Advancement of Science.
The Linnean Society of London.
The London Mathematical Society.
The Marine Biological Association of the United Kingdom.
The Mineralogical Society.
The Museums Association.
The National Academy of Sciences.
The National Institute of Industrial Psychology.
The National Union of Teachers.
*The New Zealand Institute.
The North-East Coast Institution of Engineers and Shipbuilders.
The Optical Society.
The Physical Society of London.
The Physiological Society.
The Royal Aeronautical Society.
The Royal Agricultural Society of England.
The Royal Anthropological Institute.
The Royal Astronomical Society.
The Royal Canadian Institute.
The Royal College of Physicians.
The Royal College of Surgeons of England.
*The Royal Dublin Society.
The Royal Economic Society.
The Royal Empire Society.
The Royal Geographical Society.
*The Royal Horticultural Society.
The Royal Institution of Great Britain.
The Royal Irish Academy.
The Royal Meteorological Society.
The Royal Microscopical Society.
The Royal Philosophical Society of Glasgow.
The Royal Sanitary Institute.
The Royal Society.

The Royal Society for the Protection of Birds.
The Royal Society of Arts.
The Royal Society of Canada.
The Royal Society of Edinburgh.
The Royal Society of Medicine.
The Royal Society of New South Wales.
The Royal Society of Queensland.
The Royal Society of South Africa.
The Royal Society of South Australia.
The Royal Society of Tropical Medicine and Hygiene.
The Royal Society of Victoria.
The Royal Statistical Society.
The Smithsonian Institution.
The Society for the Preservation of the Fauna of the Empire.
The Society of Antiquaries of London.
The Society of Chemical Industry.
*The South African Association for the Advancement of Science.
The Textile Institute.
The Universities Bureau of the British Empire.
The Zoological Society of London.

The Council had intimated that addresses of congratulation were not to be regarded as obligatory, but those Institutions and Cities which are marked with an asterisk above kindly forwarded addresses, together with the following :—

Preussische Akademie der Wissenschaften.

Delegates from the following Universities and Institutions were also present :—

The Columbia University of New York.
The Swarthmore College.
The Muslim University of Aligarh.
The University of Andhra.
The University of Dacca.
The University of Delhi.
The University of Helsingfors.
The University of Lucknow.
The University of Madras.
The University of the Punjab.
La Société Chimique de France.
Sociedad Española de Fisiska y Quimica.
The Asiatic Society of Bengal.
The Royal Asiatic Society, Bombay Branch.

On Wednesday evening, September 23, in the Central Hall, Westminster, at 9.0 p.m., General Smuts delivered the Presidential Address, entitled 'The Scientific World-Picture of To-day,' for which see p. 1. The Address was relayed to three other halls in the same building

The President sent the following message to the audiences in the relay halls :—

'My successors at the Bi-Centenary Meeting in 2031 will no doubt be not only heard but also seen by whatever audience may desire to see and hear him (or her), without being compelled to come to any particular place for the purpose. To those whom I shall not have the pleasure of facing to-night while giving the Presidential Address, I can only express my regret that Science has not so far advanced as to permit the Officers of the Association to make better provision on this occasion. They had

no choice but to accommodate you here, and I know that no one is more painfully conscious of the fact than they. May I recommend those of you who may be disappointed on this or any other occasion during the meeting, to study the voluminous programme in the hope that if you are denied your first choice you may yet be able to enjoy the second.'

The President visited the audiences in the relay halls at the conclusion of the proceedings.

The Address was broadcast, and a part of it was recorded, and, with the kind co-operation of the Gramophone Company, records have been placed on sale by the Association.

An organ recital was given in the Central Hall from 8.20 to 9 p.m. by Mr. A. L. Harris, A.R.C.O.

A vote of thanks for the Address was proposed by Sir F. Gowland Hopkins, President of the Royal Society. Sir Josiah Stamp, G.B.E., General Treasurer of the Association, announced the number of tickets issued for the Meeting, and referred to the Centenary Fund.

Sectional Meeting-rooms.

The normal meeting-rooms of the Sections are listed below; but certain sessions were held in the Great Hall of the University of London and in Jehanghir Hall, and the discussion on the Evolution of the Universe in Section A was held in the Central Hall, Westminster.

Section	
A (*Math. and Phys. Sciences*) (Main Section)	Imperial College of Science and Technology (main building), Physics Theatre.
(Department of Mathematics, A*)	Do., Astrophysics Room.
(Department of Cosmical Physics, A†)	Do., Small Physics Theatre.
B (*Chemistry*) . . .	Imperial College of Science and Technology (main building), Chemistry Lecture Theatre.
C (*Geology*) . . .	Royal School of Mines, Room 253.
D (*Zoology*) . . .	Natural History Museum, Reptile Gallery.
E (*Geography*) . . .	Royal Geographical Society, new Hall.
F (*Economic science and Statistics*)	City and Guilds Engineering College, Room 215.
Do. (Dept. of Industrial Co-operation, F*)	Royal School of Mines, Room 160.
G (*Engineering*) . . .	City and Guilds Engineering College, Room 04.
H (*Anthropology*) . .	Royal College of Science.
I (*Physiology*) . . .	Royal School of Mines, Room 53.
J (*Psychology*) . . .	City and Guilds Engineering College, Room 17, and Room 15 when Section divided.

Section	
K (*Botany*) . . .	Royal School of Mines, Room 153, and City and Guilds Engineering College, Room 128, when Section divided.
Do. (*Dept. of Forestry*, K*)	Botany Dept., Prince Consort Road.
L (*Educational Science*) .	Victoria and Albert Museum.
M (*Agriculture*) . . .	Imperial College of Science and Technology (main building), Physical Chemistry Lecture Theatre.
Do. (Subsection of Horticulture, M*)	Organic Chemistry Lecture Theatre.

The Conference of Delegates of Corresponding Societies met in Jehanghir Hall.

EVENING DISCOURSES, PUBLIC LECTURES, ETC.

Evening discourses to Members were given as follows:

Thursday, September 24.

Prof. W. A. Bone, F.R.S., 'The Photographic Analysis of Explosion Flames.' In the hall of the Royal Geographical Society, 9 p.m.

Sir P. Chalmers Mitchell, C.B.E., F.R.S., '"Zoos" and National Parks.' In the hall of the Royal College of Music, 9 p.m.

Saturday, September 26.

Sir Arthur Keith, F.R.S., 'The Construction of Man's Family Tree.' In the hall of the Royal Geographical Society, 9 p.m.

Sir Oliver Lodge, F.R.S., 'A Retrospect of Wireless Communication.' In the hall of the Royal College of Music, 9 p.m.

Sir J. Arthur Thomson, 'Biology in the Service of Man.' In the Great Hall of the University of London, 9 p.m.

Tuesday, September 29.

Sir James Jeans, F.R.S., 'Beyond the Milky Way.' In the Central Hall, Westminster, 9 p.m.

Dr. S. Kemp, F.R.S., 'Oceanography in the Antarctic.' In the hall of the Royal Geographical Society, 9 p.m.

Mr. H. E. Wimperis, C.B.E., 'High-speed Flying.' In the cinematograph theatre of the Imperial Institute, 9 p.m.

Bramwell Trust Lecture.—Sir Frederick Bramwell in 1903 made provision for the preparation of a lecture 'dealing with the whole question of the prime movers of 1931, and especially with the then relation between steam engines and internal combustion engines,' to be given at the Centenary Meeting. The Presidential Address to Section G (Engineering) given by Sir Alfred Ewing, K.C.B., F.R.S., in the hall of the Royal Geographical Society on Friday, September 25 at 5 p.m., was also the Bramwell lecture.

Public Lectures.—A public lecture was given by Mr. Angus Macrae on 'Guidance in the Choice of an Occupation,' at 3.30 p.m. on Monday,

September 28, in the London School of Economics, Houghton Street, Aldwych (by kind permission of the Director).

The following public lectures were given at certain Polytechnics during the period of the Meeting :—

Thursday, September 24.

Dr. L. C. Martin, 'Optics, the Servant of Science and Humanity.' At Northampton Polytechnic Institute, St. John Street, E.C. 1, 8 p.m.

Prof. I. M. Heilbron, F.R.S., 'The Fat-soluble Vitamins.' At the North-Western Polytechnic, Prince of Wales Road, Kentish Town, N.W. 5, 7.30 p.m.

Tuesday, September 29.

Prof. G. W. O. Howe, 'Michael Faraday, the Man and his Work.' At the Borough Polytechnic, Borough Road, S.E. 1, 7.30 p.m.

Dr. Alexander Wood, 'The Planning of Buildings for Good Acoustics.' At the Northern Polytechnic, Holloway, N. 7, 7.30 p.m.

Receptions.

Thursday, September 24.

A reception was given at the National Physical Laboratory, Teddington, in connection with the visit thereto on Thursday afternoon, September 24; see *Excursions and Visits*, below.

A reception was given at Bedford College for Women, Regent's Park, N.W. 1, by kind invitation of the Principal, on Thursday afternoon, September 24, from 4 to 6 p.m. Misses E. W. Gardner and G. Caton-Thompson arranged exhibits from their season's work at the Kharga Oasis.

A reception was given by the Royal Society to invited Delegates, in connection with the Faraday Centenary Celebration, on Thursday evening, September 24.

Friday, September 25.

A reception was given by H.M. Government in the Imperial Institute on Friday evening, September 25, beginning at 9 p.m.

A reception was given by the founder and director of the Wellcome Historical Medical Museum, on Friday evening, September 25, beginning at 8.30 p.m., in the Museum, 54 Wigmore Street, Cavendish Square, W. 1.

Saturday, September 26.

A visit to Hampton Court Palace took place on Saturday afternoon, September 26, by the kind invitation of Mrs. Antrobus (see page xxxiv).

Monday, September 28.

A reception was given by the Chancellor, the Court and the Senate of the University of London on Monday evening, September 28, in the University.

A reception was given at the Wellcome Historical Medical Museum, as above (Friday).

Tuesday, September 29.

A reception was given at the Forum Women's Club, 6 Grosvenor Place, Hyde Park Corner, on Tuesday afternoon, September 29.

Wednesday, September 30.

A reception was given by the Rt. Hon. the Lord Mayor and Corporation of the City of London, on Wednesday evening, September 30, at Guildhall.

A reception was held at the Science Museum, South Kensington, on Wednesday evening, September 30, by the kind permission of the Director, Sir Henry Lyons, F.R.S., who, with Lady Lyons, received Members on arrival. The President, General Smuts, took farewell of the Members present.

Exhibitions.

Museums and other institutions which were the objectives of Excursions and Visits are referred to under that general heading on later pages.

British Broadcasting Corporation: Educational Broadcasting.—A B.B.C. exhibition was arranged in the East Gallery of the University dealing principally with the educational side of broadcasting.

The main part of the hall contained a publications counter, where B.B.C. publications were on sale, special displays dealing with adult education, listening groups and school broadcasting.

To enable visitors to see the conditions under which broadcasting takes place, two models of different types of studios were on view, also a model of the London Control Room.

At the end of the hall there was a demonstration room, where demonstrations of wireless reception took place. Special demonstrations illustrating the contrast between satisfactory and unsatisfactory reception were given each day.

Chemistry Section (*B*).—Exhibits were arranged during the Meeting, adjacent to the Section Room, as indicated among the Sectional Transactions later in this volume.

Arrangements were made for the Department of Chemical Technology, Imperial College, Prince Consort Road, to be on view to Members from 2 to 5 p.m. daily, except Saturday and Sunday, during the Meeting, when demonstrations and exhibits relating to the following researches were given :—

(i) Gaseous Combustion and Explosions at High Pressures,
(ii) Catalytic Reactions at High Pressures,
(iii) The Photographic Analysis of Explosion Flames,
(iv) Gaseous Combustion in Electric Discharges,
(v) The Chemical Constitution of Coal,
(vi) Blast Furnace Reactions,
(vii) Chemical Engineering Problems.

Professors W. A. Bone, F.R.S., and G. J. Finch and the staff of the Department were in attendance to receive visitors and explain the exhibits.

Faraday Centenary Exhibition.—The private view for Members on Wednesday, September 23, has been referred to above (page xvi.).

Forestry, Department of (K*).—The British Wood-preserving Association arranged an exhibit of wood-preservation in connection with communications to the Department on Thursday, September 24. The exhibit remained open throughout the Meeting.

Geological Exhibits.—Special exhibits and demonstrations were arranged at (*a*) the Museum of Practical Geology and Survey Offices, 27 Jermyn Street, Piccadilly, where an extensive series of maps and specimens illustrating the Geology of London and the Home Counties were on view; and (*b*) the British Museum (Natural History), Cromwell Road, South Kensington. Here the following special exhibits were on view:—

1. A History of palæontology (with special Guide).
2. Palæontology and evolution.
3. Fossil reptiles.
4. Recent work on the anatomy of fossil Brachiopoda.
5. Trends in fossil corals.
6. Problematic fossils.
7. Palæontological methods in the workshop.

There was also an exhibition of maps and memoirs by the Geological Survey in the De la Beche Laboratory, Royal School of Mines.

Geophysical Instruments, &c.—An exhibit of geophysical instruments and survey methods was shown in the Science Museum, through the courtesy of Sir Henry Lyons, F.R.S., during the period of the Meeting, and in connection with the discussion on Geophysical Methods of Prospecting in Section A, Friday morning, September 25.

King's College of Household and Social Science, Campden Hill Road, W. 8.—The Biological, Chemical and Physiological Laboratories were open to inspection on Friday, September 25, Monday, September 28, and Tuesday, September 29, from 2.30 to 5 p.m.

Mathematical and Physical Sciences (*Section A*).—During the Meeting, Prof. Kerr Grant exhibited in the Physics Laboratory, adjacent to the Section room, a simple static voltmeter, a surface tension meter, a model of inertia coupled oscillators, and a contrivance for demonstrating the law of errors.

Mechanical Aids to Learning.—The second exhibition of Mechanical Aids to Learning was held from Tuesday, September 22, to Tuesday, September 29 inclusive, at South Kensington. Arrangements were made by the British Institute of Adult Education, through a joint organising committee on which the British Association, through its Education Section (L), and the Commission on Educational and Cultural Films, were represented. The exhibition comprised (*a*) exhibits of apparatus connected with broadcasting, television, the film, the gramophone, the epidiascope, and other similar inventions, with demonstrations of their working; (*b*) a series of lectures, discussions and demonstrations of the use of these types of apparatus under class-room and lecture-room conditions. The exhibition was housed in the Imperial Institute, the Institut Français (1-7 Cromwell Gardens), and the Lecture Theatre of the Science Museum. Members were admitted without charge.

Publishers' Association.—An exhibition of scientific books published by leading firms was arranged in the West Gallery of the University.

School Nature Study Union.—The President and Executive of the School Nature Study Union extended to Members of the Association an invitation to visit the Nature Study Exhibition held at the London Day Training College, Southampton Row, on Saturday, September 26, from 2.30 to 6 p.m.

Science Museum.—In addition to the daily public lectures given and tours conducted by the official lecturers, the officers of the Museum generously offered to show the collections in their charge to any Members who desired to make a closer study of any object or objects than is possible in a tour with a party. See, further, *Geophysical Instruments*, above.

Shipping, Engineering and Machinery Exhibition.—This exhibition, in Olympia, Kensington, was freely open to Members, and a special visit in connection with Section G (Engineering) was arranged.

Wellcome Historical Medical Museum, Wigmore Street, W. 1.—In addition to the receptions arranged for the evenings of Friday, September 25, and Monday, September 28, the following special exhibitions of archæological, ethnological, and folk-lore interest were held in the museum :—

1. Egypt Exploration Society. Jewellery and antiquities from the excavations at El Amarna and Armant.
2. English Folk-Dance Society. Historical folk costumes, &c.
3. Mr. Duggan-Cronin. Photographs of native life from South Africa.
4. Mr. H. W. Seton-Karr. Stone artefacts from Somaliland.
5. Dr. R. Broom, F.R.S. Collection of crania from South Africa.
6. Prof. L. Cipriani. Photographs and casts of African natives.
7. Miss Blackman. Tatu designs from modern Egypt.

St. Paul's Cathedral.

On Sunday, September 27, accommodation was reserved for Members at the service in St. Paul's Cathedral at 10.30 a.m. Preacher, the Right Rev. the Lord Bishop of Southwark.

Excursions and Visits.

A geological excursion to East Anglia took place before the Meeting, with the following time table :—

Wed., Sept. 16	Older Red Crag, London to Colchester by train. Motor coach to Walton, Beaumont and Little Oakley. To Ipswich in evening.
Thur., Sept. 17	Coralline Crag, Newer Red Crag, Chillesford Beds, &c. From Ipswich to Newbourn, Ramsholt, Sudbourne, Orford, by motor coach. Back to Ipswich.
Fri., Sept. 18	Chalk, Eocene and Glacial. From Ipswich to Bramford and Claydon, by motor coach. Drive to Norwich.

Sat., Sept. 19 — Norwich Crag, Older Glacial. From Norwich by motor coach via Thorpe to Lowestoft, Corton and Hemsby. Return via Sprowston to Norwich.

Sun., Sept. 20 — Chalk, Late Pliocene, Glacial. From Norwich by motor coach to Happisburgh and Trimmingham, &c. East end of Cromer section. Stay at Cromer.

Mon., Sept. 21 — Chalk, Late Pliocene, Glacial. Cromer to Weybourn and Hunstanton by motor coach.

Tues., Sept. 22 — Carstone, Red Chalk, Chalk, Glacial. Hunstanton. Return to London by train in evening.

Thursday, September 24.

X2 NATIONAL PHYSICAL LABORATORY, TEDDINGTON.
Visitors were given the opportunity of inspecting the laboratory and the gardens of Bushy House. Special demonstrations were arranged in connection with Section G (Engineering). Tea was provided by kind invitation of the Director.

(*Section C, Geology.*)

C1 CHARLTON AND DARTFORD HEATH.
C2 DENHAM AND HAREFIELD.

(*Section D, Zoology.*)

D1a UNIVERSITY COLLEGE, GOWER STREET (DEPTS. OF ZOOLOGY AND HUMAN ANATOMY).
Special demonstrations of zoological interest were given.

D1b LONDON SCHOOL OF HYGIENE AND TROPICAL MEDICINE, KEPPEL STREET, GOWER STREET.
A special demonstration of zoological interest was arranged, and tea was provided by the School.

(*Section E, Geography.*)

E1 IMPERIAL INSTITUTE.

(*Section H, Anthropology.*)

H1 LONDON MUSEUM.
Parties were conducted by members of the staff, and tea was provided by kind invitation of the Trustees.

(*Section I, Physiology.*)

I1 UNIVERSITY COLLEGE, GOWER STREET (PHYSIOLOGY DEPT.) AND SCHOOL OF HYGIENE AND TROPICAL MEDICINE.
Special demonstrations were arranged, and tea was provided by kind invitation of the Board of Management and the Council of the School of Hygiene and Tropical Medicine.

(*Section K, Botany.*)

K1 ROYAL BOTANIC GARDENS, KEW.
Parties were conducted round the Herbarium, Library and other places of interest by members of the staff.

(*Department K*, Forestry.*)

K*1 Forest Products Research Laboratory, Princes Risborough.
The visitors were shown round the Laboratory by guides, visiting the Sections of Wood Structure, Wood Chemistry, Timber Physics, Timber Mechanics, Seasoning, Wood Preservation, Mycology, Entomology, Utilization and Woodworking. Tea was provided at the Research Station by kind invitation of the Director.

(*Section L, Educational Science.*)

L7 Middlesex Hospital Medical School, Mortimer Street, W. 1.
The party were conducted over the teaching parts of the Hospital and were the guests of the School for lunch and tea.

L1 London Museum, Lancaster House, St. James, S.W. 1.

L2 Harrow School.
Tea was provided at the School by kind invitation of the Headmaster who also led the party.

L3 Morley College for Working Men and Women, 61 Westminster Bridge Road.

L4 Monotechnic, The School of Building. Ferndale Road, Brixton.

L5 Downham Central School, Goudhurst Road, Bromley.

L6 Open-air Nursery School, Old Church Road, Commercial Road, E.

Friday, September 25.

Morning.

(*Section, Educational Science.*)

L8 Furzedown Training College and Streatham County School.
Lunch was provided at the Furzedown Training College by kind invitation of the Principal.

Afternoon.

X3 Whipsnade Park (Zoological Society of London).

X4 Down House, the home of Darwin from 1842 to 1882, maintained by the Association as a national memorial. The Donor and Honorary Curator, Mr. G. Buckston Browne, F.R.C.S., F.S.A., kindly attended during the afternoon.

(*Section A, Mathematical and Physical Sciences.*)

A1 Royal Observatory, Greenwich.
Tea was provided by kind invitation of the Astronomer Royal, Sir F. W. Dyson, K.B.E., F.R.S.

(*Section C, Geology.*)

C3 Croham Hurst and Worms Heath.

C4 Hertford, Ware and Broxbourne.

(*Section D, Zoology.*)

D2 Royal College of Surgeons.

Parties were conducted round the Hunterian Museum by Sir Arthur Keith, F.R.S., and Mr. R. H. Burne, F.R.S. Tea was provided by kind invitation of the President and Council of the College.

(*Section E, Geography.*)

E2 Meteorological Office, South Kensington.

E3 Science Museum.

(*Section G, Engineering.*)

G4 Members connected with the Engineering Section were invited to visit the Shipping and Engineering Exhibition at Olympia during the afternoon. Tea was provided by kind invitation of the Exhibition authorities.

(*Section H, Anthropology.*)

H2 Miss Canziani's Studio and collection of folk-lore objects, mostly of Italian origin (Abruzzi) but with some Piedmontese and others from Savoy. Tea was provided by kind invitation of Miss Canziani.

H3 Messrs. Bryant and May's Fire-making Museum.

(*Section I, Physiology.*)

I2 Royal College of Physicians.

Tea was provided by kind invitation of the President and Council.

(*Section J, Psychology.*)

J1 University College, Gower Street (Psychology Dept.).

A demonstration was arranged, by kind permission of Prof. Spearman.

J2 King's College, Strand (Psychology Dept.).

A demonstration was arranged by kind permission of Dr. Aveling.

J3 Bedford College for Women (Psychology Dept.).

A demonstration was arranged by kind permission of Prof. Edgell.

(*Section K, Botany.*)

K2 Natural History Museum, to view a selection of the historical collections of specimens, manuscripts and drawings in the Department of Botany.

(*Department K*, Forestry.*)

K*2 Messrs. W. W. Howard Bros. & Co.'s Timber Wharves, Canning Town.

Tea was provided by kind invitation of Mr. Alexander L. Howard, J.P.

(*Section L, Educational Science.*)

L9 Brixton Day Continuation School.

Tea was provided by kind invitation of the School Staff.

L10 THE CHARTERHOUSE AND ST. JOHN'S GATE.
Tea was provided at St. John's Gate by kind invitation of the Order of St. John of Jerusalem.

L11 UNIVERSITY COLLEGE, GOWER STREET.
After inspection of the College premises, tea was provided by kind invitation of the Provost.

L12 STOWEY HOUSE OPEN-AIR DAY SCHOOL, CLAPHAM COMMON.

L13 MARVELS LANE L.C.C. SCHOOL, GROVE PARK, S.E.

L14 SCHOOL OF ENGINEERING AND NAVIGATION, HIGH STREET, POPLAR.
Tea was provided by kind invitation of the Principal.

L15 ST. PAUL'S GIRLS' SCHOOL, BROOK GREEN, HAMMERSMITH, W. 6.
Tea was provided by kind invitation of the High Mistress.

Saturday, September 26.

MORNING.

X1 YORK.
A party including the President and invited Officers and Members visited York, the birthplace of the Association in 1831, on Saturday and Sunday, September 26, 27. On Saturday afternoon General Smuts was admitted a freeman of the City of York, and opened the Hospitium in the Museum grounds. In the evening the Lord Mayor of York entertained the visiting Members and a distinguished local company to dinner. On Sunday morning the Lord Mayor and Corporation and the visitors attended service in the Minster, when the preacher was Canon Raven.

(*General and Section G, Engineering.*)

XG1 NEW FORD MOTOR WORKS, DAGENHAM, and LOWER REACHES OF THE THAMES.

(*Section B, Chemistry, and Section G, Engineering.*)

BG GAS LIGHT AND COKE COMPANY : WORKS AND LABORATORIES.
The party inspected the research laboratories and works of the Gas Light and Coke Company at King's Road, Fulham, and lunch was provided at the head offices of the Company by kind invitation of the Governor and Directors.

(*Section C, Geology.*)

C5 CHILTERN HILLS (WESTERN AREA).
Lunch was provided at Reading University by kind invitation.

C6 GUILDFORD DISTRICT.

(*Section E, Geography.*)

E4 ROMNEY MARSH.

(*Section H, Anthropology.*)

H4 COLCHESTER.
By kind permission of the Royal Archæological Institute, members connected with Section H (Anthropology) were allowed to take part, upon the same footing as the Institute's own members, in the Institute's excursion to Colchester.

(*Section K, Botany.*)

K3 Rothamsted Experimental Station, Harpenden.
Tea was provided by kind invitation of the Director and the Lawes Agricultural Trust Committee.

(*Department K*, Forestry.*)

K*3 Bedgebury.
This excursion was to a tract of land under the control of H.M. Forestry Commission, and the party studied the problems latent in afforestation work, and was conducted round the arboretum.

(*Section L, Educational Science.*)

L41 Heritage Craft Schools, Hospitals and Homes for Cripples, Chailey, and the Seaside Branch at Tidemills, Bishopstone, near Newhaven.
Tea was provided by the kind invitation of Dr. and Mrs. C. W. Kimmins.

L16 Christ's Hospital, West Horsham.
Lunch and tea were provided by kind invitation of the Headmaster and Governors.

L17 Pitman's College and The Intensive Business Course (Marlborough Gate).

L18 Royal Naval College and Royal Observatory, Greenwich.
Lunch was provided at the College by kind invitation of the President.

(*Section M, Agriculture.*)

M1 Rothamsted Experimental Station, Harpenden.
Tea was provided by kind invitation of the Director and the Lawes Agricultural Trust Committee.

(*Section M, Agriculture, sub-section of Horticulture.*)

M*1 East Malling Research Station.
Tea was provided by kind invitation of the Director and Mrs. T. G. Hatton.

Afternoon.

X5 Hampton Court Palace (by the kind invitation of Mrs. Antrobus).
Opportunity was given to inspect the Palace and the flower gardens, and tea was provided. Some of the ladies resident in the Palace kindly assisted to receive the party.

(*Section D, Zoology.*)

D3 Zoological Society's Gardens, Regent's Park.
Parties were formed to visit the Reptile House, Aquarium, Sanatorium and Laboratories. Col. A. E. Hamerton and Dr. S. Zuckerman gave demonstrations during the afternoon. Tea was provided at 4.30 p.m. in the new Restaurant by kind invitation of the President and Council of the Zoological Society.

(*Section F, Economics.*)

F1 Welwyn Garden City.
This excursion was arranged in relation to Prof. Unwin's paper on 'The Town-planning of Greater London.' The party visited the

St. Pancras House Improvement Society's Slum Clearance Scheme, Ossulston Street Tenement Dwellings (L.C.C.), Hampstead Garden City and Watling Cottage Estate (L.C.C.) on the outward journey and Becontree Housing Estate (L.C.C.) on the return.

(*Section H, Anthropology.*)

H5 VERULAMIUM (ST. ALBANS).
The party was conducted over the recent extensive excavations of prehistoric and Roman sites.

(*Section J, Psychology.*)

J4 BETHLEM ROYAL HOSPITAL.
By kind permission of the Governors and the Physician-Superintendent, the party were able to study the work of the hospital in its various branches.

(*Section L, Educational Science.*)

L19 SCHOOL OF ARCHITECTURE, BEDFORD SQUARE.
An exhibition of students' work was arranged at the School, and tea was provided by kind invitation of the Council of the Architectural Association.

L20 PUBLIC RECORD OFFICE, CHANCERY LANE.

L21 THE SCHOOL OF PHARMACY, BLOOMSBURY SQUARE.
Tea was provided at the School by kind invitation of the Pharmaceutical Society.

L22 WESTMINSTER SCHOOL AND WESTMINSTER ABBEY.
Tea was provided at the School by kind invitation of the Headmaster.

L23 LONDON COUNTY COUNCIL: COUNTY HALL and EDUCATION LIBRARY.
Tea was provided at the County Hall by kind invitation of the Education Officer, Mr. G. M. Gater, C.M.G., D.S.O.

L24 SHIRLEY RESIDENTIAL SCHOOL, CROYDON.

Sunday, September 27.

MORNING.

(*Section C, Geology.*)

C7 CHILTERN HILLS, EASTERN AREA.

C8 GODSTONE, TONBRIDGE and REDHILL.

(*Section D, Zoology.*)

D4 TRING MUSEUM (by kind permission of Lord Rothschild) and WHIPSNADE PARK.

(*Section K, Botany.*)

K4 TRING AND NEIGHBOURHOOD.

(*Department K*, Forestry.*)

K*4 RENDLESHAM.
This is one of the Forestry Commissioners' areas. An opportunity was given to see the work in all its phases, including nurseries where the trees are grown for planting out, plantations of different species, but principally Scots and Corsican pines, in different stages of growth.

(*Section M, Agriculture.*)

M2 IMPERIAL CHEMICAL INDUSTRIES' RESEARCH STATION, JEALOTT'S HILL.
The party was conducted over the Station, which has been set up by the Agricultural Research and Advisory Department of the Imperial Chemical Industries, Ltd. Lt.-Col. W. R. Peel, D.S.O., gave an account of the work of the Economics section of the Agricultural Research Department on 'Small Holdings.' Lunch and tea were provided by kind invitation of the Director.

X6 LONDON AIR PARK, FELTHAM.
Headquarters of the Hanworth Club, one of the largest light aeroplane clubs in the country. During the course of the afternoon there were demonstrations of motorless flying and power flying, and facilities for the inspection of the hangars and workshops were provided.

(*Section E, Geography.*)

E5 THE RIVER THAMES. From Chelsea Pier to Greenwich and back to Westminster Pier.

(*Section H, Anthropology.*)

H6 CRANMORE MUSEUM, CHISLEHURST.
Mr. H. Beasley conducted the party over his ethnographical collection, and entertained them to tea. The museum was founded for the specialised study of the material culture of the races of the Pacific, with particular reference to Maori culture. Additional series have been formed along the same lines, dealing with the art of Benin, West Africa, and of the Eskimo and peoples of the North-West coast of America.

Monday, September 28.

MORNING.

(*Section L, Educational Science.*)

L25 NO. 2 SCHOOL OF TECHNICAL TRAINING (APPRENTICES), ROYAL AIR FORCE, HALTON, BUCKS.
Lunch and tea were provided at the school by kind invitation of the Air Ministry.

AFTERNOON.

(*Conference of Delegates of Corresponding Societies.*)

Z7 DOWN HOUSE.

(*Section A, Mathematical and Physical Sciences.*)

A2 GENERAL ELECTRIC CO.'S LABORATORY, WEMBLEY.
Facilities were afforded for inspection of the many branches of the Company's work in electrical research, visits being paid to the Research Laboratories and the Osram G.E.C. glass and lamp works. Tea was provided by kind invitation of the Company.

A2 KEW OBSERVATORY, METEOROLOGICAL OFFICE.
Tea was provided by kind invitation of the Air Ministry.

(*Section C, Geology.*)

C9 Claygate and Oxshott.

C10 Dorking.

(*Section D, Zoology.*)

D5 Farnham Royal Laboratory and Entomological Field Station, Slough.

About an hour was spent in inspecting the Farnham Royal Laboratory (by kind permission of the Director of the Imperial Bureau of Entomology). The party then proceeded to the Field Station of the Entomological Department of the Imperial College of Science and Technology. The laboratory and grounds were open for inspection, by kind permission of Prof. J. W. Munro.

(*Section F, Economics, and Section G, Engineering.*)

F G Works of the London General Omnibus Co., Ltd., and the London Underground Group of Companies.

Tea was provided at Chiswick by kind invitation of the Companies.

(*Section G, Engineering.*)

G2 London Electric Railways, Tube Extension Works, Southgate.

Tea was provided by kind invitation of the London Electric Railways.

(*Section H, Anthropology.*)

H7 British Museum.

Officers connected with the departments of Egyptian and Assyrian Antiquities, British and Mediæval Antiquities, and Ceramics and Ethnography conducted parties through their departments.

(*Section I, Physiology.*)

I3 Royal College of Surgeons.

Parties were conducted round the Hunterian Museum. Tea was provided by kind invitation of the President and Council of the College.

(*Section J, Psychology.*)

J5 National Institute of Industrial Psychology.

Films were exhibited illustrative of the Institute's factory and vocational guidance work, together with graphs and diagrams of results, and pieces of research work. Tests and other methods of vocational guidance and selection were demonstrated. Tea was provided by kind invitation of the Principal.

(*Section K, Botany, and Department K*, Forestry.*)

K5 Royal Horticultural Society's Gardens, Wisley.

Tea was provided by kind invitation of the Royal Horticultural Society.

(*Section L, Educational Science.*)

L26 Acre Lane Residential School for Mentally Defectives, Brixton.

L27 Borough Polytechnic, Southwark.

Tea was provided by kind invitation of the Principal.

L28 School Clinic, Highgate New Town.

L29 James Allen's Girls' School and Botany Gardens, Dulwich.
Tea was provided by kind invitation of the Head Mistress.

L30 Royal Holloway College, Englefield Green.
Tea was provided by kind invitation of the Governors and Principal.

L31 Chelsea College of Physical Education, Manresa Road, S.W. 3.
An open afternoon was arranged to enable visitors to see demonstrations of the three-years' course.

L32 Violet Melchett Infant Welfare Centre; and Chelsea Physic Garden.

L33 Special Curriculum School, Chaucer Street, Borough.

(*Section M, Agriculture.*)

M3 National Institute for Research in Dairying, Shinfield.

(*Section M, Agriculture, sub-section of Horticulture.*)

M*2 Messrs. Lowe & Shawyer's Nurseries, Uxbridge.
Tea was provided by kind invitation of Mr. George Shawyer.

Tuesday, September 29.

X7b Antarctic Research Ship Discovery II, St. Katherine's Dock, Tower Bridge.

X8b Air Port of London, Croydon.

X9 Port of London Authority, Docks.

(*Section A, Mathematical and Physical Sciences.*)

A4 London Docks and a Modern Ocean Liner.
The party inspected the Tobacco Warehouse and the Cold Store at the Victoria and Albert Docks (by kind permission of the Port of London Authority) and then proceeded to the M.V. *Highland Princess*, where tea was provided by the kind invitation of the Nelson Line, and opportunity was given for looking over the ship and machinery (both main and refrigeratory).

A5 Radio Research Station, Slough.
Tea was provided at the Admiralty Compass Observatory, which was open for inspection.

(*Section C, Geology.*)

C11 Swanscombe.

C12 Dunton Green and Sevenoaks.

(*Section D, Zoology.*)

D6 Zoological Departments open for Inspection.
By kind permission of the Professors and College Councils, the Departments of Zoology in the following Colleges and Institutes were open for inspection by members of the Association:—
King's College, Strand.
Imperial College of Science and Technology.
Bedford College for Women.
Birkbeck College.

King's College of Household and Social Science.
Lister Institute of Preventive Medicine.

(*Section E, Geography.*)

E6 Circuit of the City of London.

(*Sections E, Geography ; F, Economics ; G, Engineering.*)

EFG Port of London Authority, Docks.
A special launch and tea were provided by kind invitation of the Chairman and Council of the Authority.

(*Section G, Engineering.*)

G3 British Broadcasting Corporation, London Regional Station. Brookman's Park.
Tea was provided by kind invitation of the Corporation.

(*Section H, Anthropology.*)

H8 Horniman Museum, Forest Hill.
Tea was provided by kind invitation of Mr. Emslie J. Horniman.

(*Section J, Psychology.*)

J6 London Child Guidance Clinic.
J7 Child Guidance Clinic, Spitalfields.

(*Section K, Botany.*)

K6 Down House.
K7b John Innes Horticultural Institution, Merton Park.
Tea was provided by kind invitation of the Council of the Institution.

(*Section L, Educational Science.*)

L34 National Training School of Cookery and Domestic Science.
Tea was provided at the School by kind invitation of the Principal.
L35 School of Arts and Crafts, and London Day Training College.
L36 Institute of Historical Research.
L37 Royal Academy of Music.
L38 Bec Secondary School, Tooting.
Tea was provided by kind invitation of the Headmaster and staff.
L39 City of London College.

Wednesday, September 30.

Afternoon.
X7 'Discovery II,' St. Katherine's Dock, Tower Bridge.
X8 Air Port of London, Croydon .

(*Section K, Botany.*)

K7 John Innes Horticultural Institution, Merton Park.

Down House.—The President, General Smuts, and the Hon. Curator Mr. G. Buckston Browne, received a party of guests.

REPORT OF THE COUNCIL, 1930-31.

Obituary.

I.—The Council has had to deplore the loss by death of the following office-bearers and supporters :—

Dr. H. M. Ami.	Mr. M. A. Giblett.
Dr. T. Ashby.	Prof. W. D. Halliburton.
Dr. T. V. Barker.	Mr. C. T. Heycock.
Prof. A. Barr.	Dr. A. Holt.
Mr. H. O. Beckit.	Sir Charles Lucas.
Sir Hugh Bell.	Prof. W. C. McIntosh (a life member since 1867).
Dr. H. Borns.	Mr. H. W. Monckton.
Dr. Florence Buchanan.	Prof. C. E. Moss.
Prof. H. Wildon Carr.	Sir Francis Grant Ogilvie.
Mr. T. F. Chipp.	Hon. Sir C. Parsons (ex-president and benefactor).
Prof. H. B. Dixon.	Sir Harry Reichel.
Prof. W. E. Dixon.	Sir Richard C. Temple, Bart.
Dr. A. R. Dwerryhouse.	
Dr. J. W. Evans.	

The Association was represented by the President, Prof. F. O. Bower, at the memorial service in Westminster Abbey for the Hon. Sir Charles Parsons.

Representation.

II.—Representatives of the Association have been appointed as follows :—

Royal Geographical Society, Centenary .	The Secretary.
National Committee on Geography . .	Prof. H. J. Fleure.
National Committee on Geodesy and Geophysics	Dr. F. J. W. Whipple.
Deputation to H.M. First Commissioner of Works on preservation of the Roman Wall	Prof. J. L. Myres.
Deputation on withdrawal of grants from public funds in Australia to the Anthropological Dept., University of Sydney. .	Dr. A. C. Haddon.
Congress of Universities of the Empire (London Meeting) . . .	Prof. P. G. H. Boswell.
Ditto (Edinburgh Meeting) . .	Prof. Sir Edward Schafer.
Ditto, Reception Committee . .	The Secretary.
British National Committee of Folk Art .	Prof. J. L. Myres.
Association Française pour l'Avancement des Sciences (Nancy)	Mr. T. Sheppard.
Joint Committee for Anthropological Research and Teaching (Royal Anthropological Institute)	Dr. A. C. Haddon.
British Commonwealth Education Conference	Dr. C. W. Kimmins.
Royal Dublin Society, Bicentenary . .	The President.
Sir Charles Parsons Memorial Committee (Royal Society)	Prof. J. L. Myres and Sir Henry Fowler.
Faraday Centenary Celebrations . .	Sir Charles Sherrington.

Resolutions from Bristol Meeting.

III.—Resolutions referred by the General Committee at the Bristol Meeting to the Council for consideration, and, if desirable, for action, were dealt with as follows :—

(*a*) On the recommendation of Section A (Mathematical and Physical Sciences) it was resolved that the mathematical tables prepared as stated

in the report of the Mathematical Tables Committee (Bristol Meeting) be printed at a cost not exceeding £200.

(*b*) A resolution from Section A (Mathematical and Physical Sciences) expressing condolence on the death of Prof. H. H. Turner, and the hope that his death would cause no discontinuity in the astronomical and seismological work carried on at the University Observatory, was adopted by the Council and forwarded to the Board of Visitors of the observatory.

(*c*) A resolution from Section H (Anthropology) supporting the establishment of a National Open-air Folk Museum in London, and a resolution from Section K (Botany) urging the importance of continuing provision for botanical research on part of the grounds of the Royal Botanic Gardens, Regent's Park, which were proposed as the site for such a museum, were considered together by a committee of Council. The Council received and adopted a report from this committee to the effect that such a museum was desirable, that it would best fulfil its objects if established in or close to London, and that the site in Regent's Park should be considered, provided that this could be done without interfering with the ground used for the scientific work of botanical departments of the University of London.

(*d*) A resolution from Section H (Anthropology) on the desirability of anthropological training for officials charged with native administration in Australia, and of preventing the extinction of the aborigines, was adopted and communicated to the Prime Minister and the Secretary of State for the Dominions.

(*e*) A resolution from Section H (Anthropology) dealing with the activity of unauthorised persons on archæological sites in South Africa and the Rhodesias, was adopted and communicated to the Secretaries of State for the Dominions and for the Colonies.

(*f*) A resolution from the Conference of Delegates of Corresponding Societies urging the establishment of nature reserves in connection with national parks in Great Britain was adopted and forwarded to the National Park Committee.

Centenary Meeting.

IV.—In connection with preparations for the Centenary Meeting, the Council have received unstinted help from all the authorities which they have had occasion to approach. These will more appropriately receive the thanks of the Association at the Meeting; but the Council wishes now to record their gratitude to the Lord Mayor, Sir W. Phené Neale, for permitting the London Committee to meet in Guildhall on April 21, and himself taking the chair.

Presidency.

V.—Sir Alfred Ewing, K.C.B., F.R.S., has been unanimously nominated as President of the Association for the year 1932 (York Meeting).

VI.—The Council recommend the following change in Statute VI, 1, for adoption by the General Committee subject to the approval of H.M. Privy Council.

The present Statute provides that:—

The President shall assume office on the first day of the Annual Meeting,

when he shall deliver a Presidential Address. He shall resign office at the next Annual Meeting, when he inducts his successor into the Chair.

The amendment recommended provides that :—

The President shall assume office on the first day of January next following the Annual Meeting at which he is appointed. He shall deliver a Presidential Address at the Annual Meeting during his year of office, and shall vacate his office on the thirty-first day of December next following that meeting.

The Council has carefully considered this important amendment, which was proposed by one of the ex-presidents of the Association and approved by a large majority of the remainder. It is recommended on these grounds :—

(1) That the President would be responsible administratively for the major part of the preparation of arrangements for the Annual Meeting over which he is elected to preside, and his influence could be more directly brought to bear upon them.

(2) In particular, he would take the chair at the joint meeting of Organising Sectional Committees in the January preceding the Annual Meeting, which has now become a regular and principal part of the mechanism of preparing the programme.

(3) As a point of minor but still recognisable importance, he would arrive at the place of the Annual Meeting as President, not as President-elect, and possible confusion in the local public mind would be avoided.

(4) After the Annual Meeting he would still be in office to preside over those meetings of the Council at which matters arising out of the Annual Meeting are principally dealt with.

The objection that the ceremony of installing the new President at the Inaugural Meeting would be lost, may be discounted, as it would still be possible for, *e.g.*, the immediate ex-president to introduce the President to the first general meeting over which he would preside, but the existing Statute VI, 1, has proved to be unworkable on occasions, sometimes because of the unavoidable absence of the outgoing President.

Down House.

VII.—The following report for the year 1930–31 has been received from the Down House Committee :—

The number of visitors to Down House during the year ending June 6, 1931, has been 5210, compared with 11,000 in the previous year. As this was the first, a diminution was to be foreseen, especially as in the earlier period a number of societies sent parties. The public interest appears to those in residence to be as great as could be expected, having regard to the geographical position of the house.

Thanks have been tendered on behalf of the Committee for a number of gifts or loans, of which three call for special notice. The magnificent gift of shrubs and herbaceous plants from the Director of Kew Gardens, which in a measure repeats history, since the garden in Darwin's time was certainly indebted to Hooker, will go far, in due season, to rehabilitate the grounds. The decision of Prof. A. C. Seward, F.R.S., to deposit the major part of the Darwin Library at Down House on loan (the library having been bequeathed by Sir Francis Darwin 'to the professor of botany in the University of Cambridge for the time being') has already been gratefully acknowledged by the Council. The important series of letters from Darwin to Fritz Müller in Brazil, which, as the Council are aware, was acquired by Prof. H. Fairfield Osborn from Müller's family for presentation to the Association, has now been received, together with a series of photostat facsimiles, contained in portfolios specially made and inscribed.

The contents of the memorial rooms have been valued and insured against loss by fire.

A catalogue of the exhibits has been prepared and printed for sale to visitors.

The hon. curator, Mr. Buckston Browne, has arranged that the rooms shall be open to the public throughout the year, except on Christmas Day and Good Friday, between 10 and 6 o'clock from April to September, and between 11 and 4 o'clock from October to March.

The rating of the property is still in dispute, the rating authority holding that unless 'complete severance' is effected between the public and residential portions of the house, the whole must be rateable. Further legal opinion is being obtained; but it is pertinent to observe that none of the Committee's advisers has as yet adduced an approximately parallel case which would carry conviction to both parties.

The income from dividends upon the endowment fund, rents, sales and donations during the calendar year 1930 amounted to £1,150, and the expenditure on running costs during the same year was £1,210. 'Capital' expenditure by the Association since the acquisition of the property amounts to £2,500, with certain further commitments in view and exclusive of the second mortgage of £700 granted to the outgoing tenant. The estimates given in last year's report of the Committee were therefore not far from the results of a complete year's experience; but the Committee's hope then expressed that additional financial support would be forthcoming in connection with the Centenary Fund has not yet been realised.

Finally, the Committee would wish to share the gratification which the Council has already expressed for Mr. Buckston Browne's munificent gift to the Royal College of Surgeons to enable the establishment of a research farm on land adjoining the Down House property.

With reference to the above report, the Council have deposited the originals of the Müller letters in the British Museum on permanent loan.

Proposed Visits to Chicago and New Zealand.

VIII. *Chicago.*—The Council have received from Dr. Henry Crew, on behalf of the Administration of the Chicago World's Fair, 1933, in co-operation with the American Association for the Advancement of Science, a generous invitation to the Association to form a party of some twenty leading British representatives of science to visit Chicago during the period of the fair, and to take part in scientific proceedings. A subsidy is offered in connection with ocean travel. The Council understand that a formal invitation will be presented to the General Committee, and they recommend that it be accepted. The principle of organising such a party additionally to the ordinary annual meeting of the Association would be new, but there is no statutory bar to it, and the Council consider that it would be a proper and desirable extension of the Association's activities.

IX. *New Zealand.*—H.E. the Governor-General of New Zealand, Lord Bledisloe, indicated that he hoped for the visit of a representative party from the Association during his period of office (which terminates in 1934), and that such a party would be assured of a warm welcome. The Council appointed a committee to consider the proposal, and in its recommendation authorised Sir Thomas Holland and the General Officers to enter into communication upon the proposal with the Governor-General, the Prime Minister, and the High Commissioner. From the last of these it was understood that, in view of the present financial situation in New Zealand, no good purpose would be served by pursuing the proposal further at present.

Finance.

X. *General Treasurer's Account.*—The Council has received reports from the General Treasurer throughout the year. His accounts have been audited and are presented to the General Committee. His prefatory statement deals with the position of the Centenary Fund. Every contribution and promise have been acknowledged on the Council's behalf, and a list will be issued at the Centenary Meeting.

Under the will of the late Sir Charles Parsons, already, as is well known, the Association's generous benefactor, the Association is a beneficiary in the sum of £2,000.

General Officers, Council, and Committees.

XI.—*The General Officers* have been nominated by the Council as follows:—

General Treasurer, Sir Josiah Stamp.

General Secretaries, Prof. J. L. Myres, Prof. F. J. M. Stratton, and Prof. P. G. H. Boswell.

Prof. Stratton offered his resignation to the Council, since considerations of health have prevented him from taking full part in the preparations for the Centenary Meeting. He had also made it clear, when first appointed, that he would be absent from the meeting in 1932. The Council did not accept Prof. Stratton's resignation, but appointed Prof. Boswell as a third (acting) general secretary, and now make the nominations stated above.

XII. *Council.*—The retiring Ordinary Members of the Council are Dr. F. C. Shrubsall, Prof. A. L. Bowley, Prof. C. Lovatt Evans, and Mr. F. C. Bartlett, together with Dr. H. Clay, who resigned owing to inability to attend.

The Council nominates the following new members: Dr. J. Drever, Prof. T. Gregory, Prof. E. B. Poulton; leaving two vacancies to be filled by the General Committee without nomination by the Council.

The full list of nominations of Ordinary Members is as follows:—

Dr. F. A. Bather.
Dr. J. Drever.
Sir Henry Fowler.
Prof. W. T. Gordon.
Sir Richard Gregory.
Prof. T. Gregory.
Prof. Dame Helen Gwynne-Vaughan.
Dr. A. C. Haddon.
Sir Daniel Hall.
Sir James Henderson.
Mr. A. R. Hinks.
Dr. C. W. Kimmins.
Col. Sir H. G. Lyons.
C. G. T. Morison.
Sir P. Chalmers Mitchell.
Prof. E. B. Poulton.
Prof. A. O. Rankine.
Dr. C. Tate Regan.
Prof. A. C. Seward.
Dr. N. V. Sidgwick.
Dr. G. C. Simpson.
Prof. J. F. Thorpe.
Mr. H. T. Tizard.

XIII. *General Committee.*—The following have been admitted as members of the General Committee: Mr. N. K. Adam, Mr. J. H. Awbery, Dr. W. N. Bond, Dr. E. M. Crowther, Dr. G. A. Dunlop, Dr. H. G. Fourcade, Mr. A. Horne, Mr. Alexander Howard.

XIV. *Corresponding Societies Committee.*—The Corresponding Societies Committee has been nominated as follows: The President of the Association (*Chairman ex-officio*), Mr. T. Sheppard (*Vice-Chairman*), Dr. C. Tierney (*Secretary*), The General Treasurer, the General Secretaries, Mr. C. O. Bartrum, Dr. F. A. Bather, Sir Richard Gregory, Mr. J. V. Pearman, Sir David Prain, Sir John Russell, Prof. W. M. Tattersall.

GENERAL TREASURER'S ACCOUNT

1930-31.

In the balance sheet there appears the sum received on account of donations to the Centenary Fund down to June 30, 1931. I have conveyed the thanks of the Council to all the subscribers, who at that date numbered over 500. Certain additional donations have been promised. But in view of the general financial situation and the experience gained from other public appeals at the present time, it was clearly inopportune to press the appeal for the Centenary Fund as strongly as it might have been pressed in favourable circumstances, and for the present it would seem that the Association can aim at little more than covering its commitments in respect of the Centenary Meeting itself. Even this object has not yet been achieved, for the statement in the balance sheet needs amplification. It shows only one-half of the grants for Imperial delegates (which will total £3,000) as having been paid, and only a small sum (£141 7s.) on account of deposit for the hire of the Central Hall, Westminster, and additional clerical assistance, which had been paid down to June 30. The income and expenditure account shows increases both in receipts from membership subscriptions, and in expenditure upon stationery, postages, and general expenses, which are directly attributable to the forthcoming meeting; but the bulk of the expenditure in connection with that meeting will fall within the ensuing financial year. It may, indeed, be hoped that next year's accounts will again show a substantial increase in membership receipts, for the same reason; but the expenses necessarily to be incurred must also show an advance which will justify a further appeal for an endowment fund as soon as circumstances become more favourable. The activities and liabilities of the Association have increased, and further endowment will be essential to consolidate the position it has attained at the close of its first century.

The direct comparison between the expenditure for printing during the past year (£1,638) and the preceding year (£1,060) is misleading, because some of the payments on this account which normally would have fallen within the year 1929–30 had been made in the year before, when the expenditure on printing was shown as £2,882. The figure for the present year, therefore, should be compared with the average for the two preceding years, £1,971, from which it will be realised that actually some saving has been effected.

J. C. Stamp,
General Treasurer.

Balance Sheet,

Corresponding Figures June 30, 1930. £ s. d.	LIABILITIES.	£ s. d.	£ s. d.
	To *General Fund*—		
	As at July 1, 1930		
10,942 19 1	As per contra		10,942 19 1
	(Subject to depreciation in value of Investments)		
	,, *Caird Fund*—		
	As at July 1, 1930		
9,582 16 3	As per contra		9,582 16 3
	(Subject to depreciation in value of Investments)		
	,, *Caird Fund Revenue Account*		
	Balance at July 1, 1930	364 12 6	
	Add Excess of Income over Expenditure		
364 12 6	for the Year as per contra . .	34 8 7	399 1 1
	,, *Sir F. Bramwell's Gift*—		
	For enquiry into Prime Movers, 1931—£50 Consols now accumulated to £165 12*s.* 10*d.*		
80 4 2	As per contra		84 4 7
	,, *Sir Charles Parsons' Gift*—		
10,000 0 0	As per contra		10,000 0 0
	,, *Sir Alfred Yarrow's Gift*—		
	As per last Account	9,055 0 0	
	Less Transferred to Income and Expenditure Account under terms of the Gift .	348 0 0	
9,055 0 0	As per contra		8,707 0 0
	,, *Life Compositions*—		
	As per last Account	1,727 2 2	
	Add received during year	240 0 0	
		1,967 2 2	
	Less Transferred to Income and Expenditure Account	15 0 0	
1,727 2 2	As per contra		1,952 2 2
	,, *Toronto University Presentation Fund*—		
	As per last Account	182 18 10	
	Add Dividends	8 15 0	
		191 13 10	
	Less Awards given	8 15 0	
182 18 10	As per contra		182 18 10
	,, *Lt.-Col. A. J. C. Cunningham's Bequest*—		
	For the preparation of New Tables in the Theory of Numbers.		
	As per last Account	2,943 14 9	
	Add—		
	Dividends . . . 110 11 2		
	Income Tax Recoverable . 16 6 0		
	Profit on Sale of Consols . 7 4 10		
		134 2 0	
		3,077 16 9	
	Less Grant made	173 2 0	
2,943 14 9	As per contra		2,904 14 9
	,, *South African Association Medal Fund*—		
200 0 0	As per last Account	200 0 0	
	Less Paid to South African Association .	200 0 0	— — —
45,079 7 9	Carried forward .		£44,755 16 9

June 30, 1931.

Corresponding Figures June 30, 1930.	ASSETS.	£	s.	d.	£	s.	d.
	By *General Fund*—						
	£4,651 10*s.* 5*d.* Consolidated 2½ per cent. Stock at cost	3,942	3	3			
	£3,600 India 3 per cent. Stock at cost	3,522	2	6			
	£879 14*s.* 9*d.* Great Indian Peninsula Railway "B" Annuity at cost	827	15	0			
	£52 12*s.* 7*d.* War Stock (Post Office Issue) at cost	54	5	2			
	£834 16*s.* 6*d.* 4½ per cent. Conversion Stock at cost	835	12	4			
	£1,400 War Stock 5 per cent. 1929/47 at cost	1,393	16	11			
	£94 7*s.* 0*d.* 4½ per cent. Conversion Stock 1940/44 at cost	62	15	0			
	£326 9*s.* 10*d.* 3½ per cent. Ditto at cost	250	0	0			
	Cash at Bank	54	8	11			
10,942 19 1					10,942	19	1
	(*£8,069 18s. 1d.* Value of Stocks at date, £8,274 18*s.* 10*d.*)						
	,, *Caird Fund*—						
	£2,627 0*s.* 10*d.* India 3½ per cent. Stock at cost	2,400	13	3			
	£2,100 London, Midland & Scottish Railway Consolidated 4 per cent. Preference Stock at cost	2,190	4	3			
	£2,500 Canada 3½ per cent. Registered Stock 1930/50 at cost	2,397	1	6			
	£2,000 Southern Railway Consolidated 5 per cent. Preference Stock at cost	2,594	17	3			
9,582 16 3					9.582	16	3
	(*£6,872 9s. 11d.* Value at date, £6,404 11*s.* 10*d.*)						
	,, *Caird Fund Revenue Account*—						
364 12 6	Cash at Bank				399	1	1
	,, *Sir F. Bramwell's Gift*—						
	£158 13*s.* 3*d.* Self Accumulating Consolidated Stock as per last Balance Sheet	80	4	2			
	6 19 7 *Add* Accumulations to June 30, 1931	4	0	5			
80 4 2	£165 12 10 (Value at date, £99 7*s.* 8*d.*)				84	4	7
	,, *Sir Charles Parsons' Gift*—						
10,000 0 0	£10,300 4½ per cent. Conversion Stock at cost (*£10,171 5s. 0d.* Value at date, £10,609)				10,000	0	0
	,, *Sir Alfred Yarrow's Gift*—						
	£9,055 5 per cent. War Loan as per last Account	9,055	0	0			
	Less Sale of £348 Stock under terms of the Gift	348	0	0			
9,055 0 0	(Value at date, £8,968 4*s.* 2*d.*)				8,707	0	0
	,, *Life Compositions*—						
	£2,949 12*s.* 4*d.* Local Loans at cost (Value at date, £2,064 14*s.* 8*d.*)	1,923	12	2			
	Cash at Bank	28	10	0			
1,727 2 2					1,952	2	2
	,, *Toronto University Presentation Fund*—						
	£175 5 per cent. War Stock at cost (*£180 13s. 9d.* Value at date, £180 5*s.* 0*d.*)	178	11	4			
	Cash at Bank	4	7	6			
182 18 10					182	18	10
	,, *Lt.-Col. A. J. C. Cunningham's Bequest*—						
	£1,187 6*s.* 10*d.* 2½ per cent. Consolidated Stock	653	0	9			
	£300 Port of London 3½ per cent. Stock 1949/99	216	0	0			
	£100 Commonwealth of Australia 4¾ per cent. Stock	93	0	0			
	£100 New Zealand 5 per cent. Stock	103	0	0			
	£800 India 6 per cent. Stock at cost	801	12	0			
	£1,274 4*s.* 10*d.* Local Loans 3 per cent. Stock at cost	836	6	5			
	Cash at Bank	201	15	7			
					2,904	14	9
2,943 14 9	(Value of Stocks at date, £2,816 17*s.* 6*d.*)						
	,, *South African Association Medal Fund*—						
200 0 0	Cash at Bank				—	—	—
45,079 7 9	Carried forward				£44,755	16	9

Balance Sheet,

Corresponding Figures June 30, 1930. £ s. d.	LIABILITIES—*continued.*		£ s. d.	£ s. d.	£ s. d.
45,079 7 9	**Brought forward**				44,755 16 9
	To *Down House Endowment Fund*—				
20,000 0 0	As per contra				20,000 0 0
	,, REVENUE ACCOUNT—				
	Sundry Creditors			265 15 11	
	Do. Do. (Down House)			14 4 10	
				280 0 9	
	,, *Income and Expenditure Account*—		£ s. d.		
	Balance at July 1, 1930		8,755 16 4		
	Add Excess of Income over Expenditure for the year.		531 1 6	9,286 17 10	
9,231 8 4				9,566 18 7	
	,, *Centenary Meeting*—				
	Sundry Donations		£2,208 5 6		
	Less Hire of Hall, Salaries, &c.	141 7 0			
	Grants for Imperial Delegates	1,500 0 0	1,641 7 0		
				566 18 6	10,133 17 1
4,310 16 1					£74,889 13 10

I have examined the foregoing Account with the Books and Vouchers and certify the sam
and the Investments and have inspected the Deeds of Down House and the Mortgage o
Approved.

ARTHUR L. BOWLEY
A. W. KIRKALDY } *Auditors.*

July 30, 1931.

June 30, 1931—*continued.*

Corresponding Figures June 30, 1930. £ s. d.	ASSETS—*continued.*	£ s. d.	£ s. d.
45,079 7 9	Brought forward		44,755 16 9
	By *Mr. G. Buckston Browne's Gift* in memory of Darwin—Down House, Kent		Not valued.
	Do. Endowment Fund—		
	£5,500 India 4½ per cent. Stock 1958/68 at cost	5,001 17 4	
	£2,500 Australia 5 per cent. Stock 1945/75 at cost	2,468 19 0	
	£3,000 Fishguard & Rosslare Railway 3½ per cent. Guaranteed Preference Stock at cost.	2,139 17 3	
	£2,500 New South Wales 5 per cent. 1945/65 Stock at cost	2,467 7 9	
	£2,500 Western Australia 5 per cent. 1945/75 Stock at cost	2,472 1 6	
	£3,340 Great Western Railway 5 per cent. Guaranteed Stock at cost	3,436 7 5	
	£2,500 Birkenhead Railway 4 per cent. Consolidated Stock at cost	2,013 9 9	
20,000 0 0			20,000 0 0
	(*£18,486 12s. 0d.* Value at date, £17,303 10*s.* 0*d.*)		
	,, REVENUE ACCOUNT—		
	Investments:—		
	£2,098 1*s.* 9*d.* Consolidated 2½ per cent. Stock at cost	1,200 0 0	
	£4,338 6*s.* 2*d.* Conversion 3½ per cent. Stock at cost	3,300 0 0	
	£400 5 per cent. War Loan Inscribed Stock at cost	404 16 0	
	(*£4,950 16s. 8d.* Value at date, £5,358 8*s.* 4*d.*)	4,904 16 0	
	Second Mortgage on Isleworth House, Orpington	700 0 0	
	,, Down House Suspense Account—		
	As per last Account	938 7 0	
	Purchase of Land adjoining Down House .	275 0 0	
	,, Down House—Income and Expenditure Account		
	Balance at July 1, 1930 . £1,566 4 0		
	Add Excess of Expenditure over Income for the year 315 16 6		
		1,882 0 6	
	Sundry Debtors and Payments in advance .	733 9 6	
	Do. (Down House)	249 15 7	
	Cash at Bank	308 16 0	
	viz: General Account . . £3,639 14 3		
	Less Down House—Charges met out of General Fund . 3,330 18 3		
	£308 16 0		
9,231 8 4	Cash in Hand	141 12 6	
			10,133 17 1
74,310 16 1			£74,889 13 10

to be correct. I have also verified the Balances at the Bankers
Isleworth House.

W. B. KEEN,
Chartered Accountant.

Income and

FOR THE YEAR ENDED

EXPENDITURE.

Corresponding Figures June 30, 1930. £	s.	d.		£	s.	d.	£	s.	d.
20	10	4	To Heat, Lighting and Power	24	19	1			
84	3	6	,, Stationery	157	0	9			
1	0	0	,, Rent	1	0	0			
167	12	10	,, Postages	263	11	8			
118	10	5	,, Travelling Expenses	216	5	8			
			,, Exhibitioners	37	14	11			
229	18	6	,, General Expenses	278	6	1			
621	15	7		978	18	2			
1,499	1	0	,, Salaries and Wages	1,794	7	0			
75	0	0	,, Pension Contribution	75	0	0			
1,060	5	11	,, Printing, Binding, &c.	1,638	2	11			
							4,486	8	1
3,256	2	6							
52	3	10							
			,, Zimbabwe Loan Exhibition				122	1	5
			,, Grants to Research Committees:—						
			Fossil Plants at Fort Gray Committee	16	8	7			
			South African Liverworts Committee	2	6	0			
			Transplant Experiments Committee	25	0	0			
			East African Lakes Committee	200	0	0			
			Galilee Caves Committee	25	0	0			
			Western Desert of Egypt Committee	85	0	0			
			South African Desert Plants Committee	60	0	0			
			Overseas Training Committee	8	0	0			
			Mycorrhiza in relation to Forestry Committee	40	0	0			
			Teaching of General Science in Schools Committee	12	2	5			
			Macedonia Committee	25	0	0			
			Plymouth Laboratory Committee	50	0	0			
			British Somaliland Committee	50	0	0			
			,, Kleinia Articulata Committee	40	0	0			
			,, Chemical Analysis of Upland Bog Waters Committee	8	0	0			
			,, Freshwater Biological Station Committee	40	0	0			
			,, Human Geography of Tropical Africa Committee	2	13	0			
			,, Vocational Tests Committee	40	0	0			
431	1	9	,, Education and Documentary Films Committee	1	17	6			
							731	7	6
2,437	8	0	,, Balance, being excess of Income over Expenditure for the year				531	1	6
6,176	16	1					£5,870	18	6

Note.—The Net Excess of Income for the Year is £215 5*s.* 0*d.*, as follows:—

	£	s.	d.
Balance as above	531	1	6
Less Down House Deficiency	315	16	6
	£215	5	0

Caird

EXPENDITURE.

£	s.	d.		£	s.	d.	£	s.	d.
			To Grants paid—						
			Seismology Committee	200	0	0			
			Bronze Age Implements Committee	50	0	0			
			Mathematical Tables Committee	65	0	0			
			Zoological Record Committee	50	0	0			
200	0	0	Naples Tables Committee	50	0	0			
							415	0	0
163	8	8	,, Balance being excess of Income over Expenditure for the Year				34	8	7
363	8	8					£449	8	7

Expenditure Account

JUNE 30, 1931.

Corresponding Figures June 30, 1930. £ s. d.	INCOME.	£ s. d.	£ s. d.
149 0 0	By Annual Regular Members (including £51, 1931/32)		158 0 0
1,252 5 0	,, Annual Temporary Members (including £659, 1931/32)		1,898 5 0
273 0 0	,, Annual Members with Report (including £331 10s., 1931/32)		634 10 0
10 0 0	,, Transferable Tickets (including £26 5s., 1931/32)		138 15 0
87 10 0	,, Students' Tickets (including £25 10s., 1931/32).		150 10 0
	,, Life Compositions Amount transferred . .		15 0 0
1,523 5 1	,, *Unexpended Balance of Donations in aid of Expenses of South Africa Meeting* . . .		—
15 19 0	,, Donation		1 0 0
1 17 6	,, *Zimbabwe Loan Exhibition—Sale of Catalogues, &c.*		—
70 9 0	,, *Lift Rent*		—
551 16 8	,, Interest on Deposit		9 18 0
227 1 9	,, Sale of Publications		578 15 10
264 3 9	,, Advertisement Revenue		275 7 5
	,, Income Tax Recoverable		227 17 2
	,, Unexpended balance of grants returned . .		31 13 1
	,, Liverpool Exhibitioners		22 10 0
	,, Dividends:—		
	Consols	130 15 5	
	India 3 per cent.	83 14 0	
	Great Indian Peninsula Railway 'B' Annuity	26 12 3	
	4½ per cent. Conversion Loan . . .	33 7 3	
	Do. Sir Charles Parsons' Gift . . .	359 4 3	
	Local Loans	59 6 1	
	War Stock	76 17 6	
	War Stock (Series A), Sir A Yarrow's Gift .	444 1 0	
1,379 10 10	3½ per cent. Conversion Loan . . .	126 10 8	1,340 8 5
	,, Sir Alfred Yarrow's Gift—		
	Proceeds of Sale of £348 War Loan in accordance with the terms of the Gift . . .	348 0 0	
342 17 6	Profit on Sale	13 6 1	361 6 1
28 0 0	,, Interest on Mortgage		27 2 6
6,176 16 1			£5,870 18 6

Fund.

£ s. d.	INCOME.	£ s. d.	£ s. d.
	By Dividends—		
	India 3½ per cent. Stock	71 5 0	
	Canada 3½ per cent. Stock	67 16 2	
	London, Midland & Scottish Railway Consolidated 4 per cent. Preference Stock .	65 2 0	
290 15 0	Southern Railway Consolidated 5 per cent. Preference Stock	77 10 0	281 13 2
72 13 8	,, Income Tax Recoverable		72 13 8
	,, Unexpended Balance of Grants returned . .		95 1 9
363 8 8			£449 8 7

d 2

Down House,

EXPENDITURE.

Corresponding Figures June 30, 1930. £ s. d.		£ s. d.	£ s. d.
754 17 11	To Wages of Staff		788 10 0
149 2 7	,, Rates, Insurance, etc.		110 8 3
115 6 2	,, Heat, Light and Drainage		182 12 5
27 3 4	,, Repairs and Renewals		31 15 7
37 1 9	,, House and Garden Sundries		36 16 5
56 7 11	,, General Expenses		69 14 11
	,, Printing		21 4 7
15 12 8	,, Photo Postcards		2 3 6
1,155 12 4			£1,243 5 8
201 2 5	To Balance brought down		115 5 5
482 4 0	,, House and Garden Equipment, Repairs, Renewals and Alterations to Buildings, Walls, Paths, etc.		167 9 4
183 4 0	,, *Law Costs.*		
	,, Costs *re* Rates Appeal	7 19 0	
	,, Cost of Inventory and Valuation	25 2 9	
			33 1 9
866 10 5			£315 16 6

Memorandum Account

	£ s. d.	£ s. d.
To Sundry Creditors	14 4 10	
,, Expenditure met from General Fund	3,330 18 3	
		3,345 3 1
		£3,345 3 1

June 30, 1931.

Corresponding Figures June 30, 1930. £ s. d.	INCOME.	£ s. d.	£ s. d.
86 16 8	By Rents Receivable		142 3 4
55 2 0	,, Income Tax Recoverable		203 11 1
	,, Dividends—		
	4½ per cent. India Stock	191 16 4	
	Fishguard and Rosslare Railway 3½ per cent. Stock	81 7 6	
	New South Wales 5 per cent. Stock . .	96 17 6	
	Great Western Railway 5 per cent. Stock .	129 8 6	
	Australia 5 per cent. Stock 1945/75 . .	96 17 6	
	Western Australia 5 per cent. Stock . .	96 17 6	
790 18 11	Birkenhead Railway 4 per cent. Stock . .	77 10 0	770 14 10
14 17 9	,, Donations		1 0 11
6 14 7	,, Sale of Postcards, etc.		10 10 1
201 2 5	,, Balance carried down		115 5 5
1,155 12 4			£1,243 5 8
866 10 5	By Balance being excess of Expenditure over Income for the Year		315 16 6
866 10 5			£315 16 6

of Down House.

		£ s. d.	£ s. d.	£ s. d.
By Suspense Account—				
Compensation paid to outgoing tenant and Redemption of Tithe			938 7 0	
,, Purchase of Land adjoining Down House . .			275 0 0	
,, Sundry Debtors			249 15 7	
			1,463 2 7	
,, Income and Expenditure Account—				
Balance at July 1, 1930 .	1,566 4 0			
Add Excess of Expenditure over Income for year as per separate Income and Expenditure Account . .	315 16 6		1,882 0 6	
				£3,345 3 1

RESEARCH COMMITTEES, Etc.

APPOINTED BY THE GENERAL COMMITTEE, MEETING IN LONDON, 1931.

Grants of money, if any, from the Association for expenses connected with researches are indicated in heavy type.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCES.

Seismological Investigations.—Dr. F. J. W. Whipple (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Sir F. W. Dyson, Sir R. T. Glazebrook, Mr. Wilfred Hall, Dr. H. Jeffreys, Sir H. Lamb, Prof. H. M. Macdonald, Prof. E. A. Milne, Mr. R. D. Oldham, Prof. H. C. Plummer, Prof. A. O. Rankine, Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, Mr. F. J. Scrase, Sir Napier Shaw, Capt. H. Shaw, Sir F. E. Smith, Mr. R. Stoneley, Sir G. T. Walker. **£250** (including **£100**, Caird Fund grant).

Calculation of Mathematical Tables.—Prof. E. H. Neville (*Chairman*), Dr. L. J. Comrie (*Secretary*), Prof. A. Lodge (*Vice-Chairman*), Dr. J. R. Airey, Dr. R. A. Fisher, Dr. J. Henderson, Dr. J. O. Irwin, Dr. E. S. Pearson, Mr. F. Robbins, Dr. A. J. Thompson, Dr. J. F. Tocher, Dr. J. Wishart. **£93** (Caird Fund grant).

SECTION B.—CHEMISTRY.

To Collect and Tabulate all available data on the Parachors of Chemical Compounds with a view to their subsequent publication.—Dr. N. V. Sidgwick (*Chairman*), Dr. S. Sugden (*Secretary*), Dr. N. K. Adam. **£10.**

SECTION C.—GEOLOGY.

To excavate Critical Sections in the Palæozoic Rocks of England and Wales.—Prof. W. W. Watts (*Chairman*), Prof. W. G. Fearnsides (*Secretary*), Mr. W. S. Bisat, Dr. H. Bolton, Prof. W. S. Boulton, Dr. E. S. Cobbold, Prof. A. H. Cox, Mr. E. E. L. Dixon, Dr. Gertrude Elles, Prof. E. J. Garwood, Prof. H. L. Hawkins, Prof. V. C. Illing, Prof. O. T. Jones, Prof. J. E. Marr, Dr. F. J. North, Mr. J. Pringle, Dr. T. F. Sibly, Dr. W. K. Spencer, Prof. A. E. Trueman, Dr. F. S. Wallis. **£20.**

The Collection, Preservation, and Systematic Registration of Photographs of Geological Interest.—Prof. E. J. Garwood (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. C. V. Crook, Mr. E. G. W. Elliott, Mr. J. F. Jackson, Mr. J. Ranson, Prof. W. W. Watts, Mr. R. J. Welch.

To investigate Critical Sections in the Tertiary and Cretaceous Rocks of the London Area. To tabulate and preserve records of new excavations in that area.—Prof. W. T. Gordon (*Chairman*), Dr. S. W. Wooldridge (*Secretary*), Mr. H. C. Berdinner, Prof. P. G. H. Boswell, Miss M. C. Crosfield, Mr. F. Gosling, Prof. H. L. Hawkins, Prof. G. Hickling. **£10.**

The Stratigraphy and structure of the Palæozoic Sedimentary Rocks of West Cornwall. —Mr. H. Dewey (*Chairman*), Mr. E. H. Davison (*Secretary*), Mr. H. G. Dines, Miss E. M. Lind Hendriks, Mr. S. Hall, Dr. S. W. Wooldridge.

To investigate the Travertines of the Kharga Oasis, Africa.—Dr. Gertrude L. Elles (*Chairman*), Miss E. W. Gardner (*Secretary*), Mr. W. N. Edwards, Dr. W. F. Hume. **£20.**

To consider and report upon Petrographic Classification and Nomenclature.—Mr. W. Campbell Smith (*Chairman*), Dr. A. K. Wells (*Secretary*), Prof. P. G. H. Boswell, Prof. A. Holmes, Prof. A. Johannsen, Prof. P. Niggli, Prof. H. H. Read, Prof. S. J. Shand, Dr. H. H. Thomas, Prof. C. C. Tilley, Dr. G. W. Tyrrell.

SECTIONS C, D, E, H.—GEOLOGY, ZOOLOGY, GEOGRAPHY, ANTHROPOLOGY.

Expedition to investigate the Biology, Geology, and Geography of Lakes Baringo and Rudolf, Northern Kenya and Lake Edward, Uganda.—Prof. J. S. Gardiner (*Chairman*), E. B. Worthington and J. T. Saunders (*Secretaries*), Dr. W. T. Calman, Prof. J. W. Gregory, Prof. R. N. Rudmose Brown, Dr. L. S. B. Leakey.

SECTIONS C, D, E, K.—GEOLOGY, ZOOLOGY, GEOGRAPHY, BOTANY.

To organise an expedition to investigate the Biology, Geology, and Geography of the Australian Great Barrier Reef.—Rt. Hon. Sir M. Nathan (*Chairman*), Sir E. H. Macartney (*Treasurer*), Prof. J. Stanley Gardiner and Mr. F. A. Potts (*Secretaries*), Dr. W. T. Calman, Dr. C. M. Yonge.

SECTION D.—ZOOLOGY.

Zoological Bibliography and Publication.—Prof. E. B. Poulton (*Chairman*), Dr. F. A. Bather (*Secretary*), Mr. E. Heron-Allen, Dr. W. T. Calman, Sir P. Chalmers Mitchell, Mr. W. L. Sclater.

To nominate competent Naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Prof. J. H. Ashworth (*Chairman and Secretary*), Prof. H. Graham Cannon, Prof. H. Munro Fox, Prof. J. Stanley Gardiner. **£50**.

To co-operate with other Sections interested, and with the Zoological Society, for the purpose of obtaining support for the Zoological Record.—Sir Sidney Harmer (*Chairman*), Dr. W. T. Calman (*Secretary*), Prof. E. S. Goodrich, Prof. D. M. S. Watson. **£50** (Caird Fund grant).

On the Influence of the Sex Physiology of the Parents on the Sex-Ratio of the Offspring. —Prof. J. H. Orton (*Chairman*), Mrs. Bisbee (*Secretary*), Prof. Carr-Saunders, Miss E. C. Herdman. **£10.**

To consider the position of Animal Biology in the School Curriculum and matters relating thereto.—Prof. R. D. Laurie (*Chairman and Secretary*), Mr. H. W. Ballance, Dr. Kathleen E. Carpenter, Mr. O. H. Latter, Prof. E. W. MacBride, Miss M. McNicol, Miss A. J. Prothero, Prof. W. M. Tattersall, Dr. Ethel N. Miles Thomas.

SECTIONS D, I, K.—ZOOLOGY, PHYSIOLOGY, BOTANY.

To aid competent investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.—Prof. J. H. Ashworth (*Chairman and Secretary*), Prof. J. Barcroft, Prof. E. W. MacBride, Dr. M. Knight. **£50** (Caird Fund grant).

SECTIONS D, K.—ZOOLOGY, BOTANY.

To try to arrange for the observation and recording of changes in the Flora and Fauna of St. Kilda since its evacuation.—Prof. J. Ritchie (*Chairman*), Prof. F. A. E. Crew (*Secretary*), Dr. A. Bowman, Prof. J. Graham Kerr, Dr. C. H. O'Donoghue, Dr. Lloyd Praeger, Prof. J. Walton.

To aid competent investigators selected by the Committee to carry out appropriate types of work at the Freshwater Biological Station, Wray Castle, Windermere.—Prof. F. E. Fritsch (*Chairman*), Mr. J. T. Saunders (*Secretary*), Dr. B. M. Griffiths, Miss P. M. Jenkin, Dr. C. H. O'Donoghue. **£75.**

SECTION E.—GEOGRAPHY.

To co-operate with the Ordnance Survey in the production of a Population Density Map (or Maps) of Great Britain and to endeavour to get this published as soon as the 1931 Census is available; and, further, to examine the possibility of making similar Maps of the Empire, utilising the International Map (1 : 1,000,000) as the base.—Brig. H. St. J. Winterbotham (*Chairman*), (*Secretary*), Mr. J. Bartholomew, Mr. F. Debenham, Prof. C. B. Fawcett, Prof. H. J. Fleure, Mr. H. King, Mr. R. H. Kinvig, Prof. A. G. Ogilvie, Prof. O. H. T. Rishbeth, Prof. P. M. Roxby, Mr. A. Stevens, Capt. J. G. Withycombe.

To inquire into the present state of Knowledge of the Human Geography of Tropical Africa, and to make recommendations for furtherance and development.—Prof. P. M. Roxby (*Chairman*), Prof. A. G. Ogilvie (*Secretary*), Prof. C. B. Fawcett, Prof. H. J. Fleure, Mr. E. B. Haddon, Mr. R. H. Kinvig, Mr. J. McFarlane, Mr. R. U. Sayce, Rev. E. W. Smith, Brig. H. St. J. Winterbotham. **£25.**

To ascertain the place which Geography occupies in the Curricula of the Universities in the various Dominions of the Empire.—Prof. C. B. Fawcett (*Chairman*), Dr. L. Dudley Stamp (*Secretary*), Mr. L. J. Burpee, Prof. F. Debenham, Dr. C. Fenner, Prof. Griffith Taylor, Prof. J. H. Wellington.

SECTIONS E, K.—GEOGRAPHY, BOTANY.

To consider the possibility of publishing the series of sixteen Maps of England and Wales prepared by Mrs. Treleaven to illustrate the distribution of Woodland and Marsh before the alterations effected during historic times and originally presented by her to Section E at the Hull Meeting (1922).—Sir John Russell (*Chairman*), Prof. P. M. Roxby (*Secretary*); Prof. H. J. Fleure, Mr. R. H. Kinvig (*Acting Secretary*), Prof. A. G. Ogilvie (*from Section E*); Dr. E. J. Salisbury, Dr. T. W. Woodhead (*from Section K*).

SECTIONS E, L.—GEOGRAPHY, EDUCATION.

To report on the present position of Geographical Teaching in Schools, and of Geography in the training of teachers; to formulate suggestions for a syllabus for the teaching of geography both to Matriculation Standard and in Advanced Courses and to report, as occasion arises, to Council through the Organising Committee of Section E upon the practical working of Regulations issued by the Board of Education (including the Scottish Education Department) affecting the position of Geography in Schools and Training Colleges.—Prof. Sir T. P. Nunn (*Chairman*), Mr. L. Brooks (*Secretary*), Mr. A. B. Archer, Mr. J. N. L. Baker, Mr. C. C. Carter, Prof. H. J. Fleure, Mr. O. J. R. Howarth, Mr. H. E. M. Icely, Mr. J. McFarlane, Rt. Hon. Sir Halford J. Mackinder, Prof. J. L. Myres, Dr. Marion Newbigin, Prof. A. G. Ogilvie, Mr. A. Stevens, Prof. C. B. Fawcett (*from Section E*); Mr. C. E. Browne, Sir R. Gregory, Mr. E. R. Thomas, Miss O. Wright, Prof. Godfrey Thomson (*from Section L*).

SECTION G.—ENGINEERING.

Earth Pressures.—Mr. F. E. Wentworth-Sheilds (*Chairman*), Dr. J. S. Owens (*Secretary*), Prof. G. Cook, Mr. T. E. N. Fargher, Prof. A. R. Fulton, Prof. F. C. Lea, Prof. R. V. Southwell, Dr. R. E. Stradling, Dr. W. N. Thomas, Mr. E. G. Walker, Mr. J. S. Wilson.

Electrical Terms and Definitions.—Prof. Sir J. B. Henderson (*Chairman*), Prof. F. G. Baily and Prof. G. W. O. Howe (*Secretaries*), Prof. W. Cramp, Dr. W. D. Dye, Prof. W. H. Eccles, Prof. C. L. Fortescue, Sir R. Glazebrook, Prof. A. E. Kennelly, Prof. E. W. Marchant, Sir F. E. Smith, Dr. W. E. Sumpner, Prof. L. R. Wilberforce.

Stresses in Overstrained Materials.—Sir Henry Fowler (*Chairman*), Dr. J. G. Docherty (*Secretary*), Prof. G. Cook, Prof. B. P. Haigh, Mr. J. S. Wilson.

SECTION H.—ANTHROPOLOGY.

To report on the Distribution of Bronze Age Implements.—Prof. J. L. Myres (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Mr. A. Leslie Armstrong, Mr. H. Balfour, Prof. T. H. Bryce, Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Mr. O. G. S. Crawford, Prof. H. J. Fleure, Dr. Cyril Fox, Mr. G. A. Garfitt. **£25** (Caird Fund grant).

To excavate Early Sites in Macedonia.—Prof. J. L. Myres (*Chairman*), Mr. S. Casson (*Secretary*), Dr. W. L. H. Duckworth, Mr. M. Thompson. **£25.**

To report on the Classification and Distribution of Rude Stone Monuments.—Mr. H. J. E. Peake (*Chairman*), Miss M. A. Murray (*Secretary*), Mr. A. L. Armstrong, Mr. H. Balfour, Prof. V. Gordon Childe, Dr. Cyril Fox, Mr. T. D. Kendrick, Mr. R. V. Sayce.

To report on the probable sources of the supply of Copper used by the Sumerians.—Mr. H. J. E. Peake (*Chairman*), Mr. G. A. Garfitt (*Secretary*), Mr. H. Balfour, Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Prof. C. H. Desch, Prof. H. J. Fleure, Sir Flinders Petrie, Mr. Rastall.

To conduct Archæological and Ethnological Researches in Crete.—Prof. J. L. Myres (*Chairman*), Mr. L. Dudley Buxton (*Secretary*), Dr. W. L. H. Duckworth, Sir A. Evans, Dr. F. C. Shrubsall.

The Investigation of a hill fort site at Llanmelin, near Caerwent.—Dr. Willoughby Gardner (*Chairman*), Dr. Cyril Fox (*Secretary*), Prof. H. J. Fleure, Mr. H. J. E. Peake, Prof. H. J. Rose, Dr. R. Mortimer Wheeler. **£25.**

To co-operate with the Torquay Antiquarian Society in investigating Kent's Cavern.—Sir A. Keith (*Chairman*), Prof. J. L. Myres (*Secretary*), Mr. M. C. Burkitt, Dr. R. V. Favell, Mr. G. A. Garfitt, Miss D. A. E. Garrod, Mr. Lacaille.

To co-operate with a Committee of the Royal Anthropological Institute in the exploration of Caves in the Derbyshire district.—Mr. M. C. Burkitt (*Chairman*), Mr. G. A. Garfitt (*Secretary*), Mr. A. Leslie Armstrong, Mr. E. N. Fallaize, Dr. R. V. Favell, Prof. H. J. Fleure, Miss D. A. E. Garrod, Dr. J. Wilfrid Jackson, Dr. L. S. Palmer, Mr. H. J. E. Peake. **£50.**

To investigate processes of Growth in Children, with a view to discovering Differences due to Race and Sex, and further to study Racial Differences in Women.—Sir A. Keith (*Chairman*), Prof. H. J. Fleure (*Secretary*), Mr. L. H. Dudley Buxton, Prof. A. Low, Prof. F. G. Parsons, Dr. F. C. Shrubsall.

To report on the progress of Anthropological Teaching in the present century.—Dr. A. C. Haddon (*Chairman*), Prof. J. L. Myres (*Secretary*), Prof. H. J. Fleure, Dr. R. R. Marett, Prof. C. G. Seligman.

To make a preliminary survey of some reported archæological sites in British Somaliland.—Dr. A. C. Haddon (*Chairman*), Mr. R. U. Sayce (*Secretary*), Prof. J. L. Myres.

To co-operate with Miss Caton-Thompson in her researches in prehistoric sites in the Western Desert of Egypt.—Prof. J. L. Myres (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Mr. H. Balfour. **£100.**

To co-operate with Mr. Burton Brown in archæological researches in Asia Minor.—Prof. J. L. Myres (*Chairman*), Prof. V. Gordon Childe (*Secretary*). **£10.**

To report to the Sectional Committee on the question of re-editing 'Notes and Queries in Anthropology.'—Mrs. B. Aitken (*Chairman*), Dr. A. I. Richards (*Secretary*), Mr. L. Dudley Buxton, Prof. C. Daryll Forde, Dr. A. C. Haddon, Capt. T. A. Joyce, Prof. C. G. Seligman, Mrs. Seligman, Miss C. Wedgwood.

To inquire into bibliographical and cataloguing arrangements already in existence in the subjects of Anthropology and Archæology with a view to providing such supplements as may be deemed necessary.—Mr. H. J. E. Peake (*Chairman*), Mr. L. W. G. Malcolm (*Secretary*), Mr. M. C. Burkitt, Prof. V. Gordon Childe, Mr. K. de B. Codrington, Mr. A. M. Hocart, Dr. E. E. Evans-Pritchard, Prof. C. G. Seligman.

SECTION I.—PHYSIOLOGY.

Colour Vision.—Prof. Sir Charles Sherrington (*Chairman*), Prof. H. E. Roaf (*Secretary*), Dr. Mary Collins, Dr. F. W. Edridge Green, Prof. H. Hartridge, Dr. J. H. Shaxby.

The supply of Oxygen at high altitudes.—Prof. J. Barcroft (*Chairman*), Mr. N. E. Odell, Mr. G. S. Adair, Dr. Raymond Greene, Major J. A. Sadd. **£5.**

To deal with the use of a Stereotatic Instrument.—Prof. J. Mellanby (*Chairman*), Mr. F. R. Curtis (*Secretary*).

SECTION J.—PSYCHOLOGY.

The factors involved in Mechanical Ability.—Dr. C. S. Myers (*Chairman*), Dr. G. H. Miles (*Secretary*), Prof. C. Burt, Mr. F. M. Earle, Dr. Ll. Wynn Jones, Prof. T. H. Pear. **£32.**

The Reliability of the Criteria used for assessing the value of Vocational Tests.—Prof. J. Drever (*Chairman*), Mr. Eric Farmer (*Secretary*), Dr. William Brown, Prof. C. Burt, Dr. J. O. Irwin, Dr. C. S. Myers. **£8**.

An inquiry into (*a*) the occupations for which a training in Psychology is necessary or desirable, (*b*) the place Psychology should occupy in the curricula for University Degrees in Arts, Science, Medicine, Education, Economics and other subjects.—Prof. F. C. Bartlett (*Chairman*), Mr. A. Rex Knight (*Secretary*), Dr. F. Aveling, Dr. W. Brown, Prof. J. Drever, Prof. B. Edgell, Mr. C. A. Mace, Prof. T. H. Pear, Dr. R. H. Thouless, Prof. C. W. Valentine, Mr. A. W. Wolters.

SECTION K.—BOTANY.

Transplant Experiments.—Sir A. W. Hill (*Chairman*), Dr. W. B. Turrill (*Secretary*), Prof. F. W. Oliver, Dr. E. J. Salisbury, Prof. A. G. Tansley. **£10**.

To consider and report on the provision made for Instruction in Botany in courses of Biology, and matters related thereto.—Prof. V. H. Blackman (*Chairman*), Dr. E. N. M. Thomas (*Secretary*), Prof. M. Drummond, Prof. F. E. Fritsch, Sir A. W. Hill, Prof. S. Maugham, Mr. J. Sager. **£5**.

Mycorrhiza in relation to Forestry.—Mr. F. T. Brooks (*Chairman*), Dr. M. C. Rayner (*Secretary*), Mr. W. H. Guillebaud. **£25**.

Fossil Plants at Fort Gray, near East London.—Dr. A. W. Rogers (*Chairman*), Prof. R. S. Adamson (*Secretary*), Prof. A. C. Seward.

The Morphology and Systematics of certain South African Liverworts and Ferns.—Prof. R. S. Adamson (*Chairman*), Prof. H. S. Holden (*Secretary*), Prof. R. H. Compton, Mrs. M. R. Levyns, Mr. N. S. Pillans. **£4 4s.** (Unexpended balance.)

To investigate the effect of conditions on the growth, structure and metabolism of Kleinia articulata.—Prof. D. Thoday (*Chairman*), Mr. N. Woodhead (*Secretary*), Dr. F. F. Blackman. **£25**.

SECTION L.—EDUCATIONAL SCIENCE.

The teaching of General Science in Schools, with special reference to the teaching of Biology.—Prof. Sir T. P. Nunn (*Chairman*), Mr. G. W. Olive (*Secretary*), Mr. C. E. Browne, Dr. Lilian J. Clarke, Mr. G. D. Dunkerley, Mr. S. R. Humby, Dr. E. W. Shann, Mr. E. R. Thomas, Mrs. Gordon Wilson, Miss von Wyss. **£20**.

Educational and Documentary Films: To enquire into the production and distribution thereof, to consider the use and effects of films on pupils of school age and older students, and to co-operate with other bodies which are studying those problems.—Sir Richard Gregory (*Chairman*), Mr. J. L. Holland (*Secretary*), Mr. L. Brooks, Mr. A. C. Cameron, Miss E. R. Conway, Mr. G. D. Dunkerley, Mr. A. Clow Ford, Dr. C. W. Kimmins, Prof. J. L. Myres, Mr. G. W. Olive, Hon. S. Rivers-Smith, Dr. Spearman, Dr. H. Hamshaw Thomas (*Section K*), Dr. F. W. Edridge Green (*Section I*). **£45**.

SECTIONS M, E.—AGRICULTURE, GEOGRAPHY.

To co-operate with the staff of the Imperial Soil Bureau to examine the soil resources of the Empire.[1]—Sir John Russell (*Chairman*), Mr. G. V. Jacks (*Secretary*), Dr. E. M. Crowther, Dr. W. G. Ogg, Prof. G. W. Robinson (*from Section M*), Prof. C. B. Fawcett, Mr. H. King, Mr. A. Stevens, Dr. S. W. Wooldridge (*from section E*).

CORRESPONDING SOCIETIES.

Corresponding Societies Committee.—The President of the Association (*Chairman ex-officio*), Mr. T. Sheppard (*Vice-Chairman*), Dr. C. Tierney (*Secretary*), the General

[1] Committee authorised by Council, December 4, 1931.

Secretaries, the General Treasurer, Mr. C. O. Bartrum, Dr. F. A. Bather, Sir Richard Gregory, Mr. J. V. Pearman, Sir David Prain, Sir John Russell, Prof. W. M. Tattersall.

Committee to take cognisance of proposals relating to National Parks by the Government and other authorities and bodies concerned, and to advise the Council as to action if desirable.—Dr. Vaughan Cornish (*Section E, Chairman*), Dr. C. Tierney (*Secretary*), Prof. P. Abercrombie, Mr. T. Sheppard, Prof. W. M. Tattersall (*Corresponding Societies*), Prof. A. H. Cox (*Section C*), Sir Chalmers Mitchell (*Section D*), Dr. H. S. Harrison (*Section H*), Sir D. Prain (*Section K*).

RESOLUTIONS & RECOMMENDATIONS.

The following resolutions and recommendations were referred to the Council by the General Committee (unless otherwise stated) for consideration and, if desirable, for action :—

From Section D.

That the British Association, although recognising that the Government of Uganda has put the Gorilla on its absolutely protected list, feels that such individual protection is an insufficient safeguard for these animals, and urges the Colonial Office to comply with the request of the Government of Belgium, the Society for the Preservation of the Fauna of the Empire and the Zoological Society of London, and place a small area in Uganda, marching with the Parc National Albert, on the same permanent footing as a National Park.

From Section E.

That the Council be asked to represent to the Registrars General for England and Wales and for Scotland the urgent need for the inclusion in the Reports of the 1931 Census of maps showing the distribution and density of the population.

It is understood that such maps can be made available by the Ordnance Survey. An experimental sheet showing the distribution of population in Hampshire is submitted.

(Approved by the General Committee for immediate action.)

From Section H.

That the Organising Committee of the International Congress for Prehistoric and Protohistoric Sciences be asked to make provision, if possible, at its Congress in 1932, for :—

(*a*) An exhibition and lecture demonstration of the Bronze Implements Catalogue of the British Association.

(*b*) An exhibition and lecture demonstration of the Megalithic Catalogue of the British Association.

From Section H.

The Derbyshire Caves Committee, having considered the question of the ultimate disposal of the finds from the Creswell Caves, and the request of the authorities of the County Museum, Derby, that the collection be placed therein, in view of an undertaking to keep the collection together and to display it in proper cases, recommends that the offer of the County Museum be accepted and that the whole of the artefacts be deposited there with the exception of the engravings. It recommends that the engraved bones should be presented to the national collection, and that casts should be made for distribution to the museum at Derby.

From the Conference of Delegates of Corresponding Societies.
(*Amended by the Committee of Recommendations.*)

The Delegates of Corresponding Societies in Conference on September 24, 1931, in London, desire to impress on the editors of all scientific publications, especially those issued by the Corresponding Societies, the importance of printing at least a limited issue of those publications, both text and plates, on a durable paper (such as those designated Grades 1 and 2 by the Library Association in its recent Report (1930), and request the Council of the British Association to communicate this resolution with its endorsement to all publishing societies with which it is in correspondence.

THE PRESIDENTIAL ADDRESS.

THE SCIENTIFIC WORLD-PICTURE OF TO-DAY.

BY

GENERAL THE RT. HON. J. C. SMUTS, P.C., C.H., F.R.S.

PRESIDENT OF THE ASSOCIATION.

After what I said at the opening this afternoon it is unnecessary for me to emphasise further the significance of this Centenary Meeting of our Association. It is a milestone which enables us to look back upon a hundred years of scientific progress, such as has no parallel in history. It brings us to a point in the advance from which we can confidently look forward to fundamental solutions and discoveries in the near future, which may transform the entire field of science. In this second and greater renaissance of the human spirit this Association and its members have borne a foremost part, to which it would be impossible for me to do justice to-night. I shall therefore not attempt to review the achievements of this century of science, but shall content myself with the simpler undertaking of giving a generalised composite impression of the present situation in science. The honour of presiding over this historic meeting, which was not of my seeking, and for which I was chosen on grounds other than my personal merits, is indeed an almost overwhelming one, and I confidently appeal for your indulgence in the difficult task which awaits me to-night.

I am going to ask the question to-night: What sort of world-picture is science leading to? Is science tending towards a definite scientific outlook on the universe, and how does it differ from the traditional outlook of commonsense?

The question is not without its interest. For our world-view is closely connected with our sense of ultimate values, our reading of the riddle of the universe, and of the meaning of life and of human destiny. Our scientific world-picture will draw its material from all the sciences. Among

these, physical science will—in view of its revolutionary discoveries in recent years—be a most important source. But no less important will be the contribution of the biological sciences with their clear revelation of organic structure and function as well as of organic evolution. And last, not least, the social and mental sciences will not only supply valuable material, but especially methods of interpretation, insights into meanings and values, without which the perspectives of our world-picture would be hopelessly wrong.

Can we from some reunion or symposium of these sciences obtain a world-picture or synoptic view of the universe, based on observation and calculation, which are the instruments of science, but reaching beyond the particular phenomena which are its immediate field to a conception of the universe as a whole ?

That was how science began—in the attempt to find some simple substances or elements to which the complex world of phenomena could in the last analysis be reduced. The century over which we now look back, with its wonderful advance in the methods and technique of exact observation, has been a period of specialisation or decentralisation. Have we now reached a point where science can again become universal in its ultimate outlook ? Has a scientific world-picture become possible ?

Of course there can be no final picture at any one stage of culture. The canvas is as large as the universe, and the moving finger of humanity itself will fill it in from age to age. All the advances of knowledge, all the new insights gained from those advances will from time to time be blended into that picture. To the deeper insight of every era of our human advance there has been some such world-picture, however vague and faulty. It has been continually changing with the changing knowledge and beliefs of man. Thus, there was the world of magic and animism, which was followed by that of the early nature gods. There was the geocentric world which still survives in the world of commonsense. There is the machine or mechanistic world-view dominant since the time of Galileo and Newton, and now, since the coming of Einstein, being replaced by the mathematician's conception of the universe as a symbolic structure of which no mechanical model is possible. All these world-views have in turn obtained currency according as some well-defined aspect of our advancing knowledge has from time to time been dominant. My object to-night is to focus attention on the sort of world-picture which results from the advances of physical, biological and mental science during the period covered roughly by the activities of our Association.

Science arose from our ordinary experience and commonsense outlook. The world of commonsense is a world of matter, of material stuff, of real separate things and their properties which act on each other and cause changes in each other. To the various things observable by the senses were added the imperceptible things—space and time, invisible forces, life and the soul. Even these were not enough, and the supernatural was added to the natural world. The original inventory was continually being enlarged, and thus a complex empirical world-view arose, full of latent contradictions, but with a solid basis of actual experience and facts behind it.

Speaking generally, we may say that this is substantially still the commonsense view of the world and the background of our common practical beliefs. How has science dealt with this commonsense empirical world-view ? The fundamental procedure of science has been to rely on sense observation and experiment, and to base theory on fact. Thus the vast body of exact science arose, and all entities were discarded which were either inconsistent with observed facts or unnecessary for their strict interpretation. The atomic view of matter was established. Ether was given a status in the physical order, which is now again being questioned in the light of the conception of space-time. New entities like energy emerged ; old entities like forces disappeared ; the principle of the uniformity of nature was established ; the laws of motion, of conservation, and of electro-magnetism were formulated ; and on their basis a closed mechanistic order of nature was constructed, forming a rigid deterministic scheme. Into this scheme it has been difficult, if not impossible, to fit entities like life and mind ; and the scientific attitude has on the whole been to put them to a suspense account and to await developments. As to the supernatural, science is or has been agnostic, if not frankly sceptical. Such, in very general terms, was the scientific outlook of the nineteenth century, which has not yet completely passed away. It will be noticed that much of the fundamental outlook of commonsense has thus survived, though clarified and purified by a closer accord with facts. This scientific view retained unimpaired and indeed stressed with a new emphasis the things of commonsense, matter, time and space, as well as all material or physical entities which are capable of observation or experimental verification. Nineteenth-century science is, in fact, a system of purified, glorified commonsense. Its deterministic theory certainly gave a shock to the common man's instinctive belief in free will ; in most other respects it conformed to the outlook of commonsense. It is true that its practical

inventions have produced the most astounding changes in our material civilisation, but neither in its methods nor in its world-outlook was there anything really revolutionary.

But underneath this placid surface, the seeds of the future were germinating. With the coming of the twentieth century, fundamental changes began to set in. The new point of departure was reached when physical science ceased to confine its attention to the things that are observed. It dug down to a deeper level, and below the things that appear to the senses, it found, or invented, at the base of the world, so-called scientific entities, not capable of direct observation, but which are necessary to account for the facts of observation. Thus, below molecules and atoms still more ultimate entities appeared; radiations, electrons and protons emerged as elements which underlie and form our world of matter. Matter itself, the time-honoured mother of all, practically disappeared into electrical energy.

> 'The cloud-capp'd towers, the gorgeous palaces,
> The solemn temples, the great globe itself:'

yea, all the material forms of earth and sky and sea were dissolved and spirited away into the blue of energy. Outstanding among the men who brought about this transformation are two of my predecessors in this Chair: Sir J. J. Thomson and Lord Rutherford. Like Prospero, like Shakespeare himself, they must be reckoned among the magicians.

Great as was this advance, it does not stand alone. Away in the last century, Clerk Maxwell, following up Faraday's theories and experiments, had formulated his celebrated equations of the electro-magnetic field, which applied to light no less than to electro-magnetism, and the exploration of this fruitful subject led Minkowski to the amazing discovery in 1908 that time and space were not separate things, but constituent elements in the deeper synthesis of space-time. Thus time is as much of the essence of things as space; it enters from the first into their existence as an integral element. Time is not something extra and superadded to things in their behaviour, but is integral and basic to their constitution. The stuff of the world is thus envisaged as events instead of material things.

This physical concept or insight of space-time is our first revolutionary innovation, our first complete break with the old world of commonsense. Already it has proved an instrument of amazing power in the newer physics. In the hands of an Einstein it has led beyond Euclid and Newton, to the recasting of the law and the concept of gravitation, and to the new relativity

conception of the basic structure of the world. The transformation of the concept of space, owing to the injection into it of time, has destroyed the old passive homogeneous notion of space and has substituted a flexible, variable continuum, the curvatures and unevennesses of which constitute to our senses what we call a material world. The new concept has made it possible to construe matter, mass and energy as but definite measurable conditions of curvature in the structure of space-time. Assuming that electro-magnetism will eventually follow the fate of gravitation, we may say that space-time will then appear as the scientific concept for the only physical reality in the universe, and that matter and energy in all their forms will have disappeared as independent entities, and will have become mere configurations of this space-time. This will probably involve an amplified concept of space-time. Einstein has recently indicated that for further advance a modification in our space-time concept will become necessary, and that the additional element of direction will have to be incorporated into it. Whatever change may become necessary in our space-time concept, there can be no doubt about the immense possibilities it has opened up.

I pass on to an even more revolutionary recent advance of physics. The space-time world, however novel, however shattering to commonsense, is not in conflict with reason. Indeed, the space-time world is largely a discovery of the mathematical reason and is an entirely rational world. It is a world where reason, as it were, dissolves the refractoriness of the old material substance and smoothes it out into forms of space-time. Science, which began with empirical brute facts, seems to be heading for the reign of pure reason. But wait a bit; another fundamental discovery of our age has apparently taken us beyond the bounds of rationality, and is thus even more revolutionary than that of space-time. I refer to the Quantum theory, Max Planck's discovery at the end of the nineteenth century, according to which energy is granular, consisting of discrete grains or quanta. The world in space-time is a continuum; the quantum action is a negation of continuity. Thus arises the contradiction, not only of commonsense, but apparently also of reason itself. The quantum appears to behave like a particle, but a particle out of space or time. As Sir Arthur Eddington graphically puts it: a quantum of light is large enough to fill the lens of a hundred-inch telescope, but it is also small enough to enter an atom. It may spread like a circular wave through the universe, but when it hits its mark, this cosmic wave instantaneously contracts to a point where it strikes with its full and undivided force.

Space-time, therefore, does not seem to exist for the quantum, at least not in its lower multiples. Nay, more: the very hitting of its mark presents another strange puzzle, which seems to defy the principles of causation and of the uniformity of nature, and to take us into the realm of chance and probability. The significant thing is that this strange quantum character of the universe is not the result of theory but is an experimental fact well attested from several departments of physics. In spite of the strange Puck-like behaviour of the quantum, we should not lightly conclude, with some prominent physicists, that the universe has a skeleton in its cupboard in the shape of an irrational or chaotic factor. Our macroscopic concepts may not fit this ultra-microscopic world of the quantum. And our best hopes for the future are founded on the working out of a new system of concepts and laws suited to this new world that has swum into the ken of science. The rapid development of wave mechanics in the last four years seems to have brought us within sight of this ideal, and we are beginning to discern a new kind of order in the microscopic elements of the world, very different from any type of law hitherto imagined in science, but none the less a rational order capable of mathematical formulation.

We may summarise these remarks by saying that the vastly improved technique of research has led to physical discoveries in recent years which have at last completely shattered the traditional commonsense view of the material world. A new space-time world has emerged which is essentially immaterial, and in which the old-time matter, and even the scientific mass, gravitation, and energy stand for no independent entities, but can best be construed as configurations of space-time. And the discovery of the quantic properties of this world points to still more radical transformations which loom on the horizon of science. The complete recasting of many of our categories of experience and thought may ultimately be involved.

From the brilliant discoveries of physical science we pass on to the advances in biological science which, although far less revolutionary, have been scarcely less important for our world-outlook. The most important biological discovery of the last century was the great fact of organic evolution; and for this fact the space-time concept has at last come to provide the necessary physical basis. It is unnecessary for my purpose to canvass the claims and discuss the views represented by the great names of Lamarck, Darwin and Mendel, beyond saying that they represent a progressive advance in biological discovery, the end of which has by

no means been reached yet. Whatever doubts and differences of opinion there may be about the methods, the mechanism, or the causes, there is no doubt about the reality of organic evolution, which is one of the most firmly established results in the whole range of science. Palæontology, embryology, comparative anatomy, taxonomy, and geographical distribution all combine to give the most convincing testimony that throughout the history of this earth life has advanced genetically from at most a few simple primitive forms to ever more numerous and highly specialised forms. Under the double influence of the internal genetic and the external environmental factors life has subtly adapted itself to the ever-changing situations on this planet. In the process of this evolution not only new structures and organs, but also new functions and powers have successively appeared, culminating in the master key of mind and in the crowning achievement of human personality. To have hammered the great truth of organic evolution into the consciousness of mankind is the undying achievement of Charles Darwin, by the side of which his discovery of natural selection as the method of evolution is of secondary importance.

The acceptance of the theory of evolution has brought about a far-reaching change in our outlook on the universe and our sense of values. The story of Creation, so intimately associated with the groundwork of most religions, has thus come to be rewritten. The unity and interconnections of life in all its manifold forms have been clearly recognised. And man himself has had to come down from his privileged position among the angels and take his proper place in the universe as part of the order of Nature. Thus Darwin completes the revolution begun by Copernicus.

Space-time finds its natural completion in organic evolution. For in organic evolution the time aspect of the world finds its most authentic expression. The world truly becomes process, where nothing ever remains the same or is a duplicate of anything else, but a growing, gathering, creative stream of unique events rolls forever forward.

But while we recognise this intimate connection between the conceptions of space-time and organic evolution, we should be careful not to identify the time of evolution with that of space-time. There is a very real difference between them. Biological time has direction, passes from the past to the future, and is therefore historical. It corresponds to the 'before' and 'after' of our conscious experience. Physical time as an aspect of space-time is neutral as regards direction. It is space-like, and may be plus or minus, but does not distinguish between past or future.

It may move in either direction, backwards or forwards, while biological time, like the time of experience, knows only a forward flow. Hence cosmic evolution, as we see it in astronomy and physics, is mostly in an opposite direction to that of organic evolution. While biological time on the whole shows a forward movement towards ever higher organisation and rising qualities throughout the geological ages, the process of the physical world is mostly in the opposite direction[1]—towards disorganisation, disintegration of more complex structures, and dissipation of energy. The second law of thermodynamics thus marks the direction of physical time. While the smaller world of life seems on the whole to be on the up-grade, the larger physical universe is on the down-grade. One may say that in the universe we witness a majority movement downward, and a minority movement upward. The energy which is being dissipated by the decay of physical structure is being partly taken up and organised into life structures—at any rate on this planet. Life and mind thus appear as products of the cosmic decline, and arise like the phœnix from the ashes of a universe radiating itself away. In them Nature seems to have discovered a secret which enables her to irradiate with imperishable glory the decay to which she seems physically doomed.

Another striking point arises here. Organic evolution describes the specific process of what we call life, perhaps the most mysterious phenomenon of this mysterious universe. When we ask what is the nature of life we are curiously reminded of the behaviour of the quantum referred to. I do not for a moment wish to say that the quantum is the physical basis of life, but I do say that in the quantum the physical world offers an analogy to life which is at least suggestive. The quantum follows the all-or-nothing law and behaves as an indivisible whole : so does life. A part of a quantum is not something less than a quantum ; it is nothing or sheer nonentity : the same holds true of life. The quantum is perhaps most easily symbolised as a wave or combination of waves, which can only exist as a complete periodicity, and whose very concept negatives its existence as partial or truncated. In other words, it is a specific configuration and can only exist as such : the same holds true of life. The quantum does not fall completely within the deterministic causal scheme : the same seems true of life. Significant, also, is the fact that quantum phenomena underlie

[1] No doubt there are exceptions to this broad generalisation. In astronomy stars and solar systems and galaxies are probably still being formed, while in physics syntheses of elements may possibly still be going on. In the same way we find in organic evolution minor phases of regression, degeneration and parasitism.

secondary qualities such as colour and the like, which the older science in its mechanistic scheme ignored, but which are specially associated with life and consciousness. Apparently the quantum does not fall completely within the causal deterministic scheme: the same is true of life. Life is not an entity, physical or other. It is a type of organisation; it is a specific principle of central or self organisation. If that organisation is interfered with we are left, not with bits of life, but with death. The nature of living things is determined, not by the nature of their parts, but by the nature or principle of their organisation. In short, the quantum and life seem to have this in common, that they both behave as wholes.

I have before now endeavoured to explore the concept of life in the light of the more general concept of the whole. A whole is not a sum of parts, or constituted by its parts. Its nature lies in its constitution more than in its parts. The part in the whole is no longer the same as the part in isolation. The interesting point is that while this concept of the whole applies to life, it is according to the recent physics no less applicable to the ultimate physical units. Thus the electron within an atom is no longer a distinct electron. There may be separate electrons, but when they cease to be separate they also cease to be. The eight electrons which circulate in an oxygen atom are merged in a whole in such a way that they have lost their separate identity; and this loss of individuality has to be taken into account in calculations as to the physical behaviour of the atom. The physicist, in fact, finds himself unable to look upon the entity which is one eighth of eight electrons as the same thing as a single electron. At the very foundation, therefore, of physics, the principle or category of the whole applies no less than in the advanced structure of life, although not in the same degree. In the ultimate analysis of the world, both at the physical and the biological level, the part or unit element somehow becomes shadowy and incoherent, and the very basis of mechanism is undermined. It would almost seem as if the world in its very essence is holistic, and as if the notion of individual parts is a practical makeshift without final validity in the nature of things.

The general trend of the recent advances in physics has thus been towards the recognition of the fundamental organic character of the material world. Physics and biology are beginning to look not so utterly unlike each other. Hitherto the great gulf in nature has lain between the material and the vital, between inorganic matter and life. This gulf is now in process of being bridged. The new physics, in dissolving the material

world of commonsense and discovering the finer structure of physical nature, has at the same time disclosed certain fundamental features which it has in common with the organic world. Stuff-like entities have disappeared and have been replaced by space-time configurations, whose very nature depends on their principle of organisation. And this principle, which I have ventured to call holism, appears to be at bottom identical with that which pervades the organic structures of the world of life. The quantum and space-time have brought physics closer to biology. As I have pointed out, the quantum anticipates some of the fundamental characters of life, while space-time forms the physical basis for organic evolution. Physics and biology are thus recognised as respectively simpler and more advanced forms of the same fundamental pattern in world-structure.

The older mechanistic conception of nature, the picture of nature as consisting of fixed material particles, mechanically interacting with each other—already rudely shaken by the relativity theory—is now being modified by the quantum physics. The attack on mechanism, thus coming from physical science itself, is therefore all the more deadly. Even in physics, organisation is becoming more important than the somewhat nebulous entities which enter into matter. Interaction is more and more recognised to be not so much mechanical as organic or holistic, the whole in some respects dominating not only the functioning but the very existence of the entities forming it. The emergence of this organic view of nature from the domain of physics itself is thus a matter of first-rate importance, and must have very far-reaching repercussions for our eventual world-view.

The nature of the organic whole is, however, much more clearly recognised in its proper sphere of biology, and especially in the rapidly advancing science of physiology. Here, too, the correct view has been much obscured by the invasion of mechanistic ideas from the physics of the nineteenth century. A crude materialism all but swamped biology for more than a generation. At the Belfast session of this Association in 1874 a famous predecessor of mine in this Chair gave unrestrained expression to this materialistic creed. All that is passing, if not already past. It must be admitted that up to a point mechanism has been useful as a first approximation and fruitful as a convention for research purposes. But if even in physics it has lost its savour, *a fortiori* has it become out of place in biology. The partial truth of mechanism is always subtended by the deeper truth of organicity or holism. So far from biology being

forced into a physical mould, the position will in future be reversed. Physics will look to biology and even to psychology for hints, clues, and suggestions. In biology and psychology it will see principles at work in their full maturity which can only be faintly and fitfully recognised in physics. In this way the exchanges of physics, biology and psychology will become fruitful for the science of the future, and lay the basis for a new scientific monism.

A living individual is a physiological whole, in which the parts or organs are but differentiations of this whole for purposes of greater efficiency, and remain in organic continuity throughout. They are parts *of* the individual, and not independent or self-contained units which *compose* the individual. It is only this conception of the individual as a dynamic organic whole which will make intelligible the extraordinary unity which characterises the multiplicity of functions in an organism, the mobile, ever-changing balance and interdependence of the numerous regulatory processes in it, as well as the operation of all the mechanisms by which organic evolution is brought about. This conception applies not only to individuals, but also to organic societies, such as a beehive or an ants' nest, and even to social organisations on the human level.

As the concept of space-time destroys the purely spatial character of things, so the concept of the organic whole must also be extended beyond the spatial limits of the organism so as to include its interaction with its environment. The stimuli and responses which render them mutually interdependent constitute them one whole which thus transcends purely spatial aspects. It is this overflow of organic wholes beyond their apparent spatial limits which binds all nature together and prevents it from being a mere assemblage of separate interacting units.

It is time, however, that we pass on to the world of mind. From matter, as now transformed by space-time and the quantum, we pass step by step through organic nature to conscious mind. Gone is the time when Descartes could divide the world into only two substances : extended substance or matter, and thinking substance or mind. There is a whole world of gradations between these two limits. On Descartes' false dichotomy the separate provinces of modern science and philosophy were demarcated. But it is as dead as the epicycles of Ptolemy, and ultimately the Cartesian frontiers between physics and philosophy must largely disappear, and philosophy once more become metaphysic in the original sense. In the meantime, under its harmful influence, the paths of matter and mind, of science and philosophy, were made to diverge farther and farther, so that

only the revolution now taking place in thought could bring them together again. I believe, however, their reunion is coming fast. We have seen matter and life indefinitely approaching each other in the ultimate constituents of the world. We have seen that matter is fundamentally a configuration or organisation of space-time; and we have seen that life is a principle of organisation whereby the space-time patterns are arranged into organic unities. The next step is to show that mind is an even more potent embodiment of the organising whole-making principle, and that this embodiment has found expression in a rising series, which begins practically on the lowest levels of life, and rises ultimately to the conscious mind which alone Descartes had in view in his classification. I have no time to follow up the matter here beyond making a few remarks.

Mind is admittedly an active, conative, organising principle. It is for ever busy constructing new patterns of things, thoughts or principles out of the material of its experience. Mind, even more than life, is a principle of whole-making. It differentiates, discriminates and selects from its vague experience, and fashions and correlates the resulting features into more or less stable, enduring wholes. Beginning as mere blind tropisms, reflexes and conditioned reflexes, mind in organic nature has advanced step by step in its creative march until in man it has become nature's supreme organ of understanding, endeavour and control—not merely a subjective human organ, but nature's own power of self-illumination and self-mastery: 'The eye with which the universe beholds itself and knows itself divine.'

The free creativeness of mind is possible because, as we have seen, the world ultimately consists, not of material stuff, but of patterns, of organisation, the evolution of which involves no absolute creation of an alien world of material from nothing. The purely structural character of reality thus helps to render possible and intelligible the free creativeness of life and mind, and accounts for the unlimited wealth of fresh patterns which mind freely creates on the basis of the existing physical patterns.

The highest reach of this creative process is seen in the realm of values, which is the product of the human mind. Great as is the physical universe which confronts us as a given fact, no less great is our reading and evaluation of it in the world of values, as seen in language, literature, culture, civilisation, society and the state, law, architecture, art, science, morals and religion. Without this revelation of inner meaning and significance the external physical universe would be but an immense empty shell or crumpled surface. The brute fact here receives its meaning, and a new

world arises which gives to nature whatever significance it has. As against the physical configurations of nature we see here the ideal patterns or wholes freely created by the human spirit as a home and an environment for itself.

Among the human values thus created science ranks with art and religion. In its selfless pursuit of truth, in its vision of order and beauty, it partakes of the quality of both. More and more it is beginning to make a profound æsthetic and religious appeal to thinking people. Indeed, it may fairly be said that science is perhaps the clearest revelation of God to our age. Science is at last coming into its own as one of the supreme goods of the human race.

While religion, art and science are still separate values, they may not always remain such. Indeed, one of the greatest tasks before the human race will be to link up science with ethical values, and thus to remove grave dangers threatening our future. A serious lag has already developed between our rapid scientific advance and our stationary ethical development, a lag which has already found expression in the greatest tragedy of history. Science must itself help to close this dangerous gap in our advance which threatens the disruption of our civilisation and the decay of our species. Its final and perhaps most difficult task may be found just here. Science may be destined to become the most effective drive towards ethical values, and in that way to render its most priceless human service. In saying this I am going beyond the scope of science as at present understood, but the conception of science itself is bound to be affected by its eventual integration with the other great values.

I have now finished my rapid and necessarily superficial survey of the more prominent recent tendencies in science, and I proceed to summarise the results and draw my conclusions, in so far as they bear on our world-picture.

In the first place we have seen that in the ultimate physical analysis science reaches a microscopic world of scientific entities, very different in character and behaviour from the macroscopic world of matter, space, and time. The world of atoms, electrons, protons, radiations, and quanta, does not seem to be in space-time, or to conform to natural law in the ordinary sense. The behaviour of these entities cannot be understood without the most abstruse mathematics, nor, apparently, without resort to epistemological considerations. We seem to have passed beyond the definitely physical world into a twilight where prophysics and metaphysics meet, where space-time does not exist, and where strictly causal law in the

old sense does not apply. From this uncertain nebulous underworld there seems to crystallise out, or literally to materialise, the macroscopic world which is the proper sphere of sensuous observation and of natural laws. The pre-material entities or units condense and cohere into constellations, which increase in size and structure until they reach the macroscopic stage of observation. As the macroscopic entities emerge, their space-time field and appropriate natural laws (mostly of a statistical character) emerge *pari passu*. We seem to pass from one level to another in the evolution of the universe, with different units, different behaviours, and calling for different concepts and laws. Similarly, we rise to new levels as later on we pass from the physical to the biological level, and again from the latter to the level of conscious mind. But—and this is the significant fact—all these levels are genetically related and form an evolutionary series; and underlying the differences of the successive levels, there remains a fundamental unity of plan or organisation which binds them together as members of a genetic series, as a growing, evolving, creative universe.

In the second place let us see how commonsense deals with this macroscopic world. On this stage commonsense recognises three levels of matter, life and mind as together composing the world. But it places them so far apart and makes them so inherently different from each other, that relations between them appear unintelligible, if not impossible. The commonsense notions of matter, life and mind make any relations between them, as well as the world which they form, an insoluble puzzle. The older science therefore attempted to reduce life substantially to terms of matter, and to put a question mark behind mind; and the result was a predominantly materialistic view of the world. The space-time relativity concept of the world has overcome the difficulty by destroying the old concept of matter, and reducing it from a self-subsistent entity to a configuration of space-time—in other words, to a special organisation of the basic world-structure. If matter is essentially immaterial structure or organisation, it cannot fundamentally be so different from organism or life, which is best envisaged as a principle of organisation; nor from mind, which is an active organiser. Matter, life, and mind thus translate roughly into organisation, organism, organiser. The all-or-none law of the quantum, which also applies to life and mind, is another indication that matter, life, and mind may be but different stages or levels of the same activity in the world which I have associated with the pervading feature of whole-making. Materialism has thus gone by the board, and the

unintelligible trinity of commonsense (matter, life, mind) has been reinterpreted and transformed and put on the way to a new monism.

In the third place, the iron determination of the older science, so contrary to direct human experience, so destructive of the free activity of life and mind, as well as subversive of the moral responsibility of the individual, has also been materially recast. It was due to the Newtonian causal scheme which, as I have indicated, has been profoundly shaken by recent developments. Relativity reduces substance to configuration or patterns, while quantum physics gives definite indications of indeterminism in nature. In any case, life through the ages shows clearly a creative advance to ever more complex organisation, and ever higher qualities, while mind is responsible for the creation of a whole realm of values. We are thus justified in stressing, along with natural necessity, an increasing measure of freedom and creativeness in the world, sufficient at least to account for organic evolution and for the appearance of moral law and endeavour. This liberation of life and spirit from the iron rule of necessity is one of the greatest gains from the recent scientific advances. Nature is not a closed physical circle, but has left the door open to the emergence of life and mind and the development of human personality. It has, in its open flexible physical patterns, laid the foundation and established the environment for the coming of life and mind. The view, to which Huxley once gave such eloquent and poignant expression, of a dualism implanted in the heart of nature, of a deadly struggle between cosmic law and moral law, is no longer justified by the subsequent advances of science.

But, in the fourth place, another dualism of a wider reach has appeared, which makes the universe itself appear to be a house divided against itself. For while the stream of physical tendency throughout the universe is on the whole downward, toward disintegration and dissipation, the organic movement, on this planet at least, is upward, and life structures are on the whole becoming more complex throughout the course of organic evolution. From the viewpoint of physics, life and mind are thus singular and exceptional phenomena, not in line with the movement of the universe as a whole. Recent astronomical theory has come to strengthen this view of life as an exceptional feature off the main track of the universe. For the origin of our planetary system is attributed to an unusual accident, and planets such as ours with a favourable environment for life are taken to be rare in the universe. Perhaps we may even say that at the present epoch there is no other globe where life is at the level manifested on the earth. Our origin is thus accidental, our position is exceptional, and our fate is

sealed, with the inevitable running down of the solar system. Life and mind, instead of being the natural flowering of the universe, are thus reduced to a very casual and inferior status in the cosmic order. A new meaning and a far deeper poignancy are given to Shakespeare's immortal lines:

> 'We are such stuff
> As dreams are made of; and our little life
> Is rounded with a sleep.'

According to astronomy, life is indeed a lonely and pathetic thing in this physical universe—a transient and embarrassed phantom in an alien, if not hostile, universe.

Such are some of the depressing speculations of recent astronomical theory. But in some respects they have already been discounted in the foregoing. For even if life be merely a terrestrial phenomenon, it is by no means in an alien environment if, as we have seen reason to think, this is an essentially organic universe. In its organic aspects the universe is on the way to life and mind, even if the goal has been actually reached at only one insignificant point in the universe. The potencies of the universe are fundamentally of the same order as its actualities. The universe might say in the words of Rabbi Ben Ezra :—

> 'All I could never be,
> All man ignored in me,
> This I was worth to God.'

Then again, the very possibility of perception, of knowledge and science depends on an intimate relation between mind and the physical universe. Only thus can the concepts of mind come to be a measure for the facts of the universe, and the laws of nature come to be revealed and interpreted by nature's own organ of the human mind. Besides science we have other forms of this inner relation between the mind and the universe, such as poetry, music, art and religion. The human spirit is not a pathetic wandering phantom of the universe, but is at home, and meets with spiritual hospitality and response everywhere. Our deepest thoughts and emotions and endeavours are but responses to stimuli which come to us, not from an alien, but from an essentially friendly and kindred universe. So far from the cosmic status of life and mind being degraded by the newer astronomy and physics, I would suggest an alternative interpretation of the facts, more in accord with the trend of evolutionary science. We have seen a macroscopic universe born or revealed to

consciousness out of a prior microscopic order of a very different character. Are we not, in the emergence of life and mind, witnessing the birth or revelation of a new world out of the macroscopic physical universe? I suggest that at the present cosmic epoch we are the spectators of what is perhaps the grandest event in the immeasurable history of our universe, and that we must interpret the present phase of the universe as a mother and child universe, still joined together by a placenta which science, in its divorce from the other great values, has hitherto failed to unravel.

Piecing together these clues and conclusions we arrive at a world-picture fuller of mystery than ever. In a way it is closer to commonsense and kinder to human nature than was the science of the nineteenth century. Materialism has practically disappeared, and the despotic rule of necessity has been greatly relaxed. In ever varying degree the universe is organic and holistic through and through. Not only organic concepts, but also, and even more so, psychological viewpoints are becoming necessary to elucidate the facts of science. And while the purely human concepts, such as emotion and value, purpose and will, do not apply in the natural sciences, they retain their unimpaired force in the human sciences. The ancient spiritual goods and heirlooms of our race need not be ruthlessly scrapped. The great values and ideals retain their unfading glory and derive new interest and force from a cosmic setting. But in other respects it is a strange new universe, impalpable, immaterial, consisting not of material or stuff, but of organisation, of patterns or wholes which are unceasingly being woven to more complex or to simpler designs. In the large it appears to be a decaying, simplifying universe which attained to its perfection of organisation in the far-distant past and is now regressing to simpler forms—perhaps for good, perhaps only to restart another cycle of organisation. But inside this cosmic process of decline we notice a smaller but far more significant movement—a streaming, protoplasmic tendency; an embryonic infant world emerging, throbbing with passionate life, and striving towards rational and spiritual self-realisation. We see the mysterious creative rise of the higher out of the lower, the more from the less, the picture within its framework, the spiritual kernel inside the phenomenal integuments of the universe. Instead of the animistic, or the mechanistic, or the mathematical universe, we see the genetic, organic, holistic universe, in which the decline of the earlier physical patterns provides the opportunity for the emergence of the more advanced vital and rational patterns.

In this holistic universe man is in very truth the offspring of the

stars. The world consists not only of electrons and radiations, but also of souls and aspirations. Beauty and holiness are as much aspects of nature as energy and entropy. Thus "in eternal lines to time it grows." An adequate world-view would find them all in their proper context in the framework of the whole. And evolution is perhaps the only way of approach to the framing of a consistent world-picture which would do justice to the immensity, the profundity, and the unutterable mystery of the universe.

Such in vague outline is the world-picture to which science seems to me to be pointing. We may not all agree with my rendering of it, which indeed does not claim to be more than a mere sketch. And even if it were generally accepted, we have still to bear in mind that the world-picture of to-morrow will in all probability be very different from any which could be sketched to-day.

THE GROWTH IN OPPORTUNITIES FOR EDUCATION AND RESEARCH IN PHYSICS DURING THE PAST FIFTY YEARS

ADDRESS BY

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PRESIDENT OF THE SECTION.

During the last year we have lost by the death of Prof. Albert Michelson a physicist whose work was of quite exceptional importance. The famous experiment known everywhere as the Michelson-Morley experiment has, since it is the basis of the Theory of Relativity, been largely responsible for the trend of physical thought during the present century. It is a very striking example of the great philosophical consequences which can result from what might seem the rather mechanical process of improving the precision of physical measurements; the importance of the experiment depended entirely on the accuracy of the measurements being great enough to detect with certainty changes amounting only to one part in a hundred million.

The additions to our knowledge of physical phenomena and the number of new ideas introduced into Physics since the last Anniversary Meeting have been so great and cover such a wide range that it would be impossible in the time at our disposal to give an account of them which would be at all adequate or even intelligible to those not already acquainted with them. There are, however, advances of another kind of great importance to the progress of Physics which lend themselves more readily to a less inadequate treatment in such an address as this. Such advances are the increase in the opportunities for teaching and research in Physics caused by the foundation of many new laboratories, the increase in the attention paid to the teaching of Physics in our schools, the endowment of research workers and the increase in the opportunities for these to obtain remunerative employment, the increased recognition of the importance of research in industry, and last but not least the improvements made in instruments used in research and the increase in the magnitude of the forces, mechanical, electric and magnetic, which are now at our disposal. The Physical Laboratories in the eighteenth century and the first half of the nineteenth were in the main collections of instruments suitable for experiments to illustrate the lectures of the Professor, and the trouble taken over these experiments was, I think, comparable with that taken now. Thus, Wollaston, who was Jacksonian Professor at Cambridge at the end of the eighteenth century, is said to

have shown over 300 experiments in his annual course of lectures, and at a later period Stokes, in his lectures on Optics, showed experiments which have not been excelled, for beauty, for educational value, for simplicity or for certainty.

In spite of the fact that until the end of the last century there were but few laboratories available for research much scientific work of the highest importance was accomplished. This, to a very great extent, was due to men who made their own laboratories and bore themselves the cost of the experiments. Thus, Joule made his experiments on the Mechanical Equivalent of Heat in his house at Manchester, Stokes like Newton made fundamental experiments on Optics in his College rooms, Spottiswoode, Huggins, De la Rue, Lord Rayleigh, and one who has had a long and intimate connection with the British Association—Dr. E. H. Griffiths—also made in their own laboratories and at their own charges additions of great value to Physical Science.

These men, like Kelvin and Maxwell, had not passed through any course of instruction in Practical Physics, for no such courses were available; they were in this respect self-taught. Most of them had learnt how to use their hands by having had when young some hobby, such as using a lathe, or dabbling in chemical experiments or photography. This training seems to have been effective for no one can say that their work is amateurish. This raises the important question, may not the present practice in which our advanced students spend a great deal of time in acquiring dexterity in the use of instruments of all kinds be a wasteful one, and could not the student who has learned how to use his hands, has a good knowledge of Physics and some practice in making accurate measurements be trusted to master in a short time the technique of any instrument he might require in a special investigation?

In the early 'seventies when I first began to study Physics at the Owens College, Manchester, there were only six Physical Laboratories in England—the Royal Institution, the Clarendon Laboratory at Oxford, those at University and King's Colleges and the one at the Royal School of Mines in London, and one at Owens College, Manchester; there were four in Scotland, one in Ireland and none in Wales. Now the number of Physical Laboratories at the Universities, University Colleges, Schools, and Institutes of Technology at which instruction is given in Physics, is considerably more than 300. Nearly the whole of this increase has occurred since our last Anniversary Meeting. The contrast between then and now would be even more marked if we took into account the size of the buildings. Some of the laboratories in those early days were very small affairs. Prof. Ayrton described Sir William Thomson's laboratory at Glasgow as consisting at one time of one room and an adjacent coal cellar. When I was at the Owens College, Manchester, though it was one of the first places where instruction was given in Practical Physics, there was no separate laboratory but only a few rooms and little apparatus. Though the laboratory was small, it was large enough for the few students who worked there, and these had much more freedom and more initiative than would be possible with the large number of students that have now to be provided for. We were allowed to choose our own experiments, we fitted up the experiments for ourselves and if they did

not work we tried again, we were not limited with respect to time and we could follow up any point of interest we happened to come across. This rather happy-go-lucky method would be quite impossible with large classes, but it was more interesting and, I think, a better training for research than the highly organised classes which large numbers necessitate. At any rate, I am glad that I came under the old and not under the new system. The new method, however, besides being inevitable has some very decided advantages. There are students who are quite immune when in the lecture room to infection from any physical idea, and only get a grip of physical principles when these come before them in the concrete form of an experiment which they make with their own hands; these men learn their theoretical as well as their practical physics in the laboratory, and the nature of the experiments, their number and their sequence are of first-rate importance; all this requires a great deal of organisation.

Science Teaching in Schools and Universities.

The movement for including Science among the studies pursued in our Universities and Schools was born almost at the same time as the British Association and the men who took the most prominent part in it, Adam Sedgwick, Herschel and, above all, Whewell, were closely connected with the Association. It was some time before the movement led to definite results, but in 1849 the University of Cambridge determined to establish a Natural Sciences Tripos. It was not at first an avenue to a degree, and the subjects were limited to those in which there were Professorships in the University, viz., Chemistry, Mineralogy, Human Anatomy and Physiology, Botany and Geology. The first examination was held in 1851. There were only six candidates, four were placed in the First Class with Prof. Liveing at the top.

In 1853 the report of the Commissioners appointed to report on the studies at Oxford and Cambridge appeared and contained recommendations in favour of further opportunities for the study of Science at the Universities. In 1864 the Royal Commission appointed to report on the seven most important Public Schools recommended that all the boys should receive instruction in Science during part at least of their school career. This was not before such a recommendation was needed. For Science in schools at that time has been described as being 'regarded with jealousy by the staff, with contempt by the boys and with indifference by the parents.'

In 1866 there was a valuable report on the teaching of Science in Schools by a Committee of the British Association. In 1867 a Commission appointed to consider the education given in those schools not included in the reference to the first Commission reported in favour of including Science in the curricula of these schools. It should be mentioned that before the publication of these reports, J. M. Wilson, who died only a short time ago, had started Science teaching at Rugby where he was a master.

The fight for the introduction of Science teaching in our Universities and Schools was long, and at times bitter; for some time little progress seemed to be made, but in 1881, the period of our last Jubilee, things

began to go with a rush. Owens College obtained, in 1880, a charter as Victoria University and could give degrees to its students, and in the early 'eighties Mason's College, Birmingham, with Poynting as Professor of Physics, University College, Liverpool, with Lodge as Professor, Yorkshire College, Leeds, with Rücker as Professor, came into existence and later became Universities.

The number of schools where Science was taught rapidly increased, and as a result of this so did the number of Science students coming to the Universities. Very clear evidence of this is the fact that in 1881, thirty years after the foundation of the Cambridge Natural Science Tripos, the number of candidates had only risen to twenty-five, while in 1891 the number was ninety-four, practically as great as any other Tripos in the University.

It can now, I think, be claimed that some Science is taught in all schools, and a good deal in a great many ; this is a great advance and has practically all been made in the last fifty years. It cannot, however, be said that even now Science occupies in our systems of education a place commensurate with its ever-increasing influence on human thought and with its importance in the progress of civilisation.

One defect of the present system is that the Entrance Scholarships offered by most of the great Public Schools have in practice the effect of attracting the abler boys to Classics. In the examination for most of these Scholarships much greater weight is given to Classics than to any other subject, and a boy must have spent most of his time on Classics if he is to do well in the examination. Thus, when he goes to the School he is much further advanced in Classics than in anything else and, naturally, takes it as his main subject. It may not, however, be the subject in which his strength really lies. For unlike Mathematics, in which marked proficiency is only attained by boys with a somewhat rare type of mind, in Classics most able boys can under skilful teaching acquire sufficient proficiency to give them a fair chance of getting an Entrance Scholarship at a Public School. These Scholarships may thus entice them along a path which does not lead to their true destination. That this actually occurs is, I think, shown by the figures given in the Report of the Committee on the Position of Natural Science in the Educational System of Great Britain, 1918. Of the Entrance Scholarships to Cambridge gained by boys from seven great Public Schools which give Entrance Scholarships, for one gained in Science, six were gained in Classics. This disproportion is far greater than the average for all Schools, showing that it is not due to the rarity of scientific talent as compared with classical, but is an artificial one due to the systems in force at these Schools.

The last thing I wish to do is to disparage Classical Studies. I think that for some boys a course in which Classics predominates is the best, and I think that in the early stages of the education of all boys Classics should play a large, perhaps even the largest, part. What I think is desirable is that the School Examination should not be so specialised as it is now, and that the papers in Classics should not be so much more advanced than those in any other subject.

It is not enough to have introduced Physics into Schools, it is necessary to develop methods of teaching which will make its study produce its full

educational effect. The problem is a difficult one, the teaching of Classics and Mathematics have long experience and tradition behind them. No such tradition exists for that of Physics. The methods have had to be evolved and it cannot be said there is yet anything like complete agreement as to which are the best. Science Masters are attacking the problem with the greatest vigour and enthusiasm, are trying out one method after another. I think there is perhaps too great a tendency to concentrate on the method to the exclusion of the personality of the teacher, a good teacher will soon find the method which in his hands gives the best results and will do better with that than with one imposed on him from outside.

Post-Graduate Study and Research.

Research is now an integral part of the training of a considerable number of our students and the importance of research for the welfare of the nation universally recognised. This, however, is quite a modern development. It had hardly started sixty years ago, and though a vigorous propaganda for the 'Endowment of Research' was being carried on by Mark Pattison, Huxley, Roscoe, Lockyer, and others, it was some time before it began to produce much effect.

Besides the apathy of the country there were at that time three great obstacles to research :—

1. The lack of Laboratories. We have already seen how this has been remedied.
2. The lack of Scholarships to enable men to stay up at the University to research after taking their degree.
3. A third obstacle was that there was hardly any chance of obtaining a livelihood by research alone, so that the only men who made it a career were those who had money or were so enthusiastic that they were reckless about monetary affairs. The case is very different now when research is a recognised profession and a fairly lucrative one.

It is not a great exaggeration to say that in those early days there was neither room, money nor a career for those who wished to research.

Things, however, soon began to mend and research gradually came to be regarded as a suitable subject for the award of Scholarships and Fellowships. I am glad to say that one of the first, if not the first, to take action in this matter was Trinity College, Cambridge, who in 1874 determined to take into account for election to Fellowship any original work which the candidates might submit. Before this the elections had been determined solely on the results of an examination held by the College. They carried out the scheme in no half-hearted way, for at the first election under the scheme in 1874 they elected Francis Balfour, the great zoologist, though it was an open secret that if the examination alone had been taken into consideration another candidate would have been elected. The scheme has been remarkably successful. Many papers of absolute first-rate importance have been submitted by the candidates and now the College has abandoned the examination altogether and only takes into account the original work submitted by the candidates.

In addition to awards for the results of successful research, Scholarships began to be founded to enable students who had just taken their degrees to get a post-graduate training in research. The rate of increase in the number of these was very slow in the last century, but this century it has got faster and faster and now grants for training in research are given by most of the Colleges, by some of the great City Companies, who have done so much for the promotion of Science and Education, by bodies like the Commissioners for the 1851 Exhibition, and, above all, by the Department of Scientific and Industrial Research, who have in the last ten years made grants to students in training of £228,970, the average number receiving grants in each year being 184.

In Cambridge the number of students doing post-graduate research increased rapidly after 1895 when a regulation came into force which enabled students who had graduated at other Universities to obtain a Cambridge degree after two years' satisfactory research work in Cambridge. This degree was at first the B.A. degree, but in 1920 a new degree, the Ph.D., was instituted by the University for which Cambridge men as well as graduates of other Universities are eligible. There are now forty-five of these students in residence taking Physics. Of these, by far the greater number hold Scholarships or are in receipt of grants. Indeed, I think it can now be said that a really first-class man has an excellent chance of getting, if he is in need of it, sufficient assistance to enable him to get a training in research.

The problem of training a large number of students in research in Physics is by no means an easy one, many things have to be taken into consideration and provided for, otherwise the post-graduate course may do more harm than good.

It is very necessary to remember that the importance of the research work done by these students lies not so much in the scientific results obtained as in the training it affords. On this point I should like to read an extract from the report of a Commission on the place of Science in Education, of which I was Chairman :—

'The training afforded by the study of Natural Science will be incomplete unless the student undertakes some piece of research in which, relying as far as possible on his own resources, he applies his knowledge of Science and of the methods of scientific investigation to the solution of some scientific problem. The effect of a year's work of this kind on the general mental development of the student is most striking. He gains independence of thought, maturity of judgment, self-reliance, his critical powers are strengthened, and his enthusiasm for science increased, in fine he is carried from mental adolescence to manhood. We think that whenever possible a year spent mainly on research should form part of the course at the University of those whose work in life will be concerned with the industrial applications of science as well as those who will devote themselves to research and teaching. It is important, however, that at this stage the teachers at the University should regard research mainly from the point of view of its value as an educational training and not as a means of getting within the year as many new scientific results as possible. The student should be encouraged to overcome his difficulties by his own efforts and the assistance given by the teacher should not be more than is

necessary to keep him from being disheartened by failure and to prevent the work from getting on lines which cannot lead to success.'

I should like to emphasise the last part of this quotation. A year or two spent on research under proper conditions is an educational training which cannot easily be overrated, but under others it may be positively harmful. It must always be borne in mind that the primary object of a University laboratory is not the same as that of a laboratory where there are no students in training, such as the National Physical Laboratory or the laboratory of a great firm. In such laboratories the main object is to get results, to discover as many new facts as possible. In a University laboratory the most important thing is to produce well-trained and well-educated men rather than to turn out the largest number of small papers. To get scientific results rapidly, the best plan is for the staff to select the subject for investigation, to determine the kind of experiment to be made, to exercise daily supervision over the work and to leave to the student little besides the taking of the observations. The intellectual development of the student is injured rather than benefited by a training like this. You cannot without disaster apply methods of mass production to education. Even in University laboratories where the importance of affording mental training is fully realised, over-specialisation is the great danger of these courses of research, and one that requires much care to avoid. The student gets so engrossed in his experiments that he grudges the time spent on going to lectures, or on reading books which are not on his own special subjects. He often spends too much time in making the experiments and too little in thinking about them. Sometimes, too, he neglects to take advantage of the opportunities afforded by a resident University for social intercourse with men of all shades of opinion and of experience. There is danger, too, of his getting into a groove and to go on working for the rest of his life on the particular subject on which he was first engaged.

I think it helps one to get new ideas if the mind does not dwell too long on one subject without interruption, and if every now and then the thread of one's thought is broken. It is, I think, a general experience that new ideas about a subject come when one is not thinking about it. I am not a psychologist and do not know the views held as to how new ideas originate, but to my mind there is considerable practical analogy between this process and one about which we have been hearing a good deal during the last few days, the induction of currents in a magnetic field. For this to occur change as well as the magnetic field is necessary. If a circuit is in such a field nothing happens as long as it is in repose, but if you disturb this repose, currents begin to flow through it. Now compare the circuit to the brain, the magnetic field to the state produced in the brain by long thought about a subject, the starting of a current to the starting of an idea. No ideas will come as long as the brain remains in the same condition without any change in its point of view, but if this changes, then currents or ideas are produced in the brain, the change as it were strikes sparks in the brain. This is one reason why I think it is desirable that the student should do a little teaching, another is that it would give him experience which may be valuable in after-life and help him to obtain a post.

Careers for Research Workers.

I now come to the subject of the careers open to men who have had a training in research. Sixty years ago the only posts open to these were teaching posts in the few physical laboratories then in existence. The number of such posts increased very rapidly towards the end of the last century, as did also the demand for science masters for the schools; but until then and indeed for some time after it may be said that roughly speaking the only posts open to research workers were posts associated with teaching. At the beginning of this century, however, the importance of research to our industries began to be realised. The most striking instance of this is the establishment, in 1901, of the National Physical Laboratory for research both in pure science and in subjects which have an immediate application to industry. The growth of this under Sir Richard Glazebrook and Sir Joseph Petavel has been phenomenal, there are now about 160 research workers employed in the laboratory and the Budget has increased twenty-fold. Other methods of linking up Science with industry are also being employed in this country. Probably the most efficient is for a firm to have its own laboratory where its own problems can be investigated, in this case the inducements for success are greatest and knowledge of the technique and processes involved most accessible.

There are several such laboratories, each with a large staff in this country. They are, however, so expensive as to be beyond the reach of any but very large firms. To extend the benefit of research to the industries generally, the Department of Scientific and Industrial Research was started by the Government in 1915. At the instigation and with the aid of grants from the Department, the members of various industries have combined and formed Research Associations with laboratories suitably equipped for research in matters relating to the particular industry. There are now more than twenty of these Associations. They have had to contend with many difficulties, at first there was plenty of money but no well-trained men, now there are plenty of men but no money. There are, however, good reasons for thinking that in spite of these difficulties the financial gain to the industries has far exceeded the expenses of the laboratories. In addition to granting aid to these Associations, the Department has established Boards for research in matters which concern all industries. There is the Fuel Research Board which deals with problems vital to the country on the production of power from coal and other fuels, there is a Food Research Board for research on the storage and transport of food, there are Boards for Building, Forest Products, Radio and Chemical Research. All these have laboratories and staffs of research workers, as have also the Research Departments of the Army, Navy and Air Services.

I have tried to find how many workers are employed in these applications of Science to industry, but have not been able to get any estimate which would be of any value; one great difficulty is to draw the line between posts which seem adequate for those who have gone through a long and expensive training in research and those which do not. One thing, however, can be said, that the demand we have had in Cambridge for workers trained in research has, until this year of acute and long-continued depression, exceeded the supply; and although it is possible

to have over-production in research workers, we do not at present seem to have reached that stage for normal times.

In considering research as a profession, it must be remembered that especially in research of a pioneering kind the worker may spend years without getting results of any very striking importance. He may get depressed, lose hope, and be inclined if he gets the chance to go into administration and organisation where there is a greater certainty of work yielding an adequate result. The researcher, if he is to have a happy life, must regard the game and not the score as the chief thing. In every research difficulties and apparently anomalous results are constantly turning up. To overcome these, to make clear and consistent what before was obscure and confused, is to some minds one of the keenest of pleasures and one which may be produced by discovering the source of a persistent leak in a discharge tube just as well as by finding a new ray. Experience shows that men with minds of this type are not very common. There are many who when they are young and just fresh from a laboratory, where there is an atmosphere of research and many research workers, are so enthusiastic about research that they think nothing else matters. Often, however, this enthusiasm soon fades and they become more interested in organisation and administration than research. Thus, those who begin by working in the Research Department of a firm tend to drift into the other departments. I think this, on the whole, is an advantage, for it diffuses the scientific spirit and outlook throughout the work of the firm, this may be as important as discoveries in the laboratory and quicker in its effects.

The increase in the number of research workers has naturally led to a corresponding increase in the number of papers on physics. From one point of view this is very gratifying, from another it is embarrassing. 'Science Abstracts' for 1930 contains abstracts of 4,165 papers on physics, corresponding to very nearly a dozen a day. It is obvious that no one can read more than a small fraction of these. It is generally more than one can do to read even those in a particular branch of physics, this leads to great specialisation. Volumes such as those of 'Science Abstracts,' which give the gist of a paper in a small space, are of great value, especially for looking up the literature of a subject over a definite period. For this purpose, however, the subject index is of vital importance, in making this index it is not enough to go by the title of a paper, the contents of a paper cannot all be got into its title. The makers of the index should have read the papers. This seems a council of perfection, but it would practically be secured if the maker of the abstract were to send in with it cards for the subject index. This is work that requires great care and sound judgment.

I do not think that abstracts alone are sufficient to cope with this avalanche of papers on physics. As far as I know we have nothing in physics corresponding to the annual reports issued by the Chemical Society on the progress of various branches of chemistry. I think it would be a very good thing if we had, and that it is a thing on which money and time might well be spent. In addition to these, there should, I think, be fuller and more critical reports issued regularly at a longer period, say quinquennially, of the character of those which from time to time have

been published by the British Association and by the National Academy of Washington. Another minor suggestion is the publication each month of the titles (without any abstracts) of the physical papers published in scientific periodicals and the Proceedings of Scientific Societies during the preceding month. This used to be a feature of Wiedemann's Beïblätter and I found it very useful.

In addition to the advances we have been considering, the instruments and appliances in laboratories are very much better and more convenient than they used to be. The most vivid impression I have of my early work in the laboratory is that of Groves Cells, these had platinum foil immersed in nitric acid for one electrode, zinc in dilute sulphuric for the other, and what with the fumes which assailed one's throat and the acid which destroyed one's clothes, the assemblage of a battery of cells was a most disagreeable business. I have not seen a Groves Cell for forty years and do not want to see another. Now instead of making up a battery we just put a plug into a hole. Another instrument which was exasperating to work with was the old quadrant electrometer, this not infrequently refused to hold its charge and neither prayers nor imprecations would induce it to do so, it has, fortunately, been replaced by more sensitive and convenient instruments. With regard to galvanometers, I have the authority of Mr. Whipple in saying that one suitably selected for the purpose for which it is required may be at least ten thousand times more efficient than the instruments available fifty or sixty years ago. The extensive use of electrical instruments in connection with electrical lighting and engineering has caused a great deal of attention to be paid to their design with the result that they are far more convenient and reliable than they used to be. The improvement of instruments is of first rate importance for the progress of Physics, a considerable increase in the efficiency of an instrument may open up a new region of physical phenomena. The most striking example of this is the effect produced by improvements in the methods of producing high vacua. Roughly speaking, we may say that modern physics depends on our power of studying individual atoms and electrons and not merely large crowds of these particles. To do this, one atom must not be hit by another while under observation, as it would make more than ten thousand collisions in a centimetre if the pressure were atmospheric; a very high vacuum is required. Until early in this century this had to be got by Sprengel pumps, which involved one raising and lowering a vessel filled with mercury for hours on end and getting what would now be considered a very poor result, but a vivid appreciation of the intensity with which Nature abhors a vacuum. All this was changed after Sir James Dewar introduced the method of producing high vacua by means of charcoal cooled by liquid air. This was not only much more rapid and convenient but produced very much higher vacua and made it possible to make experiments and measurements which could not have been made before the introduction of this method and which have revolutionised our ideas of the structure of matter. If Science helps the industries they in return help Science. An illustration is that the need of a high vacuum for hot wire valves and electric lamps made its production a matter of commercial importance with the result that the physicist has now at his

command pumps so powerful that they can maintain an exceedingly high vacuum in spite of the influx into the vessel of a stream of the particles we wish to study; this is exceedingly important when investigating charged atoms and electrons.

The hot wire valve is another instrument which has helped greatly research in physics; the immense magnification of weak effects which can be produced by it enables us to detect with certainty phenomena which before its introduction were almost beyond our ken. Those who like myself repeated, more than forty years ago, Hertz's experiments will contrast the difficulty we had in detecting electrical waves even when the source was only a few yards away with the ease with which modern methods using hot wire valves detect waves which have travelled thousands of miles.

I have alluded to advances in the efficiency of the instruments. There is another advance in them which is not so gratifying, that is the advance in price. The cost of research in physics is much greater than it used to be. Before the war when about thirty research students were working at the Cavendish Laboratory, the cost of their researches was about £300 per annum, now it would be at least five times that amount. To balance this there are now far greater sums available for research than there were in those days.

I have in this address confined myself to what may be called the machinery of research in physics. I will now say a word or two about another point. The additions to our knowledge of physical phenomena and to physical conceptions made in the last sixty years have not been excelled by those made in any period of the history of the science, and yet I remember that at the beginning of this period the view was prevalent that all the fundamental principles of physics had been discovered and that the work of the future would be to develop and co-ordinate those principles and to measure more and more accurately the value of known physical constants. This view seems ludicrous when we know that within a few years Röntgen rays, the electron and radio-activity were discovered. The existence of these was quite unexpected, and no hint of the possibility of their existence was given by any of the physical theories then extant; this view was, however, to my knowledge, held by some eminent physicists. The great generalisations expressed by the first and second Laws of Thermodynamics loomed so large in those days that it was thought that nothing was beyond their purview. This state of mind is apt to occur after a great discovery; it occurred after that of universal gravitation; there are signs that it exists now. Yet it has always been falsified by experience, and I think always will be. There are no signs that physics is approaching an asymptotic state in which the progress gets slower and slower as time goes on. The additions to our knowledge of physics made by our generation do not get smaller and smaller as one generation succeeds another, each great discovery is not a terminus but an avenue leading to new knowledge. An improvement in technique may, as we have seen, lead to fundamental changes in our views of the nature of matter and of physical processes. There is far more in physics than is dreamt of in our theories; and Nature herself, if we observe her carefully, is more suggestive of ideas than the

minds of the most imaginative of us. The ideas which revolutionise Science are just those of which our theories give no indications. Theories are the very life-blood of Physics, most of the researches in our laboratories originate in an attempt to test a theory; theory, however, may be injurious if it makes us concentrate our attention exclusively on the particular problem it suggested, and to treat as an annoyance, to be avoided by a change in method, any anomaly in the experiment which interferes with our progress to the goal; the anomaly may be the outcrop of a vein rich in new phenomena. After Röntgen had discovered X-rays, another physicist who had been working with somewhat similar apparatus said that he had noticed that any photographic plates near his tube got fogged and spoiled; he moved his plates further away and left it at that. The discovery of argon by Lord Rayleigh arose from some vexatious discrepancies in a series of weighings.

I do not think that there is any danger of the supply of new physical phenomena being exhausted and of Physicists joining the ranks of the unemployed. Rather do I believe that as each successive Centenary comes round the President of Section A will be able to say that the growth of Physics in the century which has just passed is comparable with that in any of its predecessors.

SECTION B.—CHEMISTRY.

MICHAEL FARADAY AND THE THEORY OF ELECTROLYTIC CONDUCTION.

ADDRESS BY

SIR HAROLD HARTLEY, C.B.E., M.C., F.R.S.,

PRESIDENT OF THE SECTION.

When you did me the honour of inviting me to preside over your Section at the hundredth meeting of the Association, it seemed to me almost inevitable that my address to you should be a retrospect, recalling to you some of the great achievements of chemists since 1831. This week our guests from abroad have joined us in celebrating two centenaries, and I trust you will not find it inappropriate that my address should be a tribute to the memory of Michael Faraday and a sketch of the development through the century of the work which was his classic contribution to chemical science. It would be invidious to try to make any distinction between chemistry and physics, and Faraday himself would be no party to such a division. 'Such a difference,' he said, 'is a mere play upon words, and shows ignorance rather than understanding'—and indeed he is the outstanding example of the essential unity of the two subjects. But we cannot forget to-day that Faraday was in a sense the discovery of a chemist, that he was trained in a chemical laboratory, that his early triumphs were in the field of chemistry, and that he was one of the great masters of chemical technique.

He presided over this Section in 1837 at Liverpool and again in 1846 at Southampton. There is, alas, no record of his addresses, but of the Liverpool meeting he wrote: 'To-day I think we made our Section rather more interesting that was expected, and to-morrow I expect will be good also'—and with Faraday in the Chair, no doubt it was.

I feel some hesitation in speaking to you of Faraday. It is almost impossible to say anything new of him or anything adequate to his great genius. To try and explain Faraday seems an impertinence, but my tribute to his memory, however inadequate, must be to tell again the story of the early years of his apprenticeship to chemistry, and to trace the steps which led him to the researches which still remain the foundation of electrochemistry. There is no time to speak of his early life. The first intimate glimpse we have of his amazing natural gifts and his love of science, is as a bookbinder's apprentice with only the rudiments of education, when at the age of nineteen he is writing long letters to Abbott describing his experiments in electricity and arguing convincingly about

the true nature of chlorine. Then came the happy accident in 1813 that took him to the Royal Institution at the age of twenty-two to act as assistant to Davy, who was then at the height of his powers. Faraday was always mindful of the debt he owed to Davy, and doubtless he learned much from his skill and experience and from watching his decisive experimental methods. We can picture them working alongside one another in 1815 during those fourteen crowded days which elapsed between the arrival of samples of fire damp from the Northumberland mines and the discovery of the principle of the Davy Lamp—'the result of pure experimental deduction—it originated in no accident.' Faraday soon began to carry out investigations himself, and from 1816 a constant stream of papers appears under his name. They form no connected series, and no general idea underlies them. Some come from suggestions from Davy, others arise from casual observations in the laboratory or from some new materials to be investigated, and later on there are investigations arising out of some practical need, such as those on optical glass and on alloys of iron when he attempted to produce rustless steel. The titles of some of the papers exhibit Faraday's wide range of interests and experience :—

On the Escape of Gases through Capillary Tubes (1817), in which he appears as the forerunner of Graham ; On the Solution of Silver Compounds in Ammonia (1818) ; Combinations of Ammonia with Chlorides (1818) ; On two new Compounds of Chlorine and Carbon (1820), in which he isolates hexachlorethane and tetrachlorethylene ; On new Compounds of Carbon and Hydrogen (1825), in which he isolates benzene and butylene ; On the Condensation of Several Gases into Liquids (1823) ; On the Mutual Action of Sulphuric Acid and Naphthaline (1826), in which he prepares and separates the barium salts of the α and β naphthalenesulphonic acids by means of their different solubilities. These investigations show Faraday's capacity as a practical chemist, the neatness and simplicity of his experimental methods, the quickness and accuracy of his observation, and the completeness with which he treated a subject. It was Faraday's good fortune that so important a substance as benzene was in the gas oil given to him by Gordon to investigate, but the remarkable part of the work is his separation of benzene by fractional distillation and crystallisation, and the accuracy both of his analysis made with the simplest means and of his vapour density determinations made by exploding a known volume with oxygen and measuring the contraction and the volume of carbon dioxide formed. For its date it is a little masterpiece of investigation, although a modern examiner might quarrel with results given to six places of decimals. Unlike his later work, these papers are mainly records of experiments ; they contain few references to theory, and owing to Faraday's scepticism as regards the atomic theory he seems to have taken little interest in the problem of the atomic constitution of different substances, which was just beginning to perplex chemists. New substances were discovered, purified, analysed and described and left almost without speculation as to their nature. Naphthalenesulphonic acid is called 'sulpho-naphthalic acid, which sufficiently indicates its source and nature without the inconvenience of involving theoretical views.'

But in those years Faraday was gaining that first-hand acquaintance with the properties of many substances, which was to be invaluable to

him in choosing the right materials for his experiments. He was also accumulating an unrivalled knowledge of chemical technique and gaining the confidence to which was due the boldness and directness of his experiments in the years to come. How many of you, I wonder, have read his Chemical Manipulation, published in 1827, in which he describes every kind of laboratory operation and device. The art of experimenting must almost necessarily be traditional, and I remember well how this was brought home to me by my tutor, Sir John Conroy, when he saw me committing one of the minor crimes of the laboratory. He looked rather sadly at me, and all he said was 'Harcourt would have told Dixon, Dixon would have told Baker, and Baker would have told you.' Faraday, however, wanted to make chemical manipulation less of an alchemical secret, 'taught only in the very depths of the laboratory to a highly privileged few.' The book is one of the most personal documents in scientific literature as each page is a record of his own experimental methods, showing how every detail of each operation had been thought out by him and reduced to its simplest and most effective form.

To take one example—nowhere else will you find the use of the mortar analysed in such a scientific manner as in Faraday's chapter on Comminution. The nature of the substance to be powdered, the material of the mortar, the method of holding it to secure the quickest result with the minimum of fatigue, are all subjected to the most searching examination, and every page makes one realise the concentration of effort and thought that underlay all Faraday's experiments.

There is an intensely personal quality about Faraday's work as it was all done with his own hands, and even if he used the result of others he repeated their experiments. 'I was never able to make a fact my own without seeing it. . . . If Grove, or Wheatstone, or Gassiot told me a new fact and wanted my opinion . . . I could never say anything until I had seen the fact. For the same reason, I never could work, as some professors do most extensively, by students or pupils. All the work had to be my own.' Faraday worked alone to the end of his life with no helper except the trusty Sergeant Anderson, who for almost forty years was his laboratory assistant. 'He and I are companions, in years, in work and in the Royal Institution.' Anderson deserves a place in our chemical hagiology beside Berzelius' faithful cook, Anna, whose conservatism as regards the nature of chlorine was even greater than her master's.

Throughout his life, Faraday had an intense interest in the applications of science to everyday problems, often making them the subjects of his Friday evening discourses at the Royal Institution. We are apt to forget that he was a skilful analyst and that for several years he made a considerable income as a consulting chemist by what he called his 'professional business,' until in 1831 he deliberately gave up this work lest it should interfere with his researches. But Government Departments were constantly seeking his help and advice, and for thirty years he was Scientific Adviser to Trinity House, where he gave his time unsparingly to such problems as lighting and ventilation, and even to the examination of water supplies and of samples of oils and paints. Quite late in life he had not lost his cunning as an analyst. In 1845 he was reporting to Trinity House on the adulteration of white lead, and in 1852 he made an

analysis for the Board of Ordnance in two days of the contents of a French shell fired at Salee.

In his description of the requisites of a laboratory, Faraday wrote, 'A blank writing-paper book should be upon the table, with pen and ink, to enter immediately the notes of experiments. A chair may be admitted, and one will be found quite sufficient for all necessary purposes, for a laboratory is no place for persons who are not engaged in the operations going on there. . . . The practice of delaying to note until the end of a train of experiments or to the conclusion of a day, is a bad one, as it then becomes difficult accurately to remember the succession of events. There is a probability also that some important point which may suggest itself during the writing, cannot then be ascertained by reference to experiment, because of its occurrence to the mind at too late a period.'

Faraday's own note-books are much more than a record of his experiments, as he jotted down in them in numbered paragraphs, which ran to 16,041, the ideas which flashed on him as he was working in the laboratory and his plans for new experiments, so that we can follow his progress from day to day and watch the interplay of ideas and experiments, and the swiftness and certainty with which he reached a decision. When a young man asked him the secret of his success as an investigator, Faraday answered, 'The secret is comprised in three words—Work, Finish, Publish.' What he meant by this we can see by following in his note-books the course of his researches in electrochemistry from 1831 to 1834.

It is easy to see why Faraday had to work alone with nobody to distract him. In the period of his great achievements, his experiments were rarely continuous, the intervals between them suggesting the subconscious working of his mind. He waited until the impulse came and his 'prescient wisdom' had planned the experiment and foreseen the result. As we read the pages of the note-books, discovery seems to follow discovery almost inevitably. Faraday always had a preconceived idea behind his experiments, and never were advances made with such economy of effort. Each new position was reached by a series of attacks delivered with amazing speed when everything was ripe for them. The eager intensity with which Faraday worked in the laboratory impressed all those who watched him—'His motions were wonderfully rapid; and if he had to cross the laboratory for anything, he did not walk at an ordinary step, he ran for it, and when he wanted anything he spoke quickly.' . . . . 'The rare ingenuity of his mind was ably seconded by his manipulative skill, while the quickness of his perceptions was equalled by the calm rapidity of his movements.'

The year 1831 was the turning-point of Faraday's career. There is no greater contrast in scientific literature than his earlier chemical papers, characterised by their essentially practical outlook and accomplishment and the brilliant flights of imagination which inspired his 'Experimental Researches in Electricity.'

What was it that brought about this transformation? It has been said that Faraday's powers were maturing gradually in readiness for that great outburst of intellectual activity in his fortieth year, but I believe that the change was due simply to the success of an experiment which Faraday had previously tried again and again without result. It was the

discovery of electromagnetic induction that gave the new impulse to his mind and gave him confidence in the promptings of his imagination. What are the facts ?

Electricity was one of Faraday's earliest scientific interests. Long before he went to Davy he was experimenting with home-made batteries. Already in 1816 we get a glimpse of his intuitive belief in the essential unity of the forces of nature, which was to influence so greatly the current of his researches. His first lecture to the City Philosophical Society was on the general properties of matter, and we find him speculating on the forces underlying material behaviour and on their inter-relation. 'That the attraction of aggregation and chemical affinity is actually the same as the attraction of gravitation and electrical attraction, I will not positively affirm, but I believe they are.' In 1821, Faraday repeated the experiments of Oersted, Arago and Ampère on electro-magnetism and discovered the rotation of a wire carrying a current if free to move round a magnetic pole. Magnetism had been produced from electricity, and Faraday was convinced of the possibility of obtaining electricity from magnetism. In 1824 he was experimenting with a magnet in a helix connected with a galvanometer, without result, and similar experiments were made in 1825 and 1828. Either the galvanometer was too insensitive or he failed to notice the momentary deflection when the magnet was introduced. On August 29, 1831, the induced current was detected, and ten days of decisive experiment culminated in his paper on 'The Induction of Electric Currents' which was to shape the future of electrical science and electrical industry.

To us it is of special interest that on the very day of the discovery, the first test Faraday applied, after he had observed the motion of the magnetic needle due to the induced current, was to attach platinum wires to the ends of the coil and see if he could detect any decomposition in a drop of copper sulphate solution. The test was not delicate enough, but we find him returning again and again to the chemical power of magneto-electricity until on June 11, 1832, he found in bibulous paper moistened with potassium iodide and starch the most sensitive means of detecting the chemical action of an induced current. A sentence in his letter to Richard Phillips on November 29, 1831, describing his discovery of electro-magnetic induction shows how his mind was running on the problem of conduction in solutions—'I believe it will explain perfectly the transference of elements between the poles of the pile in decomposition.'

His new discovery of magneto-electricity raised afresh in Faraday's mind the old and still disputed problem of the identity of electricities from different sources, and chemical action was one of the tests he applied to its solution. Having shown that common (frictional), voltaic, and magneto-electricity all produce similar physiological, magnetic, chemical and thermal effects, Faraday, on September 14 and 15, 1832, established quantitatively the identical nature of common and voltaic electricity by showing that such quantities of these two kinds of electricity as produced equal effects on the needle of his galvanometer also liberated equal amounts of iodine, as judged by the intensity of the brown stain, when a piece of bibulous paper moistened with potassium iodide was placed in the circuit.

In the work that led up to these decisive results, Faraday had made many experiments on the chemical action produced by a current. In one of them he placed one end of a long piece of litmus paper moistened with sodium sulphate in contact with an electrical machine, while the other end was held opposite to the discharging points. On turning the machine Faraday saw that decomposition took place, the paper becoming red 'where the positive electricity entered from the air.' This proved to him that the decomposition was not dependent on the presence of metallic poles in the solution, and on September 6 he wrote in his note-book, 'Hence it would seem that it is not a mere repulsion of the alkali and attraction of the acid by the positive pole, etc. etc., but that as the current of electricity passes whether by metallic poles or not the elementary particles arrange themselves and that the alkali goes as far as it can with the current in one direction and the acid in the other. The metallic poles used appear to be mere terminations of the decomposable substance.'

'The effects of decomposition would seem rather to depend upon a relief of the chemical affinity in one direction and an exaltation of it on the other rather than to direct attraction and repulsions of the poles.' Here we see the germ of Faraday's ideas on the nature of electrolysis.

In October and November only two days were spent on electrical experiments, and the paper on 'The Identity of Electricities' was not communicated to the Royal Society until December 10, an unusually long delay for Faraday. He was now convinced, it is true on rather slender evidence, that the amount of electro-chemical decomposition is a measure of the quantity of electricity, and the paper contains a statement of his First Law of Electrolysis. After describing the experiments with potassium iodide paper, he says, 'It also follows that for this case of electro-chemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force, is in direct proportion to the absolute quantity of electricity* which passes.'

On December 24 he wrote, 'Can an electric current voltaic or not decompose a solid body, ice. etc. etc. If it can does it give structure at the time. If it cannot what would fused gum, lac, wax, etc.' A cold spell at the end of January enabled him to put this to the test, and he found that while ice would not conduct a voltaic current, conduction occurred immediately the ice melted. 'If ice will not conduct is it because it cannot decompose?'

This led Faraday to examine the conductivity in the fused state of a number of substances which are solid at ordinary temperatures, and to study the products formed during electrolysis. It was a new field for him and he showed his usual experimental skill in devising simple methods for working at high temperatures, including even the use of the oxy-hydrogen blowpipe. He found that a number of substances resembled water in being insulators in the solid state and becoming good conductors if fused, when they were decomposed by the current, but that this phenomenon was by no means universal. He thus arrived at no general conclusion, but the experience he gained in working with fused salts was to prove invaluable later in the year in his work on electro-chemical equivalents. The experiments were finished on April 22, and on April 24

they were communicated to the Royal Society with the title 'On a New Law of Electric Conduction.'

Faraday then turned his attention to the mechanism of conduction in a liquid. On May 2 he passes a strong current through a saturated solution of sodium sulphate and examines it with polarized light both across and along the direction of the current to see if he can detect signs of arrangement of the molecules, but without result. On May 20 he determines the transfer of sulphuric acid during electrolysis by measuring the changes in concentration in two vessels connected by moist asbestos, and on May 27 he shows that the transfer of sulphuric acid differs from that of sodium sulphate of equivalent concentration, 'very evident therefore that the transfer is dependant on the mutual action of the particles.' He summed up his views in a paper to the Royal Society on June 18, the main conclusion being 'that electro-chemical decomposition does not depend on the simultaneous action of two metallic poles,' and the effects of it 'are due to a modification, by the electric current, of the chemical affinity of the particles through or by which that current is passing, giving them the power of acting more forcibly in one direction than in another, and consequently making them travel by a series of successive decompositions and recompositions in opposite directions, and finally causing their expulsion or exclusion at the boundaries of the body under decomposition.'

On May 16 no experiments were recorded in the note-book but among the ideas he jotted down was—'Is the law this (above a certain intensity, *i.e.* the one required for decomposition to take place at all) that whatever the size of plates or number intervening or constant section of decomposing matter, or variable section, or variable strength, or number of series in the battery: that . . . equal currents of electricity measured by the galvanometer evolve equal volumes of gas or effect equal chemical action in a constant medium.' A week later he writes down his plans for testing the law—'By putting cups and expts. in succession and sending the same electrical current through both or all am sure that each is submitted to an equal force. Can try well this way whether the same quantity of different intensity does the same chemical work using same dilute sulphuric acid but different sized poles, and collecting gas, and that will tell—some poles mere wires, others large plates.' Three months elapsed before he actually carried out the experiment: on August 27 he wrote, 'Pursue the investigation, whether the same quantity of electricity always produces an equivalent of chemical decomposition. . . .' On August 30 he found as he expected that the same amount of current liberated the same volume of gas irrespective of the concentration of the acid, the size of the electrodes or the intensity[1] of the current. He obtained the same results with solutions of various salts, and his comment was, 'Strange that with such different substances the same quantity of water should be decomposed by the same current.' These experiments were continued in September and Faraday was constantly puzzling over the effect of various substances in increasing the conducting power of

[1] By intensity Faraday here means current density; later he uses it in the sense of electromotive force.

water. On September 17 he showed that cells containing muriatic acid and sulphuric had given the same volume of hydrogen when connected in series, and he was now busy constructing a simple apparatus to measure the quantity of electricity by means of the volume of gas produced by it. 'The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. . . . I have therefore named it a VOLTA-ELECTROMETER.'[2] Today, following Faraday, we define our practical unit of current by its electrolytic action, and we use his name to denote the fundamental unit of electrochemistry.

On September 19, among his observations, he notes, 'Will not white hot diamond conduct. If so may perhaps crystallise carbon at white heat by power of the voltaic battery.'

He had been worried by the contraction, on standing, of the mixture of oxygen and hydrogen obtained in the electrolysis of sulphuric acid. He traced this to the catalytic activity of the platinum electrode, and showed that the positive and not the negative was effective. This observation led him to spend some weeks investigating the conditions under which platinum and other metals would assist the combination of various gases, when he discovered the retarding effects caused by small quantities of gases such as olefiant gas, carbonic oxide and sulphuretted hydrogen. The results were communicated to the Royal Society on November 30.

Faraday then returned to the investigation of the amount of chemical action produced by the current, and as he recognised that in the electrolysis of aqueous solutions it was doubtful whether the elements liberated at the poles were to be regarded as primary or secondary products, he extended the inquiry to include fused substances, which would be free from this ambiguity. On December 17 he wrote in his note-book, 'Proceeded to decompose dry chlorides, oxides, etc. to ascertain if there also the decomposition was definite and what the equivalent numbers would be.' So quickly was the final stage in the investigation accomplished that on January 9, 1834, the paper containing the Laws of Electrolysis was communicated to the Royal Society. In the first experiment on December 17 fused stannous chloride in a glass tube was decomposed with platinum wire poles with a voltameter in series, and the weight of the tin liberated compared with the weight of water (0·26486 grain) decomposed in the voltameter. '1·76 of tin had been electro-chemically evolved at the exode and of course a corresponding portion of chlorine at the cisode.

$$\text{Now } 0{\cdot}26486 : 1{\cdot}76 :: \overset{\text{W}}{9} : \overset{\text{T}}{59{\cdot}805} \text{ the tin.}$$

The number for Tin is given 58, which is very near indeed for a first experiment, and shows that the electro-chemical equivalent is the same as the Chemical equivalent here.' Note Faraday's first efforts at a new terminology, exode and cisode. Later on the same day the word 'pole,' which suggests the idea of attraction or repulsion, was struck out and 'electrode' written above it for the first time.

[2] The name was contracted to voltameter five years later.

Similar experiments continued all through December with a holiday on Christmas Day.[3] Fused lead chloride, lead borate, and lead iodide gave confirmatory evidence, but the work was difficult and often gave inconclusive results. Faraday was anxious to extend it to the deposition of metals from aqueous solutions, and he found that zinc deposited on a platinum electrode gave an electro-chemical equivalent of 34·08, while the loss in weight of amalgamated zinc in contact with platinum compared with the weight of hydrogen evolved gave in two experiments equivalents of 30·2 and 32·31. 'Excellent,' writes Faraday after the latter result. These researches had strengthened enormously the evidence for his First Law of Electrolysis—'The Chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes'—and they had established the Second Law—'Electro-chemical equivalents coincide, and are the same with ordinary chemical equivalents.'

The paper itself is the most important of Faraday's contributions to electrochemistry, and in it he summarises all his previous work. He begins by introducing the new terminology which he devised with the help of Whewell for the sake of greater precision of expression, and all his new names—electrode, anode, cathode, ion, anion, and cation, electrolyte and electrolysis—we use to-day with the significance which Faraday gave to them. After a short account of the conditions necessary for electro-chemical decomposition, he describes his new volta-electrometer and the evidence that led him to the conclusion that the amount of chemical action is dependent solely on the amount of electricity that passes through it. He next discusses whether the products of electrolysis are primary or secondary, and gives his evidence for the identity of chemical and electro-chemical equivalents. He goes on to consider the absolute quantity of electricity associated with the particles or atoms of matter, pointing out how enormous this must be since 800,000 charges of a Leyden battery each one of which would suffice to kill a cat, 'would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.' Finally he speaks of the experiments in which he showed the equivalence of the hydrogen liberated and the zinc dissolved when platinum and zinc amalgam are placed in contact in dilute acid. He writes: 'the results prove that the quantity of electricity which, being

[3] On December 19 no experiments are recorded, but a few extracts from the note-book show how busy he was, thinking and planning:—

1192. 'With regard to intensity and its meaning, etc. Define intensity if possible and state its relation to quantity, time and conducting power.'

1195–1200. 'Nervous agency of Electricity.'

1207. 'In the table I mean Real Electro chemical equivalents not hypothetical for we shall else outrun fact and lose the information directly before us. . . . I must keep my researches really Experimental and not let them deserve anywhere the character of hypothetical imaginations.'

1212. 'Search for Fluorine by using a plumbago Pos. Pole acting on a fluoride.'

1213. 'This process may finally give rise to some very good processes of analysis in determining weights or at least to some excellent modes of comparing weights of metals . . . a good principle of analysis for it will hold probably in salts as well if properly selected and may use mercury electrodes when convenient.'

A remarkable anticipation of modern methods of electrolytic analysis.

naturally associated with the particles of matter, gives them their combining power, is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that the electricity which decomposes, and that which is evolved by the decomposition of a certain amount of matter, are alike.'

In this way the theories of combination in definite proportions and of electro-chemical affinity were brought into harmony.

Faraday pointed out that 'if we adopt the atomic theory or phraseology then the atoms of bodies . . . have equal quantities of electricity associated with them.' Had he been a believer in the atomic theory he might have made the deduction that electricity, like matter, is atomic in nature. 'I must confess,' he said, 'that I am jealous of the term atom; for, though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature.' And it was left to Helmholtz, in his Faraday lecture of 1881, to point out this most startling result of Faraday's laws: 'If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity.'

As soon as the work for the great paper was ended Faraday returned to a topic that had constantly been in his mind, the long-disputed question of the source of electricity in the voltaic cell, whether it was due to chemical action or to the contact of dissimilar metals. The inquiry was linked with the question of the intensity, or, as we call it, the electromotive force, of the cell, and the relation of this to the power of cells to produce electrolysis.

On January 18, 1834, he made some experiments when Daniell was present on the number of cells necessary to electrolyse sulphuric acid, which showed him 'that the number of decompositions which on the one hand excite or produce the current and on the other retard it constitute the essential point.' 'Beautiful,' he writes, 'I think I see it all, but must go on with fluorine first.' For three weeks he tried to isolate fluorine, and on February 10 thought he had obtained it by the electrolysis of fused lead fluoride. 'Must now lay this subject aside for a while and go to the trough.' On April 7, less than two months later, his paper 'On the Electricity of the Voltaic Pile' reached the Royal Society. In it he presents fresh experimental evidence for the chemical theory of the voltaic cell, including a direct proof that a current may flow when there is no direct contact between the two metals, a slip of paper moistened with potassium iodide being interposed between them. In this paper Faraday first points out clearly the distinction he makes between the quantity of the current and its intensity, or electromotive force, and the relation of the latter to the chemical affinity of the reaction which is producing the current or opposing its passage. During the two months Faraday carried out a large number of experiments on the intensity required to produce electrolysis by varying the number of cells in the battery and seeing how many were required to produce and electrolyse various compounds in solution or in a fused state. On February 10 he notes 'The power of decomposing water a good unit of intensity in voltaic apparatus.'

It is clear from his note-book that his mind was concentrated on the relation between chemical action and the production of electricity, and

he quickly realised that whether a current passes or not depends on the relative magnitudes of the chemical affinities of the reactions taking place in the battery and in the electrolytic cells. On February 19 he wrote: 'Affinity is active at both points, but is as it were connected or related by the current of electricity in the communicating wires, or in other words affinity is electricity and vice versa.' And three days later: 'Must make out what happens in cases of chemical action with no current.'

'We seem to have the power of deciding in certain cases of chemical affinity (as of zinc with the oxygen of water) which of two modes of action of the one power shall be exerted. In the one mode we can transfer the power on it being able to produce elsewhere its equivalent of action in the other it is not transferred on but exerted at the spot. The first is the case of Voltaic Electric production, the other the ordinary cases of chemical affinity. But both are chemical actions and due to one power or principle.'

In other words Faraday saw that a chemical reaction can be carried out in two ways, either by means of a voltaic cell in which the reactants are separated by an electrolyte, or by their direct contact, and further, he identified the electromotive force of the cell with the chemical affinity of the reaction. Half a century was to elapse before the conception of chemical affinity assumed a definite form in chemists' minds, but here Faraday anticipates our modern interpretation. His method of reasoning too, is an instinctive recognition of the Law of Conservation of Energy, and it was in connection with the chemical theory of the cell that he wrote in 1840, 'in no case . . . is there a pure creation or production of power without a corresponding exhaustion of something to supply it.'

It is difficult for us to-day to realise the effect of Faraday's work on the progress of electrochemistry, the clarity of ideas which came from his new nomenclature, the quantitative treatment of the problems which was made possible by his Laws of Electrolysis, the significance of his distinction between quantity of electricity and its intensity or potential, and his association of these two electrical quantities with the magnitude of the chemical change and of the chemical affinity respectively.

Naturally, with so vast and complex a subject, so little understood, even Faraday made mistakes. He thought that in aqueous solutions hydrogen and oxygen were the ions that carried the current, other ions only appearing at the electrodes by secondary action, although in fused salts he knew that these secondary products acted as ions in his own sense of the word. Then, he supposed that only compounds containing one atom of each element could act as electrolytes, and he made rash statements about the existence of a binary oxide and sulphide of antimony. He thought, too, that an element could have only one electro-chemical equivalent, neglecting the different degrees of oxidation of metals. And he assumed that electrolytes possessed a certain measure of metallic conductivity subsidiary to their electrolytic conduction.

It is interesting to read the criticism of Berzelius on Faraday's discovery. Berzelius was the foremost chemist of the day, a pioneer in electrochemistry, a fine experimenter with encyclopædic knowledge, but conservative in outlook. In a letter to Wöhler, Berzelius criticises the paper, and says that it has diminished greatly his opinion of Faraday. He proved

experimentally Faraday's error about the existence of a suboxide and sulphide of antimony, and he pointed out the difficulty respecting elements with more than one valency. But his main reason for criticising Faraday's Laws was the improbability that the same current would decompose equivalent quantities of substances such as the oxides of silver and of potassium which differ so greatly in chemical affinity. Here Berzelius falls into the error of confusing what Faraday called the quantity and intensity of electricity, and we see how much clearer a view Faraday had of the relations between electricity and chemical action. It is amazing how Faraday's instinct guided him and kept him to the right path, enabling him to seize on the relevant evidence and neglect the apparent exceptions. Berzelius sees all the difficulties, all the apparent anomalies, detects infallibly any experimental errors, but fails to grasp the essential truth of Faraday's ideas which stand to-day without modification.

There is something uncanny in Faraday's avoidance of pitfalls and his recognition of fundamental truths. As Kohlrausch said of him, 'Er riecht die Wahrheit'—he smells the truth: or in Tyndall's words, 'Faraday was more than a philosopher: he was a prophet.'

During the century that has elapsed since Faraday's discoveries, the conduction of electricity in solutions has remained one of the central problems of chemistry and physics, and I propose now to trace briefly the steps by which we have arrived at our present position and to recall to you the chief landmarks in the history of the ionic theory. There have been three main phases in the development of the problem—Firstly, the discovery of the general relationships between the conductivity of solutions and the concentration and nature of the dissolved substances, due mainly to the work of Hittorf and Kohlrausch: secondly, the recognition of the relations of these facts to the general theories of chemistry in the classical ionic theory of Arrhenius; and, lastly, the quantitative explanation of the properties of electrolytes in the mathematical theory due to Milner, and to Debye and Hückel.

The direct successor to Faraday was Daniell, who studied in detail the changes in the concentration of electrolytes at the electrodes produced by electrolysis, which had first been observed by Faraday. His papers were published in three letters to Faraday in 1840–44, and their most important result was to show that the current in aqueous solutions is actually carried by the ions of the solute and not, as Faraday had supposed, by the ions of hydrogen and oxygen. Daniell failed to find any reasonable explanation of his transference data.

Ten years later this aspect of electrolysis was the subject of a classical investigation by Hittorf on 'The Migration of Ions during Electrolysis,' which was of the greatest significance for the theory of solutions. Hittorf realised that the changes in concentration round the electrodes could only be explained on the assumption that the ions move with different velocities, and he showed how their relative speeds could be calculated from the change in concentration, a very remarkable achievement in 1853. Not only did Hittorf show great theoretical acumen, but he was an outstanding experimentalist. He devised the methods by which transport numbers are still determined, and so accurate and comprehensive were his results, that until recently they were the main source of our knowledge

of transport numbers. Hittorf, like Faraday, thought that the transference of the two ions through a solution was by means of a Grotthus chain, a view which now seems to us difficult to reconcile with the fact that the ions move at different speeds.

Simultaneously, with the work on transference, a number of observers like Wheatstone, Wiedemann, and Beetz, were studying the conductivity of solutions in the light of Ohm's Law. Ohm's papers were actually published before Faraday's, but Faraday knew no German and we are left to speculate as to what would have been the effect on him of realising the mathematical relationship between the factors that were so often in his mind. The problem was complicated by the polarisation of the electrodes, and progress was slow until Kohlrausch solved this difficulty by the use of alternating current.

Kohlrausch must always remain the outstanding figure in the experimental study of the conductivity of solutions. For forty years his genius for exact measurement, his fine critical brain and his untiring industry were devoted mainly to this work. He devised the experimental methods we use to-day, and he surveyed for us with unerring accuracy the field of aqueous solutions. But his work went far beyond the mere collection of data. In 1876 he recognised the Law of the Independent Mobility of Ions. In 1878 he introduced that most convenient term, the equivalent conductivity of a solution, and he established the modern conception of ionic motion by calculating the mean velocities of the ions and showing that they were of the magnitude that would be expected if the ions were particles of molecular dimensions moving in accordance with the laws of hydrodynamics. In 1886, Lodge confirmed his calculation by measuring the actual velocities of the ions in a known field.

Kohlrausch quickly realised the theoretical importance of work on dilute solutions, devising special methods for their study, and perhaps it is only those who have tried to repeat his measurements both in this field and on the conductivity of pure water, who can really appreciate the experimental skill which attained such accuracy before the days of thermostats. To those of us who work in this field his papers are a constant source of inspiration, and may I acknowledge here my personal debt to him for the encouragement and the ready help which he gave so generously to an unknown beginner ?

The evidence of Kohlrausch and Hittorf for the independent movement of the ions in dilute solutions was so clear that to us it seems surprising that the ionic theory as we know it to-day was not immediately forthcoming. As early as 1857 Clausius had pointed out that, since the resistance of a solution obeys Ohm's Law, no work can be done by the current in separating the molecules into ions, which must therefore be already in existence, as a result, he supposed, of collisions between molecules. But just as the *a priori* conceptions of chemists had prevented the acceptance of Avogadro's hypothesis for nearly half a century, until the chemical evidence in its favour was overwhelming, so again convergent evidence from different quarters, chemical as well as physical, was necessary before the idea that electrolytes might be largely dissociated into electrically charged ions was even entertained. The difficulty in chemists' minds was twofold—firstly, how could mere solution separate a molecule

into the ions of elements with a great affinity for one another; and secondly, how could the ions remain in presence of water without chemical action?

The next phase is almost too well known to need recalling. In 1883 Arrhenius, working on dilute aqueous solutions, concluded that the equivalent conductivity increases with dilution because the proportion of conducting molecules increases. He then found a correspondence between the conductivities of acids and their strength as determined thermochemically by Berthelot, or by the method of displacement. This suggested to him that the molecules which are active as regards conductivity are active chemically, and it occurred to him that these active molecules are dissociated, since this would explain why the heats of neutralisation of all strong acids and bases are the same. He communicated this idea to Ostwald, who was then working on the catalytic activity of different acids, and they found that the catalytic activities of these acids was roughly proportional to their conductivities. In 1886 van't Hoff published his memoir on the analogy between dilute solutions and gases, in which he drew attention to Raoult's measurements of the freezing-points of aqueous solutions, which showed that the influence of one molecule of an electrolyte like potassium chloride was double that of a molecule of a non-electrolyte such as alcohol. 'After reading this memoir,' writes Arrhenius, 'it was quite clear to me that I might dare to say that all those substances which are active, that is all electrolytes, consist of two molecules and not of one; that is, sodium chloride is composed of two molecules, the sodium ion and the chlorine ion. Then the theory of electrolytic dissociation was expressed without any restriction (1887). I had then a threefold basis for my conclusion, the chemical one and the electrical one, and then the thermodynamical one, regarding the freezing-point. On a foundation of three points you may construct a very solid building.'

It is interesting that both Planck and van 't Hoff put to Arrhenius the difficulty that if a salt is partially dissociated into ions, then the equilibrium between the ions and molecules should be in agreement with Guldberg and Waage's Law of Mass Action, while they found by calculating the degree of dissociation by Arrhenius's formula $\alpha = \frac{\Lambda_c}{\Lambda_0}$ that this was not the case. Arrhenius suggested that a better test would be made by considering the dissociation of weak acids which varied over a much wider range and the results, as we know, confirmed his theory, and were embodied in Ostwald's Dilution Law.

On its publication in 1887 the ionic theory met with violent opposition, but, in the hands of Arrhenius, Ostwald, van 't Hoff and Nernst, its value was quickly established in the most varied fields: the theory of concentration cells and of liquid junction potentials, the behaviour of weak acids and bases and the hydrolysis of salts, the properties of indicators, the dissociation of water, and the theory of qualitative and quantitative analysis. Entirely fresh light was thrown on all these problems, and in many instances they admitted of quantitative explanation for the first time. It is true that the reason for ionisation and for the stability of ions remained unknown for many years, until the discoveries of Rutherford and Moseley had made possible Bohr's application of the quantum theory

to the problem of atomic structure. Then the connection between electricity and chemical affinity, which chemists had been seeking since the days of Berzelius and Faraday, suddenly became clear, and we learnt the cause of ionisation and of the stability of ions in solution. Metallic sodium reacts with water in order to give up an electron; the sodium ion having already lost an electron is no longer reactive.

But in spite of the many triumphs of the ionic theory, and its success so far as weak electrolytes are concerned, the discrepancy between the behaviour of strong electrolytes and the mass law, pointed out by Planck and van 't Hoff to Arrhenius, still remained. Kohlrausch had shown that in very dilute solutions the relationship between the equivalent conductivity and concentration of salts was expressed by the equation

$$\Lambda_c = \Lambda_0 - kc^{\frac{1}{2}} \qquad (k = \text{constant})$$

which is incompatible with the mass law. At the beginning of this century the outstanding problem of the ionic theory was this so-called anomaly of strong electrolytes. Up to this point the whole development of the ionic theory had come from the experimental side, and every advance had originated in some new discovery. But the solution of the final problem came not from experiment but from the mathematical physicists, who thus repaid to chemistry the debt which physics owed to Faraday.

Faraday had often drawn attention to 'the enormous electric power of each particle or atom of matter,' *i.e.* the large size of the ionic charge. Helmholtz in 1881, in his Faraday lecture, made a calculation showing that the attractive force between the electrical charges associated with hydrogen and oxygen is 71,000 billion times greater than the gravitational attraction between their masses. It would seem obvious that forces such as these must affect the behaviour of ions, and from time to time suggestions came from chemists—van Laar in 1900, Bjerrum in 1906, Sutherland in 1907—that strong electrolytes were completely dissociated, and that the variations of equivalent conductivity with concentration were due not to a change in the degree of dissociation, but to the varying effect of the interionic forces. Bjerrum had found that the molecular colour of chromium salts was independent of dilution in the absence of complex ions, and explained this on the basis of complete dissociation.

One of the earliest supporters of the ionic theory was Nernst, who made very substantial contributions to it in his theory of concentration cells and of diffusion potentials. Milner, working in his laboratory at Göttingen in 1898, was attracted by the problem of strong electrolytes, and attacked it from the point of view of the interionic forces. The mathematical difficulties were great, and it was not until 1909 that they had been overcome sufficiently to admit of the calculation of the change of total internal energy with dilution. Milner was the first to realise that the ions cannot be distributed at random in a solution since, owing to the Coulomb forces, there must be an excess of positive ions in the neighbourhood of a negative ion and *vice versa*. Thus each ion can be considered as surrounded by a spherical ionic atmosphere, the density of which decreases with the distance from the ion. The electrical potential at the surface of the central ion will therefore be affected by the ionic atmosphere, and by taking into account the changes of potential with

dilution, Milner was able to calculate the freezing-point depression of an electrolyte at different dilutions, assuming that it was completely dissociated. He did not, however, attack the problem of conductivity.

Another ten years elapsed without further progress until a discussion at Zurich in 1921 of the ingenious, but erroneous, theory devised by Ghosh attracted the attention of Debye to a field that was entirely new to him. That lucky accident tempts us to speculate on the potential value of a doubtful hypothesis, for how would the problem stand now if Ghosh's papers had not seen the light of day ?

Debye, who at the time did not know of Milner's work, found a simple mathematical solution of the problem by applying Poisson's equation to the relation between the average potential and the density of charge at any point in the sphere surrounding the central ion, and he showed that the distribution of the charge in the ionic atmosphere depends on the square root of the concentration ; thus, explaining why such diverse properties of solutions as activity coefficients and equivalent conductivities are functions of $c^{\frac{1}{2}}$. With the collaboration of Hückel, the whole problem was attacked in detail, and in 1923 they succeeded in calculating the effect of the ionic atmosphere on the mobility of the ion.

The conductivity of a solution depends on the number of ions that it contains and on their mobility. The classical theory of Arrhenius considers the effect of ionic dissociation on the former factor, whilst the Debye-Hückel theory considers the effect of the interionic forces on the latter.

When an ion moves under an external potential gradient, it has to build up continuously a fresh atmosphere in front of it while the atmosphere behind it has to die away, but since the ionic atmosphere takes a finite time to form or disperse, there will always be an excess of ions of the opposite sign in its rear, and consequently it will be subject to a retardation due to the dissymmetry of the atmosphere, which depends on the velocity with which it is moving. Further, as ions of opposite signs are moving in opposite directions and as both carry with them a certain amount of solvent, the viscous resistance to the motion of the ions will be greater than if the solvent were at rest. Thus, both these effects reduce the mobility of the ion below its value at infinite dilution, and Debye and Hückel arrived at the following equation for the variation of the equivalent conductivity of a z-valent binary electrolyte in a solvent of dielectric constant D at a temperature T,

$$\frac{\Lambda_o - \Lambda_c}{\Lambda_o} = \left\{ \frac{K_1}{(DT)^{\frac{3}{2}}} w_1 + \frac{K_2}{(DT)^{\frac{1}{2}}} w_2 b \right\} \sqrt{2c} \quad . \quad . \quad . \quad . \quad (1)$$

in which the first and second terms on the right-hand side are the dissymmetry and viscosity terms respectively, K_1 and K_2 are universal constants, w_1 and w_2 are valency factors, and b is the average radius of the ions. This reduces to

$$\Lambda_c = \Lambda_o - x\sqrt{c} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is identical in form with the empirical equation of Kohlrausch. Comparison with experimental results shows that the coefficient of $c^{\frac{1}{2}}$ in

equation (1) is of the right order of magnitude if a value is assumed for the ionic radii of 10^{-8} cm. in accordance with X-ray data. But if the value of b is calculated from the ionic mobilities at infinite dilution, assuming Stokes's law to hold, the observed and calculated coefficients are not in exact agreement, *e.g.* for potassium chloride solutions in water at 25° they are 0·461 and 0·547 respectively. This difference is greater than the experimental error.

Onsager, in 1926, pointed out that Debye and Hückel in calculating the effect of the dissymmetry of the atmosphere around a moving ion, had neglected the Brownian movement of the ion and that a correcting factor of $2 - \sqrt{2}$ must be introduced on this account. His final equation has the same general form as Debye and Hückel's, and when numerical values are inserted for the universal constants, it becomes for a z-valent binary electrolyte

$$\Lambda_c = \Lambda_o - \left[\frac{0{\cdot}986 \times 10^6}{(\mathrm{DT})^{\frac{3}{2}}}(2 - \sqrt{2})\, z^2 \Lambda_o + \frac{58{\cdot}0}{(\mathrm{DT})^{\frac{1}{2}}\eta}\, z\right]\sqrt{2zc} \quad . \quad . \quad (3)$$

where η is the viscosity of the solvent.

For various solvents at 25° this equation becomes for uni-univalent electrolytes:

For water $\Lambda_c = \Lambda_o - (0{\cdot}228\,\Lambda_o + 59{\cdot}8)\,\sqrt{c}$

,, methyl alcohol $\Lambda_c = \Lambda_o - (0{\cdot}957\,\Lambda_o + 158{\cdot}1)\,\sqrt{c}$

,, ethyl alcohol $\Lambda_c = \Lambda_o - (1{\cdot}256\,\Lambda_o + 87{\cdot}8)\,\sqrt{c}$

For sodium chloride in each solvent these equations become:

For water $\Lambda_c = 126{\cdot}4 - (28{\cdot}8 + 59{\cdot}8)\,\sqrt{c}$

,, methyl alcohol $\Lambda_c = 97{\cdot}0 - (93 + 158{\cdot}1)\,\sqrt{c}$

,, ethyl alcohol $\Lambda_c = 43{\cdot}0 - (54 + 87{\cdot}8)\,\sqrt{c}$

These equations show that the dissymmetry term and the viscosity term (the first and second coefficients of $\sqrt{c}$ respectively) are of the same order. Their relative magnitude varies, however, with the properties of the solvent and with the ionic velocities of the ions present.

The fundamental idea of the new theory, the existence of an ionic atmosphere with a finite time of formation and dispersion, has now been definitely established by the work of Wien on conductivities at high electromotive forces, and of Debye, Falkenhagen and Sack on conductivities at high frequencies. With a sufficiently great ionic velocity the atmosphere would not have time to form, while with a high enough frequency its dissymmetry would vanish owing to the negligible displacement of the ion. In both cases the experimental results showed a satisfactory agreement with theory.

The Debye-Hückel-Onsager equation enables us to calculate the change in equivalent conductivity with dilution for any electrolyte in any solvent provided that we know the ionic mobilities and valencies involved and certain physical constants of the solvent, but in comparing the

theory with the results of conductivity determinations, the assumptions underlying it must be remembered, viz.:

(1) That the electrolyte is completely dissociated into point ions and that all interionic forces except Coulomb forces can be neglected.
(2) That corrections for the overlapping of ionic atmospheres can be neglected.
(3) That the solvent between the ions retains the properties of the pure solvent.

These conditions can only be fulfilled in dilute solutions, and a great deal of work has been done recently with uni-univalent electrolytes in a number of solvents to test the theory in the dilute range.

The results show that there is a close approach to a linear relation between Λ_c and $c^{\frac{1}{2}}$, as required by the theory, for strong electrolytes in solvents with a dielectric constant greater than 20. In water, methyl and ethyl alcohols, nitromethane and acetonitrile the slopes of the conductivity curves agree well with theory for a number of electrolytes, and any large deviations are such that they can be explained by ionic association. In fact, the body of evidence now available seems sufficient to justify the use of the Debye-Hückel-Onsager equation to represent the behaviour of a perfect electrolyte in dilute solution and we have, therefore, a new means of judging whether an electrolyte is appreciably associated or not.

The term ionic association marks the contrast between the old outlook and the new. Arrhenius thought of the act of solution as separating the molecules into ions. We know now that most salts are already ionised in the crystalline state, and the question that concerns us is whether the condition of complete ionisation persists on solution of the crystal. In many cases the conductivity of an electrolyte is less than we should expect from the Debye-Hückel equation, indicating that some modification of the state of configuration of the ions has occurred which involves a decrease in the conductivity. This may be due either to the formation of a covalent linkage between the ions or to a modification of their distribution leading in the extreme case to the formation of an ion pair as suggested by Bjerrum. The term ionic association is used to cover both possibilities.

The influence of the solvent on the properties of an electrolyte is illustrated very clearly by a comparison of the behaviour of uni-univalent salts in water and in non-aqueous solvents. In water they are all strong electrolytes with a surprisingly uniform behaviour, as shown in Kohlrausch's classic diagram in the *Zeitschrift für Electrochemie* for 1907. In non-aqueous solvents, however, Walden's comprehensive investigations have shown that individual differences begin to appear as the interionic forces increase and the specific affinities of the ions are brought to light. The question naturally arises as to whether the extent of the ionic association is determined entirely by the interionic forces, *i.e.* by the dielectric constant of the solvent. This is clearly not the case, since a number of salts are strong electrolytes in methyl alcohol and weak electrolytes in nitromethane, which has a higher dielectric constant (37 as against 30·3). Walden and Ulich have pointed out that, in general, non-hydroxylic solvents such as the nitro-compounds and acetone accentuate the individual

differences between electrolytes, while the hydroxylic solvents suppress them. The reason for this is probably to be found in a difference in the nature of the solvation of the ions in the two classes of solvents. There is abundant evidence that the ions in solution have a number of solvent molecules attached to them either by co-ordinate linkages or as a result of the dipole character of the solvent, and these solvent atmospheres exert an important influence on the behaviour of the ions. For instance, the evidence of the ionic mobilities of the alkali metals and of Washburn's transference experiments leaves no doubt that the effective size of the ion is determined by its diameter, including any envelope of solvent which it carries with it.

Bjerrum has considered the effect of ionic size on the probability of the formation of ion pairs, which would contribute nothing to the conductivity of a solution, and has shown that if the sum of the radii of the two ions is below a certain value, the number of ion pairs will increase rapidly. Hence, the solvation of ions may have an important effect in preventing the ions from coming near enough together to form ion pairs.

Sidgwick has considered solvation from the electronic standpoint, and has emphasised the importance of the donor and acceptor properties of hydroxylic solvents in enabling them to form co-ordinate links with both anions and kations. Non-hydroxylic solvents, however, like nitromethane and acetone, can only form co-ordinate links with kations, leaving the anions chemically unsolvated, and it is very significant that in such solvents lithium salts are weak electrolytes, while in hydroxylic solvents they are less associated than the salts of the other alkali metals. It would thus appear that the existence of a chemical link between the solvent molecules and both ions is of cardinal importance in preventing ionic association. This view of the protective action of hydroxylic solvent molecules is confirmed by the effects of small quantities of water on the conductivity of solutions in nitromethane and other non-hydroxylic solvents. For example, the conductivity of lithium thiocyanate, a weak salt, is increased 60 per cent. by the addition of 0·1 per cent. of water, while that of tetra-ethylammonium iodide, a strong electrolyte, is only increased 0·22 per cent. by a similar addition.

Until recently, most of our ideas about electrolytic solutions were based on experience in water, and this gave quite a false impression of the simplicity of the problem, since in water, thanks to its high dielectric constant and to the protection afforded by the chemical solvation of both ions, all uni-univalent salts exhibit almost ideal behaviour. But a survey of non-aqueous solutions reveals at once a much more complex situation, in which the chemical nature of the solvent and the affinities of the ions are often the predominant factors. Thus, a purely physical theory (*pace* Faraday), like that of Debye and Hückel, while invaluable in explaining and predicting the behaviour of an ideal electrolyte, is far from giving a complete picture of electrolytic solutions even in the dilute range, since it leaves out of account the chemical nature both of the ions and of the solvent molecules.

Looking back over the century we see how the mechanism of electrolytic conduction has gradually been disclosed to us, and how in recent years the many-sided influence of the solvent has come more and more into

prominence. By its dielectric constant the solvent determines the magnitude of the interionic forces. By its power of solvating the ions it exerts a decisive influence on the extent to which they form ion pairs or undissociated molecules. And lastly by its viscosity, as well as by the extent of solvation, it determines the mobilities of the ions. The task of the immediate future is to discover the precise nature and extent of the solvent atmosphere around the ions which exerts such an important influence on their properties. And so to-day we find ourselves face to face with a new phase of the problem, and we can repeat with Faraday the words near the close of his great paper on electrolysis: 'Indeed, it is the great beauty of our science, CHEMISTRY, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the doors to further and more abundant knowledge, overflowing with beauty and utility, to those who will be at the easy personal pains of undertaking its experimental investigation.'

SECTION C.—GEOLOGY.

PROBLEMS OF GEOLOGY CONTEMPORARY WITH THE BRITISH ASSOCIATION.

ADDRESS BY

PROF. J. W. GREGORY, LL.D., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

This morning our thoughts inevitably turn back to the first meeting of this Association a century ago, with feelings of proud and respectful homage to the geologists who, despite widespread distrust of the new institution, gave it their effective support. The largest sub-committee at the meeting was that of 'Geology and Geography,' and its twelve members—Buckland, Conybeare, Egerton, J. D. Forbes, Greenough, Hutton, Murchison, J. Phillips, Sedgwick, Wm. Smith, H. Witham (palæobotanist) and Jas. Yates (one of the secretaries of the Association and author on the older rocks of the Midlands)—were all geologists. This sub-committee next year, 1832, became the 'Committee on Geology and Geography' with Greenough as President; by 1836, Geography had become a sub-section with Murchison as its vice-President; in 1838, Section C had two sets of officers, Lyell being President for Geology and Lord Prudhoe for Geography. In 1839 the name of the section was altered to 'Geology and Physical Geography.' Geography was separated as Section E in 1851. Our allied subject, Mineralogy, was at first an independent section, but was merged in Section B, 'Chemistry and Mineralogy,' in 1834.

I. Stratigraphy in 1831.

The geological problems of special interest in 1831 are shown by the contributions prepared at the request of the Association, with first amongst them W. D. Conybeare's 'Report on the Progress, Actual State and

Ulterior Prospects of Geological Science' (1st Rept. B.A. 1831/2, pp. 365–414). Such a survey would now require fifty volumes instead of the fifty pages which sufficed then. There is only time in this address to notice the main problems considered by the Geological Section in 1831 and glance at the progress achieved in regard to them.

Conybeare's Report summarised the position in General Stratigraphy, which was still based on two divisions—the Primary and the Secondary. The Primary included the 'Primarized Slate' of the Alps. The Secondary Group, thanks to the principles established by William Smith, had been classified into four Systems—the Carboniferous including the Old Red Sandstone, the New Red Sandstone (for which, owing to its variegated colouring, Conybeare then proposed the name Pœcilitic System), the Oolitic and the Cretaceous. All below the Old Red Sandstone was left as the Primitive and Transition series. The pre-Carboniferous and post-Cretaceous beds were still in confusion. Thus Phillips in 1829 in his 'Geology of Yorkshire' referred everything below the Carboniferous to the 'Slate Formation'; in 1837, his 'Treatise of Geology' divided the 'Tertiary' merely into the London Clay, the Freshwater Group, and the Crag. Conybeare's Report was illustrated by a geological section from the North of Scotland to the Adriatic near Venice; it shows that the general succession had been established from the Tertiary to the Carboniferous and Old Red Sandstone. The section illustrates the recurrence of geological hypotheses, for its representation of the schists of the Southern Highlands as the altered extension of the rocks of the Southern Uplands has been readopted in recent years by, amongst others, Dr. G. Frodin (1922) and Prof. F. E. Suess (1931).

On the Continent stratigraphy was less developed and some of its leading exponents were working on lines that have not been followed. Alexandre Brongniart, in 1829, in his 'Tableau des Terrains qui composent l'Écorce du Globe,' divided geological history into two—the Période Jovienne, the 'actual epoch' or post-diluvian, and the Période Saturnienne, anterior to 'the last revolution of the globe.' The stratified rocks he divided into four groups, the Clysmien or diluvial, the Izémien or sedimentary, the Hemilysian or transitional, and the Agalysmien or primordial; the last included an upper division the slates and killas, and a lower the schists. The sediments he classified into the Thalassic or Tertiary; the Pelagic or Secondary (Cretaceous and Oolitic) and the Abyssal or Inferior, which ranged from the Lias to the Old Red Sandstone.

England at that time unquestionably held the hegemony of the world in stratigraphy, as shown by the extent to which terms based on English names have permeated the general nomenclature. When the French Government decided on the construction of a national geological map, Élie de Beaumont and Dufrénoy were sent to England to study its classification and use it as a basis. So little progress had been made outside Europe that Macculloch[1] declared in 1831 that 'the study of Arran alone has taught us more than Asia and America united.'

The first great stratigraphical advance after 1831 was Lyell's classification in 1833 of the post-Cretaceous strata. In north-western Europe they

[1] *Syst. Geol.*, vol. I, 1831, p. vii.

lie in isolated basins and are variable in composition; hence, they could not be correlated either by continuity in the field or by their lithology. Lyell classified them by the percentage of their species of fossils that are still living; and this method, supplemented later by the use of type fossils, secured the world-wide recognition of his four Kainozoic Systems.

Lyell's achievement was followed by the foundation of the Silurian System by Murchison. In 1831 all the rocks below the Carboniferous were included in the Primary Division, of which the upper part was known as the 'Grauwacke Group.' Murchison, in 1834, showed that the 'Upper Grauwacke Series' included four fossiliferous series—the Builth and Llandeilo Flags, the Mayhill, the Wenlock and Dudley, and the Ludlow. In July, 1835,[2] he introduced the name Silurian to cover these four series, which he then renamed the Llandeilo, Caradoc, Wenlock, and Ludlow. The underlying beds were left as the 'Slaty Grauwacke.' Sedgwick,[3] a month later, in a communication to the Association at its Dublin Meeting, founded the Cambrian System for the fossiliferous rocks below the Llandeilo and the schists of Anglesey and Carnarvonshire.

The fundamental advance in Geology in the decade beginning 1830 was Lyell's demonstration of the uniformity of geological dynamics. The first volume of his Principles was published in 1830, and Murchison, in 1832, hailed it in his Presidential Address to the Geological Society as 'beginning to unfold the true papyri of geological history.' Conybeare, in his Report to the Association, said it marked 'almost a new era in the history of our science.'

A very different estimate was expressed by Adam Sedgwick, then the leader of British Geology, in his Presidential Address to the Geological Society in 1831; he declared that Lyell's championship of uniformitarianism violated sound reasoning on geological phenomena, and that 'warped by his hypothesis . . . in the language of an advocate, he sometimes forgets the character of an historian.'[4] According to Sedgwick, if Lyell's views of the uniform order of physical events were correct, 'the earth's surface ought to present an indefinite succession of similar phenomena. But as far as I have consulted the book of nature, I would invert the negative in this proposition, and affirm that the earth's surface presents a definite succession of dissimilar phenomena. If this be true, and we are all agreed that it is; and if it be also true, that we know nothing of secondary causes, but by the effects they have produced, then "the undeviating uniformity of secondary causes," "the uniform order of physical events," "the invariable constancy in the order of nature," and other phrases of like kind, are to me, as far as regards the phenomena of geology, words almost without meaning. They may serve to enunciate the propositions of an hypothesis; but they do not describe the true order of nature.'

Sedgwick agreed with Brongniart that the Geological and Historical Periods were essentially distinct; and he remarked regarding the recent appearance of man, 'were there no other zoological fact in secondary

[2] 'On the Silurian System of Rocks.' *Phil. Mag.*, vol. VII, July, 1835, pp. 46–52.
[3] *5th Rep. B.A.* for 1835-1836, Comm., pp. 60–61.
[4] *Proc. Geol. Soc.*, vol. I, 1831, pp. 303, 304–5.

geology, I should consider this, by itself, as absolutely subversive of the first principles of the Huttonian hypothesis.' . . . 'The appearance of man is a geological phenomenon of vast importance, indirectly modifying the whole surface of the earth, breaking in upon any supposition of zoological continuity, and utterly unaccounted for by what we have any right to call the laws of nature.'[5]

Murchison, on the contrary, held that Lyell's demonstration of the unbroken transition between the Pliocene and post-Pliocene had completely swept away the arbitrary demarcation between 'what had been termed the ancient and existing orders of nature.'[6]

II. The Fundamental Problems of 1831.

The geologists of 1831 worked under the handicap of three fundamental uncertainties—the variability of sea-level, the nature of species, and the processes that form mineral veins, while progress was hampered by theological tradition.

1. Sea-Level and the Mobility of the Crust.

The most disturbing doubt was as to the level of the sea. The first explanation of the occurrence of marine deposits on land was, by an obvious analogy, that the tides of some former era had had a longer period with a higher ebb and flow. The natural view that the former extension of sea over the land was due to a change in the sea was adopted by Dante, and was advocated in 1830 in the standard French textbook, d'Aubuison's 'Traité de Géognosie.' D'Aubuison rejected the idea that sloping strata had been deposited as horizontal sheets and been subsequently tilted. He held that the bedded rocks are still in their original positions and their dip is due to deposition upon pre-existing slopes. According to him the sea once covered all the highest mountains and has been lowered by its reduction in volume instead of having been enlarged by water from the interior of the earth.

The problem of sea-level so exercised the minds of the Geological Committee in 1831, that it asked Robert Stevenson—the authority on coastal engineering and grandfather of R. L. Stevenson—to report upon the erosion of the English coast and 'the permanence of sea-level.' He[7] replied that he had little to add to two previous papers (1816 and 1820). The eastern coast of England is still being worn back by the sea; but the process is less alarming since it has been recognised that the country gains more land by accretion than is being lost by abrasion. The second section of the enquiry, the variability in sea-level, is of perennial interest as many issues depend upon it. The test case about 1830 was that of the Baltic. Celsius in the eighteenth century had remarked that the Baltic appeared to be receding along parts of the Swedish coast; but as the sea-level along the German coast had undergone no change since Roman times the Swedish evidence was doubted. Leopold von Buch reconciled the two sets of observations by the hypothesis that the Swedish coast

[5] *Proc. Geol. Soc.*, vol. I, 1831, p. 306.
[6] *Ibid.*, 1832, vol. I, p. 375.
[7] *Rep. B.A.*, 1831–2, pp. 582–3.

might be rising about a pivot, while the German coast remained stationary. The facts for both sides of the Baltic were reaffirmed by a joint enquiry of the Swedish Academy of Sciences and the Russian Ministry of Marine; but the inferred rise of the land was rejected by Lyell in the first volume of his Principles (1830, pp. 231–2). He attributed the recession of the sea to the accumulation of sediment and the movement of water in the Baltic by the wind; the beds of shells, 200 ft. above the sea, as at Uddevalla, he thought an example of events that are geologically modern being historically ancient. Lyell concluded that 'the phenomena do not lend the slightest support to the Celsian hypothesis, nor to that extraordinary notion proposed in our own times by Von Buch, who imagines that the whole of the land along the northern and western shores of the Baltic is slowly and insensibly rising!'

Lyell[8] fortunately examined the evidence for himself and after a tour in Sweden, in a paper read to the Association in 1834, accepted Von Buch's conclusion and the fact that parts of the Baltic coast are rising two to three feet in a century, while other parts are stationary.

The Baltic, therefore, gives convincing testimony of the mobility of the land, which is accepted in an extreme form by some champions of isostacy. That principle was put forward from the geological evidence that the rate of the accumulation of sediments so often coincides with the rate of subsidence that the two processes must be dependent, the weight of the sediment being the cause of the subsidence. The correlative, that the unloading of an area by denudation causes its uplift, was advanced by Clarence King (1876). This cause of the rise and fall of land was maintained by Airy (1855) and Pratt (1855, 1859, 1871, &c.), when they found that the mass of the Himalaya is compensated by a deficiency of material in the foundations, and was supported by the gravity surveys of the United States by Hayford, and of the oceanic floors by Hecker and Duffield. The relief of the earth was attributed to the differences in the density of the crust, and the whole surface was regarded as maintained in hydrostatic equilibrium, the mountains rising above the general level because of the lightness of their foundations, as in the Arctic Sea icebergs and hummocks float higher than the floes.

From this doctrine it is claimed that the subsidence of the crust to form oceanic basins, and its uplift into continental masses from oceanic depths are both impossible; and that the oceans and continents have occupied approximately their present positions throughout the whole of geological time. That theory was of service as a reaction against the lightly assumed interchange, as pictured by Tennyson, of roaring streets and central seas; but the form of isostacy that represents the earth's major relief as determined by the perfect hydrostatic equilibrium of the crust is opposed to weighty evidence.

Geology has constantly suffered from the acceptance of views based on mathematical deductions. For a couple of decades during the past century geological progress was disturbed by the verdict that the age of the earth is somewhere between ten million and a hundred million years.

[8] *4th Rep. B.A.*, pp. 652–4; repeated in his Bakerian Lecture to the Royal Society the following year. *Phil. Trans.*, 1835, pp. 1–38.

Many eminent geologists bowed to authority and accepted that restriction. Huxley, with his usual insight, repudiated it as incredible and as due to the mathematical reasoning being based on an unsound foundation. Huxley's faith in geological observation has been fully justified and should encourage geology to apply the same test to isostacy.

The mathematical data for the permanence of the ocean basins seem unreliable. The claim that the ocean floors consist of a continuous sheet of heavy material (sima) was supported by gravity determinations; but the calculations are based on the assumption that the sea surface stands at the spheroid of reference; they disregard all variations in sea-level due to differences in specific gravity, to the piling up of the water by wind and by the lateral attraction of the land. The observations by Duffield during the voyage to Australia of the Association in 1914 did not fully confirm Hecker's results; and the work of Meinesz is inconsistent with the views that the ocean bed is a sheet of basic rock, and that its level is determined simply by its specific gravity.

A stronger argument was based on the claim that earthquake waves travel more quickly under the oceans than under the continents; but the waves from the earthquake off the Newfoundland Banks on November 18, 1929, went westward under North America to California at the same pace as under the Atlantic, even below two of its deeper basins, to Spain.

That some parts of the crust are in such delicate isostatic equilibrium that the surface rises when material is removed by denudation and sinks when loaded with more sediment is well established; but other parts of the surface have not this delicate poise. Regions are worn down to low-lying peneplanes, without being automatically uplifted. Basins may be filled by sediment without the subsidence of the floor which may indeed be uplifted while it is receiving an additional load. Thus parts of Palestine appear to have stood above sea-level from pre-Cambrian to Middle Cretaceous times; the country then sank below an open sea: sediments were laid down upon its floor, and show gradual shallowing conditions, due to the sea having been displaced either by the material or by the uplift of the floor. After the Cretaceous Era the land emerged from the sea and subsequently the floor of the Jordan rift-valley subsided until the shore of the Dead Sea was 1,300 feet below sea-level. This subsidence was not due to a load of sediment, as only a thin sheet had been deposited over the sunken floor. Moreover, in parts of the world faults have a downthrow of 10,000 feet, and there is no evidence that their cause was sedimentation. The dependence of subsidence and sedimentation may be often true for the geosynclines which, owing to the rupture or instability of the crust in consequence of its deformation, are bands of long-continued weakness—or asthenostrophes (weak bands or belts).

Reluctance to accept the hydrostatic equilibrium of all parts of the crust is not due to prejudice against isostacy. I supported that doctrine when those who thought that the removal of a few feet of rock could affect the level of the crust were chided for credulity, although the rise and fall of the shore in places with the ebb and flow of the tide already rested on accurate observation. That effect has since been fully established. But the extension of isostacy to the whole surface of the earth and the

claim that it proves the subsidence of an area to an oceanic depth to be a physical impossibility are contradicted by geological evidence which appear more reliable than calculations based on uncertain assumptions.

2. The Fixity of Species.

The second hindrance to geological progress in 1831 was the belief in the fixity and special creation of species, which was then entrenched by theological authority.

The Geological Committee in 1831 asked J. Phillips to prepare a systematic catalogue of British Fossils—a simpler proposition in the days of fixed species than it appears now. The chief value of fossils was regarded as indices of the age of strata; but their reliability for this purpose was not universally accepted, and led to the bout in 1832 between the two most forceful personalities in British geology at that time. Macculloch—a Franco-Scot from the Channel Islands—was one of the most clear-sighted geologists of the early part of the nineteenth century and Lyell acclaimed[9] him as his greatest geological teacher. In 1831 Macculloch published 'A System of Geology with a Theory of the Earth' (2 vols.) which involved him in controversies that lasted until the tragic ending, four years later, of his rough and stormy life. He was a medical man by training, and a chemist by profession. But his main interest was Geology, at which he worked with whole-hearted devotion. The bias given by his expert knowledge of chemistry may be illustrated by his remark that geology is so dependent on chemical principles that it can make 'scarcely a step without their aid.' He had roused Murchison's just wrath by the statement that in the ten years preceding 1831, 'Geology has scarcely received a valuable addition, and not a single fundamental one.' In repudiating this aspersion, Murchison denounced Macculloch for neglecting or deriding the value of fossils and claimed them as 'the very keystone of our fabric.' The fact that Macculloch had not swept into the palæontological day was not due to ignorance but to his recognition of the limitations in the interpretation of fossils. He pointed to the differences between the living faunas of the English Channel and of the Mediterranean as a warning that the correlation of beds by fossils is not so simple as some optimists then believed. In regard to some fossils, Macculloch was more correct than his critics. To him was due the memorable discovery at Loch Erriboll in the N.W. of Scotland of fossils intercalated in the Highland schists and gneisses. Sedgwick and Murchison[10] rejected these fossils which, they said, 'we cannot regard as organic'; and the discovery was discredited until Salter described the larger collection made by the Cornish Customs officer, Charles Peach. Macculloch—guided only by lithological evidence—was also correct in his view that the Scottish Torridon Sandstone is a 'Primary Sandstone,' whereas Sedgwick and Murchison[11] had 'no hesitation' in identifying it as Old Red Sandstone.

[9] *Proc. Geol. Soc.*, 1836, vol. II, p. 359.
[10] *Trans. Geol. Soc.* (2), vol. III, 1829, pp. 155–6.
[11] *Proc. Geol. Soc.*, vol. I, 1828, p. 79.

The fixity of species had been attacked by Lamarck, but his view of the evolution of one species into another was emphatically rejected by British authorities. Buckland,[12] who was the President of the Association at its first full meeting, denied that he was in any way disposed to favour Lamarck's theory of 'the derivation of existing species from preceding species by successive Transmutations of one form of organisation into another form, independent of the influence of any creative Agent.' Sedgwick[13] repudiated 'the doctrines of spontaneous generation and transmutation of species with all their train of monstrous consequences.' The latter doctrine with all its momentous consequences was added to the principles of geology by a recruit of 1831. Early that year, Charles Darwin began the study of geology and on his return from his first long geological excursion, which he made with Sedgwick in North Wales, received the invitation to go as naturalist with the 'Beagle.' He sailed in her from Plymouth in December, 1831.

Darwin's work on the voyage was mainly concerned with volcanic rocks, with gravity differentiation in molten rocks, with uplifts accompanying earthquakes, and with the evidence of widespread areas of subsidence and uplift as proved by his luminous and now firmly established theory of Coral Islands. Darwin's work on crustal movements was of primary importance; but by his doctrine of Evolution by Natural Selection, over which the tussle between Huxley and Bishop Wilberforce made the Oxford meeting of 1860 famous, Darwin was the most potent influence on the thought of the Victorian era. Darwinism gave palæontology a guiding principle, which greatly enhanced the value of fossils, and relieved geologists of worry over such evidence as that in Chas. Moore's paper to the Association at the York meeting of 1881 (Rep. B.A., p. 610) on the occurrence of feathers in the Laurentian rocks. Evolution gave Geology the fresh interest of gradually unrolling the panorama of life, and to fossils the additional importance of being the positive evidence of the lines along which organisms have developed.

It is true that many evolutionists have rejected Darwin's explanation of evolution as due to natural selection; but according to the general interpretation of the geological record, evolution has been mainly controlled by the environment and has proceeded slowly during the long periods of relative quiescence and more quickly when the tumultuous heaving of the crust produced relatively rapid changes in the physical conditions, as in the depth of the sea, in the temperature of sea-water, and in climate owing to the altered distribution of land and water and relief of the land.

Whatever the conclusion may be as to the motive force of evolution, Darwin's 'Origin of Species' convinced the world of the fact of evolution, and that the successive faunas and floras were part of a progressive series and not a number of independent special creations.

The influence of Darwin on the whole Philosophy of Geology was so helpful that he was the dominant factor in its progress during the second quarter-century of the Association's work.

[12] *Bridgewater Treatise*, vol. I, 1837, p. 585.
[13] *Proc. Geol. Soc.*, vol. I, 1831, p. 305.

2b. Theological Influence on Geology.

The invariability of species was a belief imposed on Geology in 1831 by theological authority, which was still dominant, and many geologists sought peace by maintaining the agreement of Geology and Genesis. Thus, in 1833, Benjamin Silliman wrote on the 'Consistency of the Discoveries of Modern Geology with the Sacred History of the Creation and the Deluge,' and during the same decade was issued a library of such works as J. Pye Smith's 'The Relation between Holy Scriptures and some facts of Geological Science' (1839 and many later editions)—the book recommended to me when I turned to Geology for an explanation of the erratic courses of Essex rivers. Dr. Chalmers repeated his 'geological argument on behalf of a Deity' in his 'Natural Theology' (1835, I, p. 229). The geological standpoint of the day was shown in Buckland's Bridgewater Treatise (1836, p. 414). He argued that living Zoophytes and their extinct predecessors 'all are so similarly constructed on one and the same general type and show such perfect Unity of Design throughout the infinitely varied modifications, under which they now perform, and ever have performed the functions allotted to them, that we can find no explanation of such otherwise mysterious Uniformity, than by referring it to the agency of one and the same Creative Intelligence.' He extended this argument to the whole organic world (*ibid.*, pp. 581–2), as 'such a systematic recurrence of analogous Designs, producing various ends by various combinations of Mechanism, multiplied almost to infinity in their details of application, yet all constructed on the same few common fundamental principles which pervade the living forms of organised Beings that we reasonably conclude all these past and present contrivances to be part of a comprehensive and connected whole, originating in the Will and Power of one and the same Creator.' Hence, the persistence of the same structural plans and the absence of those freak animals, which might be expected if animals had arisen by special Creation—instead of being regarded as evidence of evolution—was by Buckland claimed as proof of special creation by one Creator; and, therefore, as equal evidence against atheism and polytheism. The facts on Buckland's hypothesis indicate that the Almighty did all his designs Himself, and had no drawing and designing office where angels of different grades planned organisms of different degrees of importance.

Buckland admitted the considerable antiquity of the earth but defended the creation of man (*ibid.*, p. 597) at 4004 B.C., and declared in regard to the six days of creation, 'I see no reason for extending the length of any of these beyond a natural day.'

Buckland was, nevertheless, too heterodox for some members of the Association. His view that the earth is indefinitely older than the creation of man was vehemently attacked by Dean Cockburn, the Dean of York, in 1838, and at the meeting of the Association there in 1844. The general feeling was in support of Buckland, but it was not until about thirty years later that geology secured the independence claimed in 1832 by its doughty champion, Murchison,[14] in his assertion of the 'entire disconnexion of our science with the inspired writings.'

[14] *Proc. Geol. Soc.*, vol. I, 1832, p. 377.

Theological prepossessions also maintained the faith of 1831 in the Diluvium as the relic of a world-wide flood. The phenomena of the drift were then quite unintelligible: but men were collecting and recording facts of which they could offer no explanation. Macculloch attributed the Parallel Roads of Glen Roy to a lake whose waters were lowered in stages by the breaching of a barrier at the mouth of the glen: and his inability to explain the barrier did not betray him as to the nature of the terraces, in which he was nearer the truth than Darwin who regarded them as sea beaches. Jas. Maxwell,[15] in 1832, recorded a granite boulder 42 ft. by 38 ft. at Appin on Loch Linnhe that appeared to him quite inexplicable. It was not until 1840 that Louis Agassiz' recognition of the glacial origin of the British diluvium provided the barrier for Macculloch's Glen Roy lake, the transport of Maxwell's boulder, and relieved Geology of the Noachian deluge.

3. The Origin of Ore-Deposits.

The third geological problem to which the Association directed attention was the nature of ore-deposits; and J. Taylor, who was Treasurer of both the Association and the Geological Society, at the meeting in 1833 read a 'Report on the State of Knowledge respecting Mineral Veins.'[16]

The explanation of their formation was delayed by erroneous conceptions as to chemical possibilities. 'Never,' said Macculloch,[17] 'has there been a science, unless it be physic, so encumbered with rubbish as geology,' and he complained that geology suffered great discredit owing to the extent to which some of its exponents 'obtruded their ignorant speculations to enthral the mind of the equally ignorant.' He could have justified his criticisms by some arguments on ore genesis. The classification of mineral veins had been begun in 1791 by Werner, who attributed most of them to the filling of fissures from solutions. Hutton, in 1795, declared, on the contrary, the deposition of native metals from solution a 'physical impossibility,' and that sulphide ores could only be formed at high temperatures; and he concluded from the absence of any trace of the assumed solvent that mineral veins are igneous injections which solidified on the escape of the heat. In 1831, in reaction from such speculations as to genesis, the standard classifications relied on the form of the deposits; thus, Waldung von Waldenstein in 1824 divided them into sheets, stockworks, and masses. Current opinion in England at the time may be seen from Taylor's Report. As the veins of Alston Moor contain lead where they traverse the limestone but not where they cross the shale, Taylor rejected their formation by igneous injection and attributed ordinary ores to sublimation. As it was realised that many mineral veins were too wide to have been formed by the infilling of open fissures, the electric deposition of the ores was advocated by R. W. Fox in papers to the Association[18] and elsewhere. The sublimation hypothesis had been

[15] *Trans. Geol. Soc.* (2), vol. III, pt. 3, p. 488.

[16] *Rep. B.A.* (1833), 1834, pp. 1–25.

[17] *Treatise*, 1831, vol. I, p. vii.

[18] *Rep. B.A.* (1834), 1835, pp. 572–4; vol. VI (1837), pp. 133–7; for 1838, 1839, *Trans.*, p. 90; also *Phil. Mag.*, XIV, 1839, pp. 145–6. *Trans. Geol. Soc.*, ser. 2, vol. V, 1840, pp. 497–8. For modern discussion of electric activity on ore-deposits, v. R. C. Wells, *Bull. U.S.G.S.*, 548, 1914.

advocated by Ami Boué in 1822 and in 1829 in his 'Geognostisches Gemälde von Deutschland' (p. 143), and supported by the general association of mineral veins and igneous rocks; he held that extensive ore-deposits are found only near 'Granite, Syenite, Porphyry and Trap-rocks' (*ibid.*, pp. 42–3). A. L. Necker[19] also urged, despite a few apparent exceptions, the general 'connexion of igneous with metalliferous deposits.'

In the same year the main alternative explanation—the connexion of mineral veins with great fractures—was put forward by J. Fournet in a long memoir, 'Études sur les Dépôts Métallifères,' published in Amédée Burat's edition of d'Aubuison's 'Traité de Géognosie,' (vol. III, 1835, pp. 383–621). Fournet assigned the minor veins to the filling of shrinkage cracks, and small faults; but he regarded the veins of most economic value, as deposited along the major faults due to the 'violent movements' that accompany the upheaval of mountain chains.

That the ores along these fractures were due to rising waters was rendered the more probable by the paper contributed by C. G. B. Daubeny[20] at the 1836 meeting on 'Mineral and Thermal Waters.' The deep-seated source of the ores was strengthened in the famous memoir by Élie de Beaumont on 'Les Émanations volcaniques et métallifères' in 1847, in which he showed the importance of pneumatolyic processes in the formation of some ores. Meanwhile, the theory that ores were derived from waters percolating through the rocks beside the veins had been revived by Bischof (1847), but was generally rejected until his disciples, Forchhammer (1855) and especially Sandberger (1882, 1885), found particles of the ordinary ore metals in all kinds of ancient rocks.

The ascensionist views of the deep-seated source of ores was then eagerly replaced, owing to the preference for a tangible than for an inaccessible source—by the lateral secretion theory, which was extended by Emmons and others of the American school. It was dominant for twenty years, until in 1893 the deep-seated source of most ores was advocated by Posepny in the paper that initiated the present stage of the ore genesis investigations. An intermediate view was advocated by J. D. Kemp and others who recalled attention to the general association of mineral veins with igneous rocks; and the discovery therein of traces of various lode metals led to the view that the constituents of the lodes are derived from igneous rocks in which they are as primary components as the iron and magnesium in the ferro-magnesian silicates, and the manganese in the manganese silicates.

That view is perhaps still the generally accepted theory; but there are serious objections to it: for though there is no inherent improbability in the occurrence of small proportions of many metals in the igneous rocks, the quantity appears insufficient to have provided the metals of the lodes.

The frequent association of igneous rocks and ores may be due to both having been introduced in consequence of crustal disturbances. Many areas with abundant igneous rocks are barren of ores, and such igneous rocks as contain the ordinary lode metals show evidence of alteration by

[19] *Trans. Geol. Soc.* (2) III, 1835, pp. 497-8.
[20] 6*th Rep. B.A.* for 1836, 1837, pp. 1–96.

agencies that probably introduced the ores. Many of the claims for the presence of metals as primary constituents of igneous rocks are invalid. Thus, the oft-quoted gold in the 'diorite' of the Ayrshire Mine in Rhodesia is secondary.[21] In other cases the evidence may be wanting. The first gold recorded from Kenya Colony was in the nepheline-syenite of Mount Jombo, S.W. of Mombasa. The rock appeared quite fresh and for thirty-nine years there has been nothing to show that its gold was not a primary constituent. Miss McKinnon Wood in her second journey to the Kenya coastlands has recently found quartz-lodes containing copper, lead and zinc in neighbouring rocks; and after that discovery there is no reason to regard the gold as an original constituent of the nepheline-syenite.

The distribution of ores indicates their derivation from a layer below the igneous rocks. During earth-movements, masses of the subcrustal layer are forced upward, while metallic emanations rise up the fractures and on reaching the zone above that of the dissociation of water are dissolved in superheated water; the mineral solutions continue the ascent till they reach a zone where the ores are deposited in veins or impregnations.

The primary ores of most metals, other than iron and manganese, appear of deep-seated origin; but after the ore bodies have been exposed and attacked by the agents of denudation, they provide the detrital metallic particles of placer deposits. Where the rocks are permeated by alkaline or acid-charged water, the ore particles are removed in solution and thus placer ores are exceptional among the older rocks. Current opinion seems reluctant to recognise the extent to which ores have been distributed mechanically, and the process has been often disguised by their solution and redeposition *in situ*. In addition to such ores as the gold of the Rand, the copper shale of Mansfeld, and the lead sandstone of Commern being of placer origin, the scattering of ores in detrital grains must have happened frequently and the relics of this process may be more numerous than most authorities on ore-deposits are at present prepared to admit.

4. Élie de Beaumont's Classification of Mountains.

The deep-seated source of ore-deposits bears on the geology of the inner earth, which was exercising geologists in 1831. Appreciation of the help that physics and astronomy could give geology is shown by the inclusion in the Report of the Association (p. 407) for 1832, amongst subjects recommended for examination by geologists, of an 'accurate examination of the conclusions deducible from the known density of the earth, as to the solid structure and composition of its interior'; and 'the examination of the visible disk of the moon, with the view of extending our general knowledge of volcanic forces' (p. 410). The ambition to explain the general plan of the earth inspired the early cosmographers, such as Burnet (1684); but he and his successors in the eighteenth century tried to build without bricks or straw, and their theories collapsed when confronted with elementary facts.

After the foundation of Geology at the end of the eighteenth and early

[21] *Trans. Inst. Min. Eng.*, vol. XXXI, 1907, p. 85.

in the nineteenth century and the discovery of the geographical outlines of the hitherto unknown regions, the efforts to correlate the main structures in the relief of the earth were resumed. The most stimulating worker in this field was then Élie de Beaumont. His correlation and classification of mountain chains were based on the view that the interior of the earth is slowly cooling and contracting so that the rigid outer shell undergoes alternate deformation and recovery of the spheroidal form when the crust again closely embraces the shrinking internal mass. Élie de Beaumont concluded that these crustal movements account for the mountain ranges and main relief of the earth. He realised that the crust is a unit which is affected as a whole by each of the orogenic episodes which upheaved mountain systems at the same date in even distant parts of the earth. He regarded these episodes as separated by long intervals of repose, and held that fold-mountain chains of the same orogeny are recognisable by their trend. He classified the mountains of Europe into four systems, upraised at different dates and each with a characteristic trend, and he claimed that all the members of each system are parallel, according to his use of that term. As the trends were determined by the fracture of a spherical or subspherical shell, he considered that they would be on a regular geometrical pattern, liable to merely local variations, for he treated the crust as practically uniform in strength.

Élie de Beaumont's views attracted earnest attention and at the first meeting of the Association Sedgwick and Conybeare were asked to report whether his maxim that mountain ranges with the same trend were of the same age holds true for the British Isles. The two authors[22] issued a brief report. Sedgwick declared that the older British strata are in strict accordance with Élie de Beaumont's theory (*ibid.*, p. 591) and expressed enthusiastic approval of it in his Presidential Address to the Geological Society. 'The steps,' said Sedgwick,[23] 'by which he reaches this noble generalisation are so clear and convincing as to be little short of physical demonstration. It forms an epoch in the history of our science; and I am using no terms of exaggeration when I say, that in reading the admirable researches of M. de Beaumont, I appeared to myself, page after page, to be acquiring a new geological sense, and a new faculty of induction; and I cannot express my feelings of regret, that during my recent visit to the Eastern Alps I did not possess this grand key to the mysteries of nature.'

Élie de Beaumont's system appeared to be opposed in one respect to Lyell's; for whereas it laid stress on violent earth-movements at recurrent episodes, Lyell laid stress on the continuity of geological evolution, and held that the changes in progress to-day have never been interrupted by different forces or by the same forces acting with extraordinary energy. Sedgwick declared the opposition of the two systems and his adherence to that of Élie de Beaumont (*ibid.*, p. 311). He said 'that the system of M. Élie de Beaumont is directly opposed to a fundamental principle vindicated by Mr. Lyell, cannot admit of doubt. And I have decided to the best of my judgment, in favour of the former author,

[22] *Rep. B.A.* 1st and 2nd meetings, 1831–1832, pp. 587–91.
[23] *Proc. Geol. Soc.*, vol. I, 1832, p. 308.

because his conclusions are not based upon any *a priori* reasoning, but on the evidence of facts; and also, because, in part, they are in accordance with my own observations.'

But the principles of Élie de Beaumont and Lyell are not essentially inconsistent. Lyell repeatedly remarked that the intensity of the forces has varied in the past as it does locally to-day. He insisted that the forces were the same although they may at times have been spent. A lull lasts until the force has regained fresh energy. A volcano has a continuous history, although it may be dormant between its eruptions. The orogenic episodes are the culmination of long accumulated stresses. The Jurassic and Cretaceous disturbances in the Alps were the preliminary movements to the convulsions of the Middle Kainozoic.

Élie de Beaumont's episodes of world-wide mountain formation were not due to the sudden application of an extraneous force but to a long continued stress that was relieved by quick changes during the restoration of stability to the deformed crust. Sedgwick called these episodes 'catastrophes'; but they are as much the result of continuously acting agencies as the eruptions of a volcano and the quiet processes of the dormant intervals.

Conybeare[24] was more critical of Élie de Beaumont's theory. He recognised that some facts support it, but others are inconsistent. He objected to describing the Urals and the American Cordillera as parallel,[25] and to reference to the axes of folds as if they were mathematically straight; and he held that the correlation of English folds by parallelism is unsatisfactory.

E. Boué and other French geologists were more severe in their criticisms; they represented the view that mountain uplifts happened at distinct periods as not new, and the original part of the theory, on which rested the synchronism of distant mountains, as not true.

Élie de Beaumont's work, from his 'Recherches sur quelques unes des révolutions de la Surface du Globe' in 1828, to his 'Système de Montagnes' (3 vols., 1852), laid the foundations of the modern study of the general plan of the earth. His two fundamental principles were that the main movements in the earth's crust are due to its compression owing to the shrinkage of the internal mass, and that the collapse of the crust determines the main features in the relief of the globe. The shrinkage is probably due more to closer packing of the constituents than to cooling. The argument has constantly been advanced that the contraction of the earth cannot have been sufficient to have caused mountain folding; it has also been claimed that the earth is expanding and not shrinking, and has been growing hotter instead of cooler. That the main folding of the crust has been due to compression that at first acted on every part of the earth of which we have evidence and later was confined to special belts seems one of the most certain of geological facts.

[24] *Phil. Mag. n.s.*, 1832, pp. 118–26; 1834, pp. 404–14.

[25] Élie de Beaumont's theory has been often misjudged owing to his use of the term parallel in its original sense of side by side, and not as restricted by mathematical usage. It was objected that though mountain lines along the parallels of latitude are parallel, those along the meridians are not. Élie de Beaumont applied the term to lines parallel to a great circle or along great circles that intersect at a pole and to parallels drawn in reference to a pole elsewhere than at the two existing poles.

Élie de Beaumont, in accordance with the geographical and geological information available between 1820 and 1850, unfortunately adopted as the basis of his fold and fracture pattern for the earth the pentagonal network, which had to him the recommendation of its possession of a high degree of symmetry.

The most obvious fact in the map of the world is that it has no such highly developed symmetry as Élie de Beaumont's system. The pattern is in better accordance with the facts adopted by Lowthian Green, the tetrahedral arrangement of land and water, with the continents upraised at the antipodes to the oceanic depressions, just as on a tetrahedron the projecting coigns are antipodal to a face, of which the middle is nearer the centre, or from the point of view of gravity, is lower than the edges and the coigns. The development of this plan is natural, since by it the crust of the earth would most easily adapt itself to the smaller space into which it is compressed by the shrinkage of the internal mass.

I endeavoured to show in 1899 (Geogr. Journ., vol. XII, pp. 225-40) that Lowthian Green's theory agrees both with the existing distribution of ocean and continent, and with geological history, as it explains the alternation of the slow subsidence of the ocean floors and of crustal storms during which fold-mountain chains are raised by lateral compression; it also explains the alternate emergence of the lands as the ocean basins are enlarged by the sinking of their floors and submergence of the lands by the world-wide advance of the sea due to the shallowing of the oceans when the spheroidal form is recovered after the tetrahedral deformation has exceeded the stability of the crust.

Élie de Beaumont's elaborate classification of mountains has collapsed; for although the foundations were sound, the girders of his superstructure were not. Knowledge of the history and structure of mountain chains was then inadequate and much of it was erroneous. Mountain chains are not straight like crystal edges, but bend in long curves around resistant blocks. Their trend is no test of their age. Moreover, a great mountain chain is not all built at one episode; the Alps are due to uplifts that have happened from at least the Lower Jurassic to the Upper Kainozoic. Lowthian Green, working on the same foundation of a contracting globe, built a structure that was more stable as it was in fuller agreement with the map of the world. If the earth had been a spheroid of revolution and its crust homogeneous, mountain ranges might have developed with the high symmetry of the pentagonal network; but the numerous irregularities in the form and strength of the crust have led to its deformation by fewer faces and have produced a mountain system and arrangement of land and water which is tetrahedral and not pentagonal.

5. Mountain Structure.

Élie de Beaumont's system was built upon the mountain chains; and his conception of their structure was defective; he regarded them as symmetrical ridges and he failed to appreciate the contribution to the Association in 1842 by Henry Darwin Rogers which laid the foundation of the modern theory of mountain formation. Rogers had early in the history of the Association rendered it a medium of co-operation between

American and British Geologists, for he[26] had communicated an instructive 'Report on the Geology of North America' in which he adopted Lyell's Kainozoic nomenclature. In 1842 he read to the Association his joint paper with his brother, W. B. Rogers,[27] 'On the Physical Structure of the Appalachian Chain, as exemplifying the Laws which have regulated the elevation of great Mountain Chains generally.' The Rogers considered the facts at variance with Élie de Beaumont's hypothesis that dislocations of the same age are parallel to the same great circle of the sphere, as they found in the Appalachians nine simultaneous groups of folds which vary in trend up to 60°. They explained the folds as waves in the crust due to a broad belt being pushed forward with accompanying asymmetric folding, overfolding, and inversion. This explanation of fold-mountain chains by a wave-like advance of the crust was adopted by J. D. Dana for mountains in general. He attributed to the Rogers the first geological demonstration of the contraction of the earth, which had been suggested by Descartes and Newton. Rogers' view was confirmed by Bailey Willis[28] for the Appalachians and adopted for the Alps by Suess in his 'Entstehung der Alpen' (1875).

Suess showed that the existing physiography of Europe was mainly due to the Alpine System—including the Pyrenees, Alps, Carpathians and Balkans—having been pushed northward against resistant masses which threw back the waves like forelands along a coast. The Carpathian Mountains advanced northward between the resistant masses of Bohemia and the Platform of South-Western Russia, as waves sweep forward in a bay between two headlands.

Suess in the investigation of mountain structure had the advantage over the geologists of the thirties of more certain petrology. They still worried over the igneous origin of granite, which was regarded by some of them as a metamorphic sediment, and the films of mica in gneiss and micaceous sandstone as due to the same cause. Even a decade later the best informed petrologists were at issue as to whether granite had been injected as a molten mass at a high temperature, or was due to aqueo-igneous action at a low temperature. This controversy waged for years between Durocher and Scheerer in the Bulletin of the Geological Society of France.[29]

These authors were groping in the dark like physiologists before the development of histology. A great advance in the interpretation of the igneous and metamorphic rocks was rendered possible by Sorby's application to them of the microscopic study of transparent sections. He was not the first to prepare thin rock slices, which Williamson had used in his study of fossil plants. Sorby applied the method to the igneous rocks. He[30] announced its illuminating results to the Association at Leeds in 1858 in two papers—'On a new method of determining the Temperature and Pressure at which various Rocks and Minerals were formed,' and 'On some

[26] *Rep. B.A.*, 4th (for 1834), 1835, pp. 1–66.
[27] 12*th Rep. B.A.* (1842), 1843, *Trans.*, pp. 40–2.
[28] 13*th Ann. Rep. U.S.G.S.*, 1893, p. 228.
[29] Ser. 2, vol. IV, 1847, pp. 468–98, 1019–43; vol. VI, 1849, pp. 644–54; vol. VIII, 1851, pp. 500–8.
[30] *Rep. B.A.* (1858), 1859, *Trans.*, pp. 107–8.

Peculiarities in the Arrangement of the Minerals in Igneous Rocks.' He showed that the crystals and bubbles in the fluid cavities in granite prove its deep-seated origin, and that the relations of augite and leucite in Vesuvian lavas demonstrate that the sequence of minerals in igneous rocks is determined not by their fusibility but by their order of crystallisation out of a cooling solution.

The determination of the depth and temperature of consolidation of an igneous rock by its fluid cavities is not quite so simple and certain as Sorby thought; but he explained the anomalies that had led to the Durocher-Scheerer controversy, and his method judiciously applied gave more positive information than had been previously available.

The interpretation of mountain structure had an important reaction on stratigraphy. Suess realised that the great mountain regions of the world were not all due to the folding of the crust; and while vast areas have sunk to form the ocean basins, huge horizontal blocks and sheets of marine deposits have been left as high plateaux, owing to the subsidence of the adjacent areas.

Suess undertook the study of the world geology to interpret the major movements of the crust. He recognised that some encroachments of the sea upon the land were world-wide; he called them the marine transgressions, and explained them by the reduction in size of the ocean basins by the uprise of their floors. Lyell had recognised that this movement had probably caused some invasions of the land by the sea. Dana repeated this view (Manual, 1871, p. 723). Lyell could only regard this process as a probability, but Suess had convincing evidence of these transgressions from areas which geologically were unknown in the time of Lyell.

Many cases of the rise of the sea surface may be due to changes in the oceans' basins and not to a vertical uplift of the land. Suess went further and regarded some high-level horizontal beds as left in their original position by the down-sagging of the crust elsewhere during the contraction of the earth. Dana had previously (Manual, 1871, p. 723) explained the main continental plains at the average height of 1,000 feet above sea-level as relics left upraised by the deepening of the ocean basins.

Suess was so impressed by the predominance of downward movements in sunklands, rift-valleys, and oceanic deeps, and by the absence of any mechanism that he regarded as adequate for widespread horizontal uplifts, that he considered all vertical regional movements must be downward. Suess went too far in his denial of the possibility of widespread vertical uplifts. Various agencies, such as the subcrustal flow of material displaced by an oceanic subsidence, will uplift areas without appreciable tilting. But that the land sometimes emerges owing to lowering of the sea surface and at others is submerged by the rise of the sea-level is now universally admitted. Suess' transgressions give geology a physical basis of correlation more precise and world-wide than is possible from palæontology. A transgression may be simultaneous over the whole world, whereas any special fauna does not appear at all places at precisely the same time.

This fact was early recognised by many geologists.

6. 'The Dogma of Universal Formations.'

In 1831 the stratigraphical principle that Conybeare dismissed as 'Werner's dogma of Universal Formations' was being actively discussed. He held that in distant lands corresponding formations need not be synchronous. This idea—a forecast of Huxley's doctrine of homotaxis—was also adopted by H. D. Rogers in a paper to the Association in 1834, when he adopted Lyell's Kainozoic series not as implying strict identity in time but 'comparative chronological relations' (Rep. B.A. for 1834, 1835, p. 32).

Homotaxis became less important when the age of the earth was expanded from the short estimates once advocated. If the earth's history had occupied only a hundred million years, the geological epochs would have been so brief that more than one would have been required for the migration of a fauna from its centre of origin to its antipodes. Now that as much time is available as the greediest geologist can desire, the length of the minor divisions of geological time is adequate for the spread of faunas throughout any accessible and suitable environment. There is no longer any question of the Devonian fauna of Australia having lived at the same time as the Carboniferous fauna of Europe.

The zonal divisions on the other hand are being found less universal than had been thought. Faunas spread at various rates and by different routes; hence they do not everywhere succeed one another in the same order.

The view of Oppel (1856, &c.) that Ammonite zones in all parts of the world follow in an identical succession, and the expectation that graptolite zones are equally regular and world-wide, have proved exaggerations; and Dr. Spath points out that the sequence of zonal Ammonites differs in different basins (Geol. Mag., 1931, pp. 184–6). Faunas moreover must have often survived only in one area, like the Monotremes and Trigonia in Australia, or have taken shelter in one area when they were driven from another, such as the Cretaceous types of reptiles in the Eocene of Nigeria (Swinton, Bull. G. S. Nigeria, Bull. 13, 1930, pp. 52–4). Nevertheless, the correspondence is remarkable between distant representatives of any one geological epoch. Geologists were once confident that the gaps in the geological column in Europe would be filled by discoveries elsewhere. Such terms as Permo-Carboniferous, Permo-Triassic, Trias-Jura, Cretaceo-Tertiary, Cretaceo-Eocene, Mio-Pliocene, &c., expressed the hope that the beds thus named would fill gaps in the European sequence. Most of these strata have been found to correspond in time with those known in Europe. Palæontologically, it is disappointing that the blanks are so widespread and that no fossils have been found except the *Beltina* fauna and obscure impressions, older than those known to geologists in 1831. The hope that a succession of limestones would be found somewhere to reveal the passage from the Palæozoic to the Mesozoic corals is still unfulfilled.

The lesson from extra-European stratigraphy that seems the most remarkable is the world-wide range of the geological Systems and the general similarity of geological development. The Systems represent definite units in the world's history and not artificial divisions like a week or a calendar month. The physical tests of correlation are of increasing value, and the transgressions are more precise time markers than fossils.

III. Geology in Education.

The progress of geology in most branches of its work contrasts with the decline in its educational status. The clamour at the end of the war for more scientific education might have been expected to revive the educational service of geology ; but it has not shared the extra time given to science in general education, and there has been a drop in the number of students at some schools of geology. This fall has been in part due to the increased attention to geography in English schools, a development geologists heartily welcome. The decline in the educational use of geology is the more remarkable since it has continued to lose many active workers by their absorption in administration. The value of geology as general training in affairs is widely recognised in practice, and is doubtless due to the insight gained by research on problems as varied and complex as those of daily life. The geologists' ordinary task is the interpretation of a tangle of uncertain factors.

During the conferences on the position of science in Education held by the Conjoint Board of Scientific Societies one of the champions of classics remarked that natural science provided magnificent educational material, but it was useless as its teachers had no idea how to use it. When pressed for an explanation of this opinion he referred to the confused and illogical nomenclature of natural science ; he added that when someone with a better trained or more orderly mind introduced a consistent nomenclature it was soon muddled or abandoned.

Geology affords illustrations of the basis for this criticism. After Phillips had provided the logical sequence—Palæozoic, Mesozoic, and Kainozoic—the first term was soon adopted ; Secondary slowly gave place to Mesozoic and was long retained by some Surveys ; but Tertiary is still generally used in this country, though it is being steadily abandoned by English-speaking countries overseas.

So long as geologists prefer to use such combinations as Palæozoic, Mesozoic, and Tertiary ; deny that coal and slate are minerals ; call sand clay to support a theory of its origin ; speak of mud as rock, and use terms that distress those with literary instincts, such as peneplains instead of peneplanes, geology is likely to be regarded as of less value in secondary education than classics, mathematics and physics.

An illogical nomenclature may be preferred by those who are so interested in results that they are indifferent to their expression ; but the price paid is the lowered value of the subject as a medium of education.

IV. The Geological Leaders of the four quarter-centuries.

The scope of geology is so vast and varied that few geologists view it from the same standpoint or would select the same men as the most influential leaders in the quarter-centuries since the Association began its work. My own impression is that from 1830 to 1855 the true prophet of geology was Lyell, with his establishment of the mobility of the land and the uniformity of geological processes. From 1855 to 1880 the main advance was, I think, due to Darwin—who established the fact of evolution and enabled fossils to be interpreted more intelligently and reliably. In the third quarter-century, 1880 to 1905, the influences were more complex.

About 1880 the geologists of the United States revealed phenomena in their Western Mountains which showed that the yardstick that had been proved reliable in North-Western Europe and the Atlantic States of America, was not applicable everywhere. The United States Geological Survey had in 1870 published only two of its Annual Reports and a preliminary report on Colorado ; it issued the first of its Bulletins in 1874 and Gilbert's Monograph on the Henry Mountains in 1877 : its great influence began about 1880. Nevertheless, despite the powerful stimulus of North America on geological thought in the third quarter of the past century the individual influence that seems to me to have been most profound was that of E. Suess.

The last quarter-century is still too near for reliable appreciation of its achievements ; but among the fundamental advances have been those revealing the structure of the inner earth, and especially the interpretation of earthquakes, in which the pioneer was a devoted adherent of the Association, John Milne. The recent study of ore-deposits confirms the evidence from earthquakes that the core of the earth is surrounded by concentric shells and that the metallic ores arise from the shell below the plutonic rocks as gases and solutions. The distribution of ores, if those of any particular metal be considered in reference to their age, is also along bands, due to ruptures in the crust through which the volatile and liquid ore-transporting agents escape from the metallic barysphere. The geology of mineral fields and the extension to most of the younger mountain ranges of the world of the thrustplanes which, though early recognised in mining operations, were first demonstrated in stratigraphy by Lapworth and the British Geological Survey in the North-West of Scotland, have proved that fold-mountains are formed along belts of compression. The intense folding of the crust which at first was world-wide has been confined in later times to narrow belts—showing that the accommodation of a thickening rigid crust to a contracting internal mass has been the dominant influence on geological evolution.

SECTION D.—ZOOLOGY.

A HUNDRED YEARS OF EVOLUTION.

ADDRESS BY

PROF. E. B. POULTON, D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

THINKING over the subject of this address, I have been encouraged by a metaphor given me by Oliver Wendell Holmes at a delightful dinner of the Boston 'Saturday Club' in January, 1894—'Memory in old age is a palimpsest with the records beneath standing out more clearly than those above.' And, indeed, memories of my first British Association, at York in 1881, are clearer than those of many in later years. It was a great meeting, as befitted the fiftieth anniversary, and nearly every Sectional President had been a President of the Association. It also marked a turning-point in evolutionary controversy, being, I believe, the last meeting at which opposition was offered to evolution as apart from its motive cause or causes. From 1881 onwards the battles in this Section have been over Lamarckism and Natural Selection, and their factors, especially Heredity; over the size of the steps and the rate of progress. Evolution itself has been generally accepted. It was different at York in 1881. Dr. Wright's indignation, when the Reptilian affinites of *Archæopteryx* were explained in the Geological Section, was stirred by the hated doctrine which gave meaning and life to the demonstration. I well remember, too, how Prof. O. C. Marsh, discussing one of the meetings in this Section with a young and inexperienced naturalist, said that he had felt rather anxious about the way in which his paper on the Cretaceous toothed birds of America would be received by the President, Sir Richard Owen. His fears were, however, groundless, and all was well.

The difference between the controversies raised in the first and the second of these half-centuries of evolution reminds us that long before Darwin saw his way to an explanation of evolution he was satisfied that evolution was a fact; reminds us, too, that we are celebrating another great centenary, for he sailed in the *Beagle* on December 27, 1831, thus entering upon the five years' voyage which, in his own words, 'was by far the most important event in my life, and has determined my whole career'—the voyage which provided him with the evidence that evolution is a fact. The idea of Natural Selection as a motive cause did not come to him until October 1838, just two years after his return.

The independent discovery and publication of the principle of Natural Selection by Dr. W. C. Wells[1] in 1818 and by Patrick Matthew in 1831

[1] Wells, like Matthew and Chambers, was a Scotsman. He was born (May 1757) of Scottish parents in Charlestown, South Carolina. In the troubled times preceding the Declaration of Independence his father, to quote his own words, 'obliged me to wear a tartan coat, and a blue Scotch bonnet, hoping by these means to make me consider myself a Scotchman. The persecution I hence suffered produced this effect completely.'

followed a very different course, for neither of these men realised the significance of the idea which had come to him. Wells wrote that he had ventured to expound it, 'though at the hazard of its being thought rather fanciful than just,' and Matthew half apologises for the amount of space which has been taken from his main subject. He was, nevertheless, anxious to claim credit when, twenty-nine years later, the importance of the discovery was revealed to him in the *Gardener's Chronicle* reprint of the *Times* review of the *Origin*, the review of which Huxley said, 'I wrote it, I think, faster than I ever wrote anything in my life.' It is interesting to speculate upon what might have happened if the author had called his book 'Arboriculture and Naval Timber' instead of the more severely technical 'Naval Timber and Arboriculture'; for, with the former title, the work might well have been consulted by Darwin who would have been led by the table of contents to discover its significance.

Robert Chambers' *Vestiges of the Natural History of Creation*, which appeared in 1844, undoubtedly includes illuminating thoughts far in advance of the time. Thus, more than once, the author writes of organic life 'pressing in' when suitable conditions arose, 'so that no place which could support any form of organic being might be left for any length of time unoccupied,' and he also speaks of withdrawals when the appropriate conditions pass away. Then, too, Anton Dohrn's 'Functionswechsel' is foreshadowed in the conclusion that 'organs, while preserving a resemblance, are often put to different uses. For example: the ribs become, in the serpent, organs of locomotion, and the snout is extended, in the elephant, into a prehensile instrument.' And, contrasted with this, the author points to the performance of the same functions by 'organs essentially different,' and then to the consideration of rudimentary structures and the recognition that 'such curious features are most conspicuous in animals which form links between various classes.' Of great interest, too, is the forcible rebuke administered to those who maintain that an animal origin for man is a degrading thought.

It was a credulous age and we need not be astonished at the author's belief in a spontaneous, or as he preferred to call it, 'aboriginal,' generation of clover in waste moss ground treated with lime, and his opinion that an explanation based on the presence of dormant or transported seed was 'extremely unsatisfactory'; or, again, his acceptance of the hypothesis, held by some authorities at the time, that parasitic entozoa were produced from 'particles of organised matter' within the host, such a development being, he considered, 'in no small degree favourable to the general doctrine of an organic creation by law.' The authorship of the *Vestiges* was revealed in Alexander Ireland's Introduction to the 12th edition, published in 1884, thirteen years after Chambers' death. The secrecy appears to have been mainly due to a rule, laid down in the Chambers' publishing business, that 'debateable questions in politics and theology' should be avoided.

I have devoted some little time to the *Vestiges*, which I think has hardly received its due, although Darwin fully acknowledged its importance in preparing many minds for a belief in evolution. We know, too, that the author warmly supported the Darwinian cause in the controversy which arose over the *Origin*, and that it was his advocacy which rendered

possible the great encounter at Oxford in 1860; for when Huxley told him that he did not mean to attend the meeting of the Section on June 30—'did not see the good of giving up peace and quietness to be episcopally pounded,' 'Chambers broke into vehement remonstrances, and talked about my deserting them. So I said, "Oh! if you are going to take it that way, I'll come and have my share of what is going on."' And after the meeting, J. R. Green wrote to his College friend, Boyd Dawkins—'I was introduced to Robert Chambers the other day and heard him chuckle over the episcopal defeat.'

Owing to the kindness of Lady Boyd Dawkins and Dr. Leonard Huxley, I am able to print a very interesting and hitherto unpublished letter in which Huxley confirmed the well-known description of the debate given by Green:—

'4 Marlborough Place,
'Abbey Road, N.W.
'June 11, 1883.

'My dear Boyd Dawkins,

'Many thanks for the extract from Green's letter. His account of the matter appears to me to be accurate in all essentials, though, of course, I cannot be sure of the exact words that were used on either side.

'It is curious that your letter should have reached me this morning, when in a couple of hours I shall start for Cambridge for the purpose of delivering the Rede Lecture on the subject of "Evolution." I should not have chosen this topic of my own mere motion. But I found that nothing else would satisfy the expectations of Cambridge! Truly "the whirligig of time brings its revenges."

'I am,
'Yours very sincerely,
'T. H. Huxley.'

Robert Chambers' work appears to have provided the stimulus which led to the preparation of an interesting and surprising manuscript[2] by the younger J. Searles Wood. It was found by my friend, Sir Sidney Harmer, among his father's papers, and bears a note, dated 1866 and signed J.S.W. Jr., stating that it 'was written about 1848 or 1849 and the pencil alterations made at the time.' The paper, however, bearing a water-mark of 1850, supplies a rather comforting correction, for the author was not born until 1830, and at eighteen or nineteen must have been a very precocious youth to have written such a manuscript. Searles Wood, who was a great admirer of Lamarck, was evidently stirred by the immense success of the *Vestiges* and doubtless especially by the statement that the hypothesis of the French naturalist 'deservedly incurred much ridicule, although it contained a glimmer of truth.' The manuscript is, however, far more than a defence of Lamarck: it contains powerful arguments in favour of evolution, based upon the very grounds which convinced Darwin himself—the 'wonderful relationship in the same continent between the

[2] Presented by Sir Sidney Harmer, F.R.S., to the Linnean Society of London. It is hoped that the manuscript will be published in the Proceedings of the Society when the rather difficult task of editing has been completed.

dead and the living,' and between island species, especially in the Galapagos, and those of the nearest continental area. It will be remembered that Darwin's pocket-book for 1837, referring to this very evidence, contains the words, 'These facts (especially latter) origin of all my views.' At the side of a page on which the argument on island life is developed, Searles Wood had noted—'When I wrote this, Mr. Darwin had not broached his hypothesis and was not known to be any other than a believer in creation. J.S.W. Jr. 1866.' Darwin's 'Journal' was first published in 1839, the second edition in 1845, but I have not heard of any reader except Searles Wood who recognised, before the appearance of the Darwin-Wallace Essay and the *Origin*, that Organic Evolution was an irresistible conclusion from the facts recorded by the author. Other important arguments, brought forward in the manuscript, will, I am sure, be read with the utmost interest when it appears in the Linnean Proceedings.

A curiously interesting event in 1858, the year of the Darwin-Wallace Essay, was the appearance of *Omphalos*, so well described, with the eager expectation and bitter disappointment of its author, in Sir Edmund Gosse's *Father and Son*. It is unnecessary to repeat on this occasion the often told and never-to-be-forgotten story of the Joint Essay and the Linnean Society's celebration of its fiftieth anniversary, when Wallace protested in noble and inspiring words against the undue credit which he considered had been allotted to him for his share in the discovery of Natural Selection—a discovery brought to him, as it was brought to Darwin, by the reading of Malthus *On Population*.

The effect of the Darwin-Wallace Essay upon Canon Tristram and the appearance, a few weeks before the *Origin*, of his paper on the Ornithology of the Sahara, was brought before this Section by Prof. Newton at Manchester in 1887, and by the author himself at Nottingham in 1893. It is, however, desirable to emphasise its significance afresh in view of recent attempts to throw doubt on the value of concealing coloration in desert areas. Tristram was led to a belief in Natural Selection when he read the Essay in the light of a recent experience of many months in the Algerian Sahara, where he had observed that 'the upper plumage of every bird, . . . and also the fur of all the small mammals, and the skin of all the Snakes and Lizards, is of one uniform isabelline or sand colour,' and had come to realise the absolute necessity for the vast majority of the species to be thus concealed upon the uniform surface of the desert. Precisely the same necessity had been recognised in South Africa nearly half a century earlier by Burchell, when he observed the protective resemblance of a *Mesembryanthemum* and a grasshopper to pebbles, and the defensive value of thorns and acrid secretions in a bare dry country 'where every juicy vegetable would soon be eaten up by the wild animals.' Burchell's mention of plants with an 'acrid or poisonous juice' suggests the meaning of the conspicuousness of the relatively few black, slow-moving insects which have been thought to throw doubt upon the whole theory of protective coloration in the desert. The problem is complex and the struggle for existence is waged in many ways, important among them being the physiological adaptations by which the imperative need for moisture is satisfied—a subject on which much light has been thrown by P. A. Buxton.

Coming now to the meetings of the British Association and of this Section in the second half-century, we are naturally led to the discussion, 'Are Acquired Characters Hereditary,' at Manchester in 1887, when Weismann, Ray Lankester, Hubrecht and many others spoke; and to the same subject at Newcastle in 1889 when Francis Galton and Fairfield Osborn, our welcome guest to-day, took part in the debate.

It was only natural that Weismann's conclusions should rouse intense opposition, for they undermined the foundations on which so much evolutionary theory had been erected. I remember Sir William Turner's words at one of our meetings about this time—'Whoever believes that acquired characters are not transmitted looks upon life with a single eye' —not in the Biblical sense, but implying monocular vision; also Lawson Tait's dogmatic statement, at a meeting of the Midland Institute at Oxford in 1890, that a believer in Weismann's conclusion 'says that the sun shines black.' One result of the new doctrine—the collapse of Herbert Spencer's *Synthetic Philosophy*, so largely built upon Lamarckian principles, was especially distressing to those who remembered a beneficent power in teaching the world to think; remembered, too, what it had done for themselves in earlier years. But not all naturalists were startled and amazed when Weismann 'awoke us from our dogmatic sleep.' I well remember Ray Lankester's reply when I first mentioned the subject to him—'I believe Weismann is right. I have always doubted the statement that acquired characters are transmitted.' And his two old Oxford friends, H. N. Moseley and Thiselton-Dyer, were also ready to follow Weismann from the first. Two sayings of Weismann may be recalled here—how the 'Continuity of the Germplasm,' the theory which first led him to doubt the accepted views on heredity, came to him when he discovered that 'there was something which had to be carefully preserved' throughout the development of a Hydrozoon, viz., that unexpended portion of the parental germ-cell which will give rise to the germ-cells of the offspring. Shielded and 'carefully preserved' as was this carrier of hereditary qualities, how improbable was the conclusion that it would be effected by the happenings in distant parts of the organism, how doubly improbable the supposition that the effect would reproduce the result of these happenings in the offspring. All this is, of course, well known, but it is interesting to recall it as told by Weismann himself. His other remark was to the effect that if acquired characters could be transmitted, we should not be obliged to search for the evidence. It would have been obvious everywhere.

Although, as my friend and colleague, Prof. Goodrich, has written—'these conclusions of Weismann . . . are the most important contribution to the science of evolution since the publication of Darwin's *Origin of Species*,'[3] it was soon realised that the statement of the problem required revision and that Weismann's terms 'Blastogenic' and 'Somatogenic' were inaccurate; for the germinal or inherent characters are no less dependent on external causes than the somatic or acquired characters. This criticism was developed by Adam Sedgwick in his address to this Section at Dover in 1899 and by Goodrich at Edinburgh in 1921; also, between these two addresses, by Archdall Reid. Furthermore, in the spring of 1890, when I was giving a course of University Extension

[3] *Living Organisms*, Oxford, 1924, pp. 50, 51.

Lectures on ' Evolution and Heredity ' at Gresham College, the same idea was expressed in an answer written by one of the students. I was very fortunate in my audience, which included Prof. A. G. Tansley, F.R.S., Wilfrid Mark Webb, and W. Platt Ball.[4] The last-named student, in one of his answers, wrote to the following effect, if I may trust a memory of over forty years—' Acquired characters are due to external causes acting upon inherent potentialities; inherent characters are due to inherent potentialities acted upon by external causes.' The distinction, which seems at first sight difficult and confusing, is very clearly shown by a simple diagram given by Prof. Goodrich,[5] who considers that the expression ' acquired character ' should be dropped. Its history is, however, so interesting—Erasmus Darwin (1794), Lamarck (1809), Prichard (1813)—and its use still so general that we may hope for its continuance, considering especially the vital importance in everyday life of the facts which it describes. It is difficult to imagine Johannsen's term—' phenotype'—replacing it in discussing the problems of education or crime.

In mentioning the name of the illustrious anthropologist, James Cowles Prichard, I may remind the Section that the non-transmission of acquired characters was maintained by him in the second edition (1826) of his great work, *Researches into the Physical History of Mankind.*[6] I have recently studied the first edition (1813) and find that the same conclusion was affirmed at this earlier date. Thus, on page 195, the author states, ' the changes produced by external causes in the appearance or constitution of the individual, are temporary, and in general acquired characters are transient and have no influence on the progeny.' Again, on page 232, arguing that age-long exposure to heat did not cause the dark colour of tropical races, he continues ' and this fact is only an instance of the prevalence of the general law, which has ordained that the offspring shall always be constructed according to the natural and primitive constitution of the parents and therefore shall inherit only their connate peculiarities and not any of their acquired qualities '—a very remarkable statement to find in a book published eighteen years before the first meeting of the British Association. I must also mention on this occasion the paper[7] contributed to the second meeting at Oxford in 1832, in which Prichard contends, in opposition to Cuvier, ' that the various tribes of men are of one origin.'

The rediscovery of Mendel's work—epoch-making although the birth of the epoch was long delayed—produced an immense effect on the papers and discussions in this Section. Much of the controversy in the first and second decades of this century arose out of the belief that only large variations—or as they were called, ' mutations,' using an old word

[4] Author of ' The Effect of Use and Disuse.' ' Nature Series,' London. The excellent term ' Use-inheritance ' to signify ' the direct inheritance of the effects of use and disuse in kind,' was suggested in this book.

[5] *Ibid.*, p. 54. See also p. 62, n. 1.

[6] *Science Progress*, April 1897. Reprinted in *Essays on Evolution*, Oxford 1908.

[7] ' Abstract of a Comparative Review of Philological and Physical Researches as applied to the History of the Human Species.' The abstract occupies fifteen pages of B.A. Reports, vol. i. (including the first two meetings).

with a new meaning—are subject to Mendelian inheritance, and to the related belief that small variations are not inherited at all. But towards the end of this period the foundations of the controversy vanished, for as Prof. H. S. Jennings[8] pointed out in 1917, the work of W. E. Castle and T. H. Morgan proved that the smallest characters are hereditary, so that 'the objections raised by the mutationists to gradual change through selection are breaking down as a result of the thoroughness of the mutationists' own studies.' To give a single illustration—between a red-eyed and a white-eyed fruit-fly (*Drosophila*) seven gradations of colour intervene, each of them 'heritable in the normal Mendelian manner.' Furthermore, in the middle member of this series, 'Bridges has found seven modifying factors, each of which alters its intensity and gives rise to a secondary grade of colour. Now each [all] of these modifying factors are described "specifically as mutations; as actual changes in the hereditary material."' The author finally concludes that 'Evolution, according to the typical Darwinian scheme, through the occurrence of many small variations and their guidance by natural selection, is perfectly consistent with what experimental and palæontological studies show us'; indeed, it appears to him to be 'more consistent with the data than does any other theory,' a conclusion confirmed by Dr. R. A. Fisher's recent work, 'The Genetical Theory of Natural Selection.' Mendelian heredity also provided an effective answer to a difficulty by which Darwin had been greatly troubled—the supposed 'swamping effect of intercrossing' on which Fleeming Jenkin had written a powerful article.[9] Moreover, it cannot be doubted that Mendelian research, by demonstrating the paramount importance of germinal qualities, played a great part in promoting the general acceptance of Weismann's teaching.

A mistaken belief prevailed in the early years of the Mendelian rediscovery that a new theory of evolution had been revealed to the world and that Darwinism had been abandoned. The true position was emphatically stated by Miss E. R. Saunders at the Cardiff Meeting in 1920—'Mendelism is a theory of heredity; it is not a theory of evolution.'

I need not dwell upon the palæontological evidence for continuous evolution, as Prof. Osborn is here and we shall soon have the pleasure of listening to one who can tell us of the conclusions to be inferred from the matchless record of past ages in the great museum of which he is the Director.

The important subject of Geographical Races or Sub-species will be discussed next Tuesday, and I will now only refer to the splendid work of the Tring Zoological Museum under the guidance of Lord Rothschild, Dr. Hartert and Dr. Jordan, and the conclusions published in their journal in 1903.[10] 'Geographical varieties . . . represent various steps in the evolution of daughter-species'; and 'whoever studies the distinctions of

[8] *Journ. Washington Acad. Sci.*, vol. vii., No. 10, May 19, 1917, p. 281; *American Naturalist*, vol. li., May 1917, p. 301. The statements here reproduced are quoted from a brief summary of these two papers in *Proc. Ent. Soc. Lond.*, 1917, p. lxxxv.

[9] *North British Review*, June 1867.

[10] *Nov. Zool.*, vol. x., 1903, p. 492.

geographical varieties closely and extensively, will smile at the conception of the origin of species *per saltum.*'

The age of the earth, as estimated by Lord Kelvin and Prof. Tait, was one of Darwin's 'sorest troubles.' 'I should rely much on pre-Silurian times,' he wrote in 1871, 'but then comes Sir W. Thomson, like an odious spectre.' Lord Salisbury's treatment of this subject in his address at Oxford in 1894 will be remembered by many. Entirely accepting the fact that Darwin had 'disposed of the doctrine of the immutability of species,' he ridiculed the demands which evolution by Natural Selection makes upon the bank of time. 'Of course if the mathematicians are right, the biologists cannot have what they demand. If, for the purposes of their theory, organic life must have existed on the globe more than a hundred million years ago, it must, under the temperature then prevailing, have existed in a state of vapour. The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race.' I venture to refer to this difficulty, although a difficulty no longer, because it provides a good illustration of the help which so often comes to us at these meetings, and also recalls a vigorous personality, our kindly Treasurer for many years, Prof. John Perry. Walking together on the Sunday of the Leeds Meeting in 1890 he explained to me the evidence on which Thomson and Tait had relied, and said that he believed the argument founded on the cooling of the earth to be sound. When, however, he heard Lord Salisbury's address four years later, and decided to re-examine the evidence, he soon discovered that an important consideration had been overlooked. With his kind help I chose this subject, together with the biological evidence for the age of the habitable globe, for my address at Liverpool in 1896. In the following year as we were travelling across Canada after the Toronto Meeting and the chance of collecting insects for a few minutes at each station could not be resisted, Lord Kelvin said to his wife: 'My dear, I think we must forgive Poulton for thinking that the earth is so very old when he works so hard in one day out of all the endless millions of years in which he believes!' A quarter of a century later 'the Age of the Earth' was the subject of a joint discussion at Edinburgh, when the Thomson-Tait limitation of time was abandoned in consequence of researches on radioactivity.

We now come to biological criticisms of evolution by Natural Selection, especially those urged by my friend Sir John Farmer in his presidential address to the Botanical Section at Leicester in 1907,[11] and concisely restated in 1927.[12] In the latter publication the theory of evolution as it was held forty years ago, and, I may add, very nearly as it is held to-day, was described as 'the notion that the basis of evolutionary change in living forms lay in the gradual summation of almost imperceptibly small variations, and that, in fact, specific change was attributable to selection

[11] An answer to the criticisms in this address appeared in the Introduction to *Essays on Evolution*, Oxford, 1908, p. xliv.

[12] *Proc. Roy. Soc.*, B., vol. 101, 1927, pp. i, ii.

and accumulation of these small variations as the result of environmental conditions.' Except for the implied restriction of selection to 'almost imperceptibly small variations,' the statement appears to express fairly the opinion of many believers in Natural Selection at the centenary of the British Association. One main criticism of this belief was that it led to 'the facile teleology, which, like a noxious weed, had overgrown the solid framework of evolutionary doctrine.' But this was not a necessary nor, in my opinion, a common result of the evolutionary beliefs of those years. Let me give two examples of teleological interpretations offered forty years ago, interpretations which are anything but 'noxious weeds,' being extremely interesting in themselves and pointing directly to further researches and a further strengthening of the 'solid framework.'

On his return from a visit to Ceylon and Southern India in 1889 and 1890 Sir John Farmer gave at Oxford a most interesting lecture on his experiences. I recall two of his observations which have always seemed to me most illuminating. One concerned a *Loranthus*, which is so successful that it threatens the very existence of certain introduced trees. It possesses a viscid fruit which adheres to stem and leaves; then from the seed the embryo puts out a sucker borne at the end of a rather thick stalk which curls down and fixes itself to anything it touches. The stalk then straightens and the fruit, containing the germinating seed, is borne aloft. If, however, as he believed, the sucker becomes attached to an unsuitable surface, the stalk bends over again and makes another attempt to reach a living structure which can be penetrated; and if this fails the process continues, causing the fruit to travel in search of favourable opportunities, naturally denied to those which he often saw thickly covering the telegraph wires in the Nilgiri Hills.[13]

The second observation was made upon flowering plants which depend for cross-fertilisation on insect-visitors and the honey which attracts them. Such flowers are well known to be robbed by insects which bite their way in and steal the honey without doing their work. Now Sir John Farmer observed that in certain species this difficulty was met by the development, on the outside of the flower, of special glands attractive to a bodyguard of ants so that the lazy visitors would be compelled to seek the proper entrance and the thieves driven away. This observation has always seemed to me especially significant, as showing how the simple operation of Natural Selection may simulate a rather elaborate process of reasoning. We may wonder whether it would have satisfied the zoologist of whom Darwin wrote to Lyell: 'Dr. Gray of the British Museum remarked to me that "*selection* was obviously impossible with plants! No one could tell him how it could be possible!" And he may now add that the author did not attempt it to him!'

But if either or both of these interpretations should be disproved, if the ants in these and other analogous associations should be shown, as some believe, to be parasites doing no useful work for the plant—what then? Well, once again hypothesis will have played a fruitful part in stimulating and guiding research.

[13] My friend has kindly refreshed my memory on some of the details.

An often repeated objection to Natural Selection is the difficulty or impossibility of accounting for the earliest stages of useful structures. It is, of course, unwise to attempt an explanation of an unknown origin. We can only await further discoveries and oftentimes admit that there is little hope of success. But the difficulty is frequently completely met by Anton Dohrn's principle of 'change of function.' A new function is often taken over by an organ adapted to perform another, the two at first overlapping and the younger gradually supplanting the older. The various uses of Vertebrate limbs supply a good illustration.

Another valuable principle, working in association with Anton Dohrn's, is the 'Organic Selection' of Mark Baldwin, Lloyd Morgan and Fairfield Osborn. The power of individual adaptability 'acts as the nurse by whose help the species . . . can live through times in which the needed inherent variations are not forthcoming.' But this power of adaptability is itself a product of selection. 'The external forces which awake response in an organism generally belong to its inorganic (physical or chemical) environment, while the usefulness of the response has relation to its organic environment (enemies, prey, &c.). Thus one set of forces supply the stimuli which evoke a response to another and very different set of forces.' [14]

What other theories of evolution have been offered to us by those who would reject or limit the power of Natural Selection? Some of them have been mentioned by a writer in a recent number of *Nature*[15]—'orthogenic variations,' 'established organic architecture,' 'metabolic routine,' 'laws of growth' and 'conditions of organic stability.' Others were named in Sir Peter Chalmers Mitchell's Huxley Memorial Lecture in 1927, and I agree with his description of them as a 'brood of imaginary vital forces, gods placed in machines to account for modes of working we do not understand'; although, in many instances, some supposed manifestation of an internal developmental force receives a ready explanation along the lines suggested by H. W. Bates in his classical paper:—[16]

'The operation of selecting agents, gradually and steadily bringing about the deceptive resemblance of a species to some other definite object, produces the impression of there being some innate principle in species which causes an advance of organisation in a special direction. It seems as though the proper variation always arose in the species, and the mimicry were a predestined goal.' Then, after mentioning other suggested hypotheses, he concludes that all are 'untenable, and the appearances which suggest them illusory. Those who earnestly desire a rational explanation, must, I think, arrive at the conclusion that these apparently miraculous, but always beautiful and wonderful, mimetic resemblances, *and therefore probably every other kind of adaptation in beings*,[17] are brought about by agencies similar to those we have here discussed.'

The writer in *Nature* who marshalled his array of supposed developmental forces, contrasted with them 'the old nightmare view of evolution

[14] Poulton in *Proc. American Assoc. for Adv. Sci.*, vol. xlvi., 1897, p. 241.
[15] Vol. 127, March 28, 1931, p. 479.
[16] *Trans. Linn. Soc. Lond.*, vol. xxiii. (1862), Pt. III (1862), Mem. XXXII, p. 514.
[17] Italicised for the purpose of this address.

Élie de Beaumont, in accordance with the geographical and geological information available between 1820 and 1850, unfortunately adopted as the basis of his fold and fracture pattern for the earth the pentagonal network, which had to him the recommendation of its possession of a high degree of symmetry.

The most obvious fact in the map of the world is that it has no such highly developed symmetry as Élie de Beaumont's system. The pattern is in better accordance with the facts adopted by Lowthian Green, the tetrahedral arrangement of land and water, with the continents upraised at the antipodes to the oceanic depressions, just as on a tetrahedron the projecting coigns are antipodal to a face, of which the middle is nearer the centre, or from the point of view of gravity, is lower than the edges and the coigns. The development of this plan is natural, since by it the crust of the earth would most easily adapt itself to the smaller space into which it is compressed by the shrinkage of the internal mass.

I endeavoured to show in 1899 (Geogr. Journ., vol. XII, pp. 225-40) that Lowthian Green's theory agrees both with the existing distribution of ocean and continent, and with geological history, as it explains the alternation of the slow subsidence of the ocean floors and of crustal storms during which fold-mountain chains are raised by lateral compression ; it also explains the alternate emergence of the lands as the ocean basins are enlarged by the sinking of their floors and submergence of the lands by the world-wide advance of the sea due to the shallowing of the oceans when the spheroidal form is recovered after the tetrahedral deformation has exceeded the stability of the crust.

Élie de Beaumont's elaborate classification of mountains has collapsed ; for although the foundations were sound, the girders of his superstructure were not. Knowledge of the history and structure of mountain chains was then inadequate and much of it was erroneous. Mountain chains are not straight like crystal edges, but bend in long curves around resistant blocks. Their trend is no test of their age. Moreover, a great mountain chain is not all built at one episode ; the Alps are due to uplifts that have happened from at least the Lower Jurassic to the Upper Kainozoic. Lowthian Green, working on the same foundation of a contracting globe, built a structure that was more stable as it was in fuller agreement with the map of the world. If the earth had been a spheroid of revolution and its crust homogeneous, mountain ranges might have developed with the high symmetry of the pentagonal network ; but the numerous irregularities in the form and strength of the crust have led to its deformation by fewer faces and have produced a mountain system and arrangement of land and water which is tetrahedral and not pentagonal.

5. Mountain Structure.

Élie de Beaumont's system was built upon the mountain chains ; and his conception of their structure was defective ; he regarded them as symmetrical ridges and he failed to appreciate the contribution to the Association in 1842 by Henry Darwin Rogers which laid the foundation of the modern theory of mountain formation. Rogers had early in the history of the Association rendered it a medium of co-operation between

American and British Geologists, for he[26] had communicated an instructive 'Report on the Geology of North America' in which he adopted Lyell's Kainozoic nomenclature. In 1842 he read to the Association his joint paper with his brother, W. B. Rogers,[27] 'On the Physical Structure of the Appalachian Chain, as exemplifying the Laws which have regulated the elevation of great Mountain Chains generally.' The Rogers considered the facts at variance with Élie de Beaumont's hypothesis that dislocations of the same age are parallel to the same great circle of the sphere, as they found in the Appalachians nine simultaneous groups of folds which vary in trend up to 60°. They explained the folds as waves in the crust due to a broad belt being pushed forward with accompanying asymmetric folding, overfolding, and inversion. This explanation of fold-mountain chains by a wave-like advance of the crust was adopted by J. D. Dana for mountains in general. He attributed to the Rogers the first geological demonstration of the contraction of the earth, which had been suggested by Descartes and Newton. Rogers' view was confirmed by Bailey Willis[28] for the Appalachians and adopted for the Alps by Suess in his 'Entstehung der Alpen' (1875).

Suess showed that the existing physiography of Europe was mainly due to the Alpine System—including the Pyrenees, Alps, Carpathians and Balkans—having been pushed northward against resistant masses which threw back the waves like forelands along a coast. The Carpathian Mountains advanced northward between the resistant masses of Bohemia and the Platform of South-Western Russia, as waves sweep forward in a bay between two headlands.

Suess in the investigation of mountain structure had the advantage over the geologists of the thirties of more certain petrology. They still worried over the igneous origin of granite, which was regarded by some of them as a metamorphic sediment, and the films of mica in gneiss and micaceous sandstone as due to the same cause. Even a decade later the best informed petrologists were at issue as to whether granite had been injected as a molten mass at a high temperature, or was due to aqueo-igneous action at a low temperature. This controversy waged for years between Durocher and Scheerer in the Bulletin of the Geological Society of France.[29]

These authors were groping in the dark like physiologists before the development of histology. A great advance in the interpretation of the igneous and metamorphic rocks was rendered possible by Sorby's application to them of the microscopic study of transparent sections. He was not the first to prepare thin rock slices, which Williamson had used in his study of fossil plants. Sorby applied the method to the igneous rocks. He[30] announced its illuminating results to the Association at Leeds in 1858 in two papers—'On a new method of determining the Temperature and Pressure at which various Rocks and Minerals were formed,' and 'On some

[26] *Rep. B.A.*, 4th (for 1834), 1835, pp. 1–66.
[27] 12*th Rep. B.A.* (1842), 1843, *Trans.*, pp. 40–2.
[28] 13*th Ann. Rep. U.S.G.S.*, 1893, p. 228.
[29] Ser. 2, vol. IV, 1847, pp. 468–98, 1019–43; vol. VI, 1849, pp. 644–54; vol. VIII, 1851, pp. 500–8.
[30] *Rep. B.A.* (1858), 1859, *Trans.*, pp. 107–8.

Peculiarities in the Arrangement of the Minerals in Igneous Rocks.' He showed that the crystals and bubbles in the fluid cavities in granite prove its deep-seated origin, and that the relations of augite and leucite in Vesuvian lavas demonstrate that the sequence of minerals in igneous rocks is determined not by their fusibility but by their order of crystallisation out of a cooling solution.

The determination of the depth and temperature of consolidation of an igneous rock by its fluid cavities is not quite so simple and certain as Sorby thought; but he explained the anomalies that had led to the Durocher-Scheerer controversy, and his method judiciously applied gave more positive information than had been previously available.

The interpretation of mountain structure had an important reaction on stratigraphy. Suess realised that the great mountain regions of the world were not all due to the folding of the crust; and while vast areas have sunk to form the ocean basins, huge horizontal blocks and sheets of marine deposits have been left as high plateaux, owing to the subsidence of the adjacent areas.

Suess undertook the study of the world geology to interpret the major movements of the crust. He recognised that some encroachments of the sea upon the land were world-wide; he called them the marine transgressions, and explained them by the reduction in size of the ocean basins by the uprise of their floors. Lyell had recognised that this movement had probably caused some invasions of the land by the sea. Dana repeated this view (Manual, 1871, p. 723). Lyell could only regard this process as a probability, but Suess had convincing evidence of these transgressions from areas which geologically were unknown in the time of Lyell.

Many cases of the rise of the sea surface may be due to changes in the oceans' basins and not to a vertical uplift of the land. Suess went further and regarded some high-level horizontal beds as left in their original position by the down-sagging of the crust elsewhere during the contraction of the earth. Dana had previously (Manual, 1871, p. 723) explained the main continental plains at the average height of 1,000 feet above sea-level as relics left upraised by the deepening of the ocean basins.

Suess was so impressed by the predominance of downward movements in sunklands, rift-valleys, and oceanic deeps, and by the absence of any mechanism that he regarded as adequate for widespread horizontal uplifts, that he considered all vertical regional movements must be downward. Suess went too far in his denial of the possibility of widespread vertical uplifts. Various agencies, such as the subcrustal flow of material displaced by an oceanic subsidence, will uplift areas without appreciable tilting. But that the land sometimes emerges owing to lowering of the sea surface and at others is submerged by the rise of the sea-level is now universally admitted. Suess' transgressions give geology a physical basis of correlation more precise and world-wide than is possible from palæontology. A transgression may be simultaneous over the whole world, whereas any special fauna does not appear at all places at precisely the same time.

This fact was early recognised by many geologists.

6. 'The Dogma of Universal Formations.'

In 1831 the stratigraphical principle that Conybeare dismissed as 'Werner's dogma of Universal Formations' was being actively discussed. He held that in distant lands corresponding formations need not be synchronous. This idea—a forecast of Huxley's doctrine of homotaxis—was also adopted by H. D. Rogers in a paper to the Association in 1834, when he adopted Lyell's Kainozoic series not as implying strict identity in time but 'comparative chronological relations' (Rep. B.A. for 1834, 1835, p. 32).

Homotaxis became less important when the age of the earth was expanded from the short estimates once advocated. If the earth's history had occupied only a hundred million years, the geological epochs would have been so brief that more than one would have been required for the migration of a fauna from its centre of origin to its antipodes. Now that as much time is available as the greediest geologist can desire, the length of the minor divisions of geological time is adequate for the spread of faunas throughout any accessible and suitable environment. There is no longer any question of the Devonian fauna of Australia having lived at the same time as the Carboniferous fauna of Europe.

The zonal divisions on the other hand are being found less universal than had been thought. Faunas spread at various rates and by different routes; hence they do not everywhere succeed one another in the same order.

The view of Oppel (1856, &c.) that Ammonite zones in all parts of the world follow in an identical succession, and the expectation that graptolite zones are equally regular and world-wide, have proved exaggerations; and Dr. Spath points out that the sequence of zonal Ammonites differs in different basins (Geol. Mag., 1931, pp. 184–6). Faunas moreover must have often survived only in one area, like the Monotremes and Trigonia in Australia, or have taken shelter in one area when they were driven from another, such as the Cretaceous types of reptiles in the Eocene of Nigeria (Swinton, Bull. G. S. Nigeria, Bull. 13, 1930, pp. 52–4). Nevertheless, the correspondence is remarkable between distant representatives of any one geological epoch. Geologists were once confident that the gaps in the geological column in Europe would be filled by discoveries elsewhere. Such terms as Permo-Carboniferous, Permo-Triassic, Trias-Jura, Cretaceo-Tertiary, Cretaceo-Eocene, Mio-Pliocene, &c., expressed the hope that the beds thus named would fill gaps in the European sequence. Most of these strata have been found to correspond in time with those known in Europe. Palæontologically, it is disappointing that the blanks are so widespread and that no fossils have been found except the *Beltina* fauna and obscure impressions, older than those known to geologists in 1831. The hope that a succession of limestones would be found somewhere to reveal the passage from the Palæozoic to the Mesozoic corals is still unfulfilled.

The lesson from extra-European stratigraphy that seems the most remarkable is the world-wide range of the geological Systems and the general similarity of geological development. The Systems represent definite units in the world's history and not artificial divisions like a week or a calendar month. The physical tests of correlation are of increasing value, and the transgressions are more precise time markers than fossils.

III. Geology in Education.

The progress of geology in most branches of its work contrasts with the decline in its educational status. The clamour at the end of the war for more scientific education might have been expected to revive the educational service of geology; but it has not shared the extra time given to science in general education, and there has been a drop in the number of students at some schools of geology. This fall has been in part due to the increased attention to geography in English schools, a development geologists heartily welcome. The decline in the educational use of geology is the more remarkable since it has continued to lose many active workers by their absorption in administration. The value of geology as general training in affairs is widely recognised in practice, and is doubtless due to the insight gained by research on problems as varied and complex as those of daily life. The geologists' ordinary task is the interpretation of a tangle of uncertain factors.

During the conferences on the position of science in Education held by the Conjoint Board of Scientific Societies one of the champions of classics remarked that natural science provided magnificent educational material, but it was useless as its teachers had no idea how to use it. When pressed for an explanation of this opinion he referred to the confused and illogical nomenclature of natural science; he added that when someone with a better trained or more orderly mind introduced a consistent nomenclature it was soon muddled or abandoned.

Geology affords illustrations of the basis for this criticism. After Phillips had provided the logical sequence—Palæozoic, Mesozoic, and Kainozoic—the first term was soon adopted; Secondary slowly gave place to Mesozoic and was long retained by some Surveys; but Tertiary is still generally used in this country, though it is being steadily abandoned by English-speaking countries overseas.

So long as geologists prefer to use such combinations as Palæozoic, Mesozoic, and Tertiary; deny that coal and slate are minerals; call sand clay to support a theory of its origin; speak of mud as rock, and use terms that distress those with literary instincts, such as peneplains instead of peneplanes, geology is likely to be regarded as of less value in secondary education than classics, mathematics and physics.

An illogical nomenclature may be preferred by those who are so interested in results that they are indifferent to their expression; but the price paid is the lowered value of the subject as a medium of education.

IV. The Geological Leaders of the four quarter-centuries.

The scope of geology is so vast and varied that few geologists view it from the same standpoint or would select the same men as the most influential leaders in the quarter-centuries since the Association began its work. My own impression is that from 1830 to 1855 the true prophet of geology was Lyell, with his establishment of the mobility of the land and the uniformity of geological processes. From 1855 to 1880 the main advance was, I think, due to Darwin—who established the fact of evolution and enabled fossils to be interpreted more intelligently and reliably. In the third quarter-century, 1880 to 1905, the influences were more complex.

About 1880 the geologists of the United States revealed phenomena in their Western Mountains which showed that the yardstick that had been proved reliable in North-Western Europe and the Atlantic States of America, was not applicable everywhere. The United States Geological Survey had in 1870 published only two of its Annual Reports and a preliminary report on Colorado; it issued the first of its Bulletins in 1874 and Gilbert's Monograph on the Henry Mountains in 1877: its great influence began about 1880. Nevertheless, despite the powerful stimulus of North America on geological thought in the third quarter of the past century the individual influence that seems to me to have been most profound was that of E. Suess.

The last quarter-century is still too near for reliable appreciation of its achievements; but among the fundamental advances have been those revealing the structure of the inner earth, and especially the interpretation of earthquakes, in which the pioneer was a devoted adherent of the Association, John Milne. The recent study of ore-deposits confirms the evidence from earthquakes that the core of the earth is surrounded by concentric shells and that the metallic ores arise from the shell below the plutonic rocks as gases and solutions. The distribution of ores, if those of any particular metal be considered in reference to their age, is also along bands, due to ruptures in the crust through which the volatile and liquid ore-transporting agents escape from the metallic barysphere. The geology of mineral fields and the extension to most of the younger mountain ranges of the world of the thrustplanes which, though early recognised in mining operations, were first demonstrated in stratigraphy by Lapworth and the British Geological Survey in the North-West of Scotland, have proved that fold-mountains are formed along belts of compression. The intense folding of the crust which at first was world-wide has been confined in later times to narrow belts—showing that the accommodation of a thickening rigid crust to a contracting internal mass has been the dominant influence on geological evolution.

SECTION D.—ZOOLOGY.

A HUNDRED YEARS OF EVOLUTION.

ADDRESS BY

PROF: E. B. POULTON, D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

THINKING over the subject of this address, I have been encouraged by a metaphor given me by Oliver Wendell Holmes at a delightful dinner of the Boston 'Saturday Club' in January, 1894—'Memory in old age is a palimpsest with the records beneath standing out more clearly than those above.' And, indeed, memories of my first British Association, at York in 1881, are clearer than those of many in later years. It was a great meeting, as befitted the fiftieth anniversary, and nearly every Sectional President had been a President of the Association. It also marked a turning-point in evolutionary controversy, being, I believe, the last meeting at which opposition was offered to evolution as apart from its motive cause or causes. From 1881 onwards the battles in this Section have been over Lamarckism and Natural Selection, and their factors, especially Heredity; over the size of the steps and the rate of progress. Evolution itself has been generally accepted. It was different at York in 1881. Dr. Wright's indignation, when the Reptilian affinites of *Archæopteryx* were explained in the Geological Section, was stirred by the hated doctrine which gave meaning and life to the demonstration. I well remember, too, how Prof. O. C. Marsh, discussing one of the meetings in this Section with a young and inexperienced naturalist, said that he had felt rather anxious about the way in which his paper on the Cretaceous toothed birds of America would be received by the President, Sir Richard Owen. His fears were, however, groundless, and all was well.

The difference between the controversies raised in the first and the second of these half-centuries of evolution reminds us that long before Darwin saw his way to an explanation of evolution he was satisfied that evolution was a fact; reminds us, too, that we are celebrating another great centenary, for he sailed in the *Beagle* on December 27, 1831, thus entering upon the five years' voyage which, in his own words, 'was by far the most important event in my life, and has determined my whole career'—the voyage which provided him with the evidence that evolution is a fact. The idea of Natural Selection as a motive cause did not come to him until October 1838, just two years after his return.

The independent discovery and publication of the principle of Natural Selection by Dr. W. C. Wells[1] in 1818 and by Patrick Matthew in 1831

[1] Wells, like Matthew and Chambers, was a Scotsman. He was born (May 1757) of Scottish parents in Charlestown, South Carolina. In the troubled times preceding the Declaration of Independence his father, to quote his own words, 'obliged me to wear a tartan coat, and a blue Scotch bonnet, hoping by these means to make me consider myself a Scotchman. The persecution I hence suffered produced this effect completely.'

followed a very different course, for neither of these men realised the significance of the idea which had come to him. Wells wrote that he had ventured to expound it, 'though at the hazard of its being thought rather fanciful than just,' and Matthew half apologises for the amount of space which has been taken from his main subject. He was, nevertheless, anxious to claim credit when, twenty-nine years later, the importance of the discovery was revealed to him in the *Gardener's Chronicle* reprint of the *Times* review of the *Origin*, the review of which Huxley said, 'I wrote it, I think, faster than I ever wrote anything in my life.' It is interesting to speculate upon what might have happened if the author had called his book 'Arboriculture and Naval Timber' instead of the more severely technical 'Naval Timber and Arboriculture'; for, with the former title, the work might well have been consulted by Darwin who would have been led by the table of contents to discover its significance.

Robert Chambers' *Vestiges of the Natural History of Creation*, which appeared in 1844, undoubtedly includes illuminating thoughts far in advance of the time. Thus, more than once, the author writes of organic life 'pressing in' when suitable conditions arose, 'so that no place which could support any form of organic being might be left for any length of time unoccupied,' and he also speaks of withdrawals when the appropriate conditions pass away. Then, too, Anton Dohrn's 'Functionswechsel' is foreshadowed in the conclusion that 'organs, while preserving a resemblance, are often put to different uses. For example: the ribs become, in the serpent, organs of locomotion, and the snout is extended, in the elephant, into a prehensile instrument.' And, contrasted with this, the author points to the performance of the same functions by 'organs essentially different,' and then to the consideration of rudimentary structures and the recognition that 'such curious features are most conspicuous in animals which form links between various classes.' Of great interest, too, is the forcible rebuke administered to those who maintain that an animal origin for man is a degrading thought.

It was a credulous age and we need not be astonished at the author's belief in a spontaneous, or as he preferred to call it, 'aboriginal,' generation of clover in waste moss ground treated with lime, and his opinion that an explanation based on the presence of dormant or transported seed was 'extremely unsatisfactory'; or, again, his acceptance of the hypothesis, held by some authorities at the time, that parasitic entozoa were produced from 'particles of organised matter' within the host, such a development being, he considered, 'in no small degree favourable to the general doctrine of an organic creation by law.' The authorship of the *Vestiges* was revealed in Alexander Ireland's Introduction to the 12th edition, published in 1884, thirteen years after Chambers' death. The secrecy appears to have been mainly due to a rule, laid down in the Chambers' publishing business, that 'debateable questions in politics and theology' should be avoided.

I have devoted some little time to the *Vestiges*, which I think has hardly received its due, although Darwin fully acknowledged its importance in preparing many minds for a belief in evolution. We know, too, that the author warmly supported the Darwinian cause in the controversy which arose over the *Origin*, and that it was his advocacy which rendered

possible the great encounter at Oxford in 1860; for when Huxley told him that he did not mean to attend the meeting of the Section on June 30—'did not see the good of giving up peace and quietness to be episcopally pounded,' 'Chambers broke into vehement remonstrances, and talked about my deserting them. So I said, "Oh! if you are going to take it that way, I'll come and have my share of what is going on."' And after the meeting, J. R. Green wrote to his College friend, Boyd Dawkins—'I was introduced to Robert Chambers the other day and heard him chuckle over the episcopal defeat.'

Owing to the kindness of Lady Boyd Dawkins and Dr. Leonard Huxley, I am able to print a very interesting and hitherto unpublished letter in which Huxley confirmed the well-known description of the debate given by Green:—

'4 Marlborough Place,
'Abbey Road, N.W.
'June 11, 1883.

'My dear Boyd Dawkins,

'Many thanks for the extract from Green's letter. His account of the matter appears to me to be accurate in all essentials, though, of course, I cannot be sure of the exact words that were used on either side.

'It is curious that your letter should have reached me this morning, when in a couple of hours I shall start for Cambridge for the purpose of delivering the Rede Lecture on the subject of "Evolution." I should not have chosen this topic of my own mere motion. But I found that nothing else would satisfy the expectations of Cambridge! Truly "the whirligig of time brings its revenges."

'I am,
'Yours very sincerely,
'T. H. Huxley.'

Robert Chambers' work appears to have provided the stimulus which led to the preparation of an interesting and surprising manuscript[2] by the younger J. Searles Wood. It was found by my friend, Sir Sidney Harmer, among his father's papers, and bears a note, dated 1866 and signed J.S.W. Jr., stating that it 'was written about 1848 or 1849 and the pencil alterations made at the time.' The paper, however, bearing a water-mark of 1850, supplies a rather comforting correction, for the author was not born until 1830, and at eighteen or nineteen must have been a very precocious youth to have written such a manuscript. Searles Wood, who was a great admirer of Lamarck, was evidently stirred by the immense success of the *Vestiges* and doubtless especially by the statement that the hypothesis of the French naturalist 'deservedly incurred much ridicule, although it contained a glimmer of truth.' The manuscript is, however, far more than a defence of Lamarck: it contains powerful arguments in favour of evolution, based upon the very grounds which convinced Darwin himself—the 'wonderful relationship in the same continent between the

[2] Presented by Sir Sidney Harmer, F.R.S., to the Linnean Society of London. It is hoped that the manuscript will be published in the Proceedings of the Society when the rather difficult task of editing has been completed.

dead and the living,' and between island species, especially in the Galapagos, and those of the nearest continental area. It will be remembered that Darwin's pocket-book for 1837, referring to this very evidence, contains the words, 'These facts (especially latter) origin of all my views.' At the side of a page on which the argument on island life is developed, Searles Wood had noted—'When I wrote this, Mr. Darwin had not broached his hypothesis and was not known to be any other than a believer in creation. J.S.W. Jr. 1866.' Darwin's 'Journal' was first published in 1839, the second edition in 1845, but I have not heard of any reader except Searles Wood who recognised, before the appearance of the Darwin-Wallace Essay and the *Origin*, that Organic Evolution was an irresistible conclusion from the facts recorded by the author. Other important arguments, brought forward in the manuscript, will, I am sure, be read with the utmost interest when it appears in the Linnean Proceedings.

A curiously interesting event in 1858, the year of the Darwin-Wallace Essay, was the appearance of *Omphalos*, so well described, with the eager expectation and bitter disappointment of its author, in Sir Edmund Gosse's *Father and Son*. It is unnecessary to repeat on this occasion the often told and never-to-be-forgotten story of the Joint Essay and the Linnean Society's celebration of its fiftieth anniversary, when Wallace protested in noble and inspiring words against the undue credit which he considered had been allotted to him for his share in the discovery of Natural Selection—a discovery brought to him, as it was brought to Darwin, by the reading of Malthus *On Population*.

The effect of the Darwin-Wallace Essay upon Canon Tristram and the appearance, a few weeks before the *Origin*, of his paper on the Ornithology of the Sahara, was brought before this Section by Prof. Newton at Manchester in 1887, and by the author himself at Nottingham in 1893. It is, however, desirable to emphasise its significance afresh in view of recent attempts to throw doubt on the value of concealing coloration in desert areas. Tristram was led to a belief in Natural Selection when he read the Essay in the light of a recent experience of many months in the Algerian Sahara, where he had observed that 'the upper plumage of every bird, . . . and also the fur of all the small mammals, and the skin of all the Snakes and Lizards, is of one uniform isabelline or sand colour,' and had come to realise the absolute necessity for the vast majority of the species to be thus concealed upon the uniform surface of the desert. Precisely the same necessity had been recognised in South Africa nearly half a century earlier by Burchell, when he observed the protective resemblance of a *Mesembryanthemum* and a grasshopper to pebbles, and the defensive value of thorns and acrid secretions in a bare dry country 'where every juicy vegetable would soon be eaten up by the wild animals.' Burchell's mention of plants with an 'acrid or poisonous juice' suggests the meaning of the conspicuousness of the relatively few black, slow-moving insects which have been thought to throw doubt upon the whole theory of protective coloration in the desert. The problem is complex and the struggle for existence is waged in many ways, important among them being the physiological adaptations by which the imperative need for moisture is satisfied—a subject on which much light has been thrown by P. A. Buxton.

Coming now to the meetings of the British Association and of this Section in the second half-century, we are naturally led to the discussion, 'Are Acquired Characters Hereditary,' at Manchester in 1887, when Weismann, Ray Lankester, Hubrecht and many others spoke; and to the same subject at Newcastle in 1889 when Francis Galton and Fairfield Osborn, our welcome guest to-day, took part in the debate.

It was only natural that Weismann's conclusions should rouse intense opposition, for they undermined the foundations on which so much evolutionary theory had been erected. I remember Sir William Turner's words at one of our meetings about this time—'Whoever believes that acquired characters are not transmitted looks upon life with a single eye' —not in the Biblical sense, but implying monocular vision; also Lawson Tait's dogmatic statement, at a meeting of the Midland Institute at Oxford in 1890, that a believer in Weismann's conclusion 'says that the sun shines black.' One result of the new doctrine—the collapse of Herbert Spencer's *Synthetic Philosophy*, so largely built upon Lamarckian principles, was especially distressing to those who remembered a beneficent power in teaching the world to think; remembered, too, what it had done for themselves in earlier years. But not all naturalists were startled and amazed when Weismann 'awoke us from our dogmatic sleep.' I well remember Ray Lankester's reply when I first mentioned the subject to him—'I believe Weismann is right. I have always doubted the statement that acquired characters are transmitted.' And his two old Oxford friends, H. N. Moseley and Thiselton-Dyer, were also ready to follow Weismann from the first. Two sayings of Weismann may be recalled here—how the 'Continuity of the Germplasm,' the theory which first led him to doubt the accepted views on heredity, came to him when he discovered that 'there was something which had to be carefully preserved' throughout the development of a Hydrozoon, viz., that unexpended portion of the parental germ-cell which will give rise to the germ-cells of the offspring. Shielded and 'carefully preserved' as was this carrier of hereditary qualities, how improbable was the conclusion that it would be effected by the happenings in distant parts of the organism, how doubly improbable the supposition that the effect would reproduce the result of these happenings in the offspring. All this is, of course, well known, but it is interesting to recall it as told by Weismann himself. His other remark was to the effect that if acquired characters could be transmitted, we should not be obliged to search for the evidence. It would have been obvious everywhere.

Although, as my friend and colleague, Prof. Goodrich, has written—'these conclusions of Weismann . . . are the most important contribution to the science of evolution since the publication of Darwin's *Origin of Species*,'[3] it was soon realised that the statement of the problem required revision and that Weismann's terms 'Blastogenic' and 'Somatogenic' were inaccurate; for the germinal or inherent characters are no less dependent on external causes than the somatic or acquired characters. This criticism was developed by Adam Sedgwick in his address to this Section at Dover in 1899 and by Goodrich at Edinburgh in 1921; also, between these two addresses, by Archdall Reid. Furthermore, in the spring of 1890, when I was giving a course of University Extension

[3] *Living Organisms*, Oxford, 1924, pp. 50, 51.

Lectures on 'Evolution and Heredity' at Gresham College, the same idea was expressed in an answer written by one of the students. I was very fortunate in my audience, which included Prof. A. G. Tansley, F.R.S., Wilfrid Mark Webb, and W. Platt Ball.[4] The last-named student, in one of his answers, wrote to the following effect, if I may trust a memory of over forty years—'Acquired characters are due to external causes acting upon inherent potentialities; inherent characters are due to inherent potentialities acted upon by external causes.' The distinction, which seems at first sight difficult and confusing, is very clearly shown by a simple diagram given by Prof. Goodrich,[5] who considers that the expression 'acquired character' should be dropped. Its history is, however, so interesting—Erasmus Darwin (1794), Lamarck (1809), Prichard (1813)—and its use still so general that we may hope for its continuance, considering especially the vital importance in everyday life of the facts which it describes. It is difficult to imagine Johannsen's term—'phenotype'—replacing it in discussing the problems of education or crime.

In mentioning the name of the illustrious anthropologist, James Cowles Prichard, I may remind the Section that the non-transmission of acquired characters was maintained by him in the second edition (1826) of his great work, *Researches into the Physical History of Mankind.*[6] I have recently studied the first edition (1813) and find that the same conclusion was affirmed at this earlier date. Thus, on page 195, the author states, 'the changes produced by external causes in the appearance or constitution of the individual, are temporary, and in general acquired characters are transient and have no influence on the progeny.' Again, on page 232, arguing that age-long exposure to heat did not cause the dark colour of tropical races, he continues 'and this fact is only an instance of the prevalence of the general law, which has ordained that the offspring shall always be constructed according to the natural and primitive constitution of the parents and therefore shall inherit only their connate peculiarities and not any of their acquired qualities'—a very remarkable statement to find in a book published eighteen years before the first meeting of the British Association. I must also mention on this occasion the paper[7] contributed to the second meeting at Oxford in 1832, in which Prichard contends, in opposition to Cuvier, 'that the various tribes of men are of one origin.'

The rediscovery of Mendel's work—epoch-making although the birth of the epoch was long delayed—produced an immense effect on the papers and discussions in this Section. Much of the controversy in the first and second decades of this century arose out of the belief that only large variations—or as they were called, 'mutations,' using an old word

[4] Author of 'The Effect of Use and Disuse.' 'Nature Series,' London. The excellent term 'Use-inheritance' to signify 'the direct inheritance of the effects of use and disuse in kind,' was suggested in this book.

[5] *Ibid.*, p. 54. See also p. 62, n. 1.

[6] *Science Progress*, April 1897. Reprinted in *Essays on Evolution*, Oxford 1908.

[7] 'Abstract of a Comparative Review of Philological and Physical Researches as applied to the History of the Human Species.' The abstract occupies fifteen pages of B.A. Reports, vol. i. (including the first two meetings).

with a new meaning—are subject to Mendelian inheritance, and to the related belief that small variations are not inherited at all. But towards the end of this period the foundations of the controversy vanished, for as Prof. H. S. Jennings[8] pointed out in 1917, the work of W. E. Castle and T. H. Morgan proved that the smallest characters are hereditary, so that 'the objections raised by the mutationists to gradual change through selection are breaking down as a result of the thoroughness of the mutationists' own studies.' To give a single illustration—between a red-eyed and a white-eyed fruit-fly (*Drosophila*) seven gradations of colour intervene, each of them 'heritable in the normal Mendelian manner.' Furthermore, in the middle member of this series, 'Bridges has found seven modifying factors, each of which alters its intensity and gives rise to a secondary grade of colour. Now each [all] of these modifying factors are described "specifically as mutations; as actual changes in the hereditary material."' The author finally concludes that 'Evolution, according to the typical Darwinian scheme, through the occurrence of many small variations and their guidance by natural selection, is perfectly consistent with what experimental and palæontological studies show us'; indeed, it appears to him to be 'more consistent with the data than does any other theory,' a conclusion confirmed by Dr. R. A. Fisher's recent work, 'The Genetical Theory of Natural Selection.' Mendelian heredity also provided an effective answer to a difficulty by which Darwin had been greatly troubled —the supposed 'swamping effect of intercrossing' on which Fleeming Jenkin had written a powerful article.[9] Moreover, it cannot be doubted that Mendelian research, by demonstrating the paramount importance of germinal qualities, played a great part in promoting the general acceptance of Weismann's teaching.

A mistaken belief prevailed in the early years of the Mendelian rediscovery that a new theory of evolution had been revealed to the world and that Darwinism had been abandoned. The true position was emphatically stated by Miss E. R. Saunders at the Cardiff Meeting in 1920 —'Mendelism is a theory of heredity; it is not a theory of evolution.'

I need not dwell upon the palæontological evidence for continuous evolution, as Prof. Osborn is here and we shall soon have the pleasure of listening to one who can tell us of the conclusions to be inferred from the matchless record of past ages in the great museum of which he is the Director.

The important subject of Geographical Races or Sub-species will be discussed next Tuesday, and I will now only refer to the splendid work of the Tring Zoological Museum under the guidance of Lord Rothschild, Dr. Hartert and Dr. Jordan, and the conclusions published in their journal in 1903.[10] 'Geographical varieties . . . represent various steps in the evolution of daughter-species'; and 'whoever studies the distinctions of

[8] *Journ. Washington Acad. Sci.*, vol. vii., No. 10, May 19, 1917, p. 281; *American Naturalist*, vol. li., May 1917, p. 301. The statements here reproduced are quoted from a brief summary of these two papers in *Proc. Ent. Soc. Lond.*, 1917, p. lxxxv.

[9] *North British Review*, June 1867.

[10] *Nov. Zool.*, vol. x., 1903, p. 492.

geographical varieties closely and extensively, will smile at the conception of the origin of species *per saltum*.'

The age of the earth, as estimated by Lord Kelvin and Prof. Tait, was one of Darwin's 'sorest troubles.' 'I should rely much on pre-Silurian times,' he wrote in 1871, 'but then comes Sir W. Thomson, like an odious spectre.' Lord Salisbury's treatment of this subject in his address at Oxford in 1894 will be remembered by many. Entirely accepting the fact that Darwin had 'disposed of the doctrine of the immutability of species,' he ridiculed the demands which evolution by Natural Selection makes upon the bank of time. 'Of course if the mathematicians are right, the biologists cannot have what they demand. If, for the purposes of their theory, organic life must have existed on the globe more than a hundred million years ago, it must, under the temperature then prevailing, have existed in a state of vapour. The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race.' I venture to refer to this difficulty, although a difficulty no longer, because it provides a good illustration of the help which so often comes to us at these meetings, and also recalls a vigorous personality, our kindly Treasurer for many years, Prof. John Perry. Walking together on the Sunday of the Leeds Meeting in 1890 he explained to me the evidence on which Thomson and Tait had relied, and said that he believed the argument founded on the cooling of the earth to be sound. When, however, he heard Lord Salisbury's address four years later, and decided to re-examine the evidence, he soon discovered that an important consideration had been overlooked. With his kind help I chose this subject, together with the biological evidence for the age of the habitable globe, for my address at Liverpool in 1896. In the following year as we were travelling across Canada after the Toronto Meeting and the chance of collecting insects for a few minutes at each station could not be resisted, Lord Kelvin said to his wife: 'My dear, I think we must forgive Poulton for thinking that the earth is so very old when he works so hard in one day out of all the endless millions of years in which he believes!' A quarter of a century later 'the Age of the Earth' was the subject of a joint discussion at Edinburgh, when the Thomson-Tait limitation of time was abandoned in consequence of researches on radioactivity.

We now come to biological criticisms of evolution by Natural Selection, especially those urged by my friend Sir John Farmer in his presidential address to the Botanical Section at Leicester in 1907,[11] and concisely restated in 1927.[12] In the latter publication the theory of evolution as it was held forty years ago, and, I may add, very nearly as it is held to-day, was described as 'the notion that the basis of evolutionary change in living forms lay in the gradual summation of almost imperceptibly small variations, and that, in fact, specific change was attributable to selection

[11] An answer to the criticisms in this address appeared in the Introduction to *Essays on Evolution*, Oxford, 1908, p. xliv.

[12] *Proc. Roy. Soc.*, B., vol. 101, 1927, pp. i, ii.

and accumulation of these small variations as the result of environmental conditions.' Except for the implied restriction of selection to 'almost imperceptibly small variations,' the statement appears to express fairly the opinion of many believers in Natural Selection at the centenary of the British Association. One main criticism of this belief was that it led to 'the facile teleology, which, like a noxious weed, had overgrown the solid framework of evolutionary doctrine.' But this was not a necessary nor, in my opinion, a common result of the evolutionary beliefs of those years. Let me give two examples of teleological interpretations offered forty years ago, interpretations which are anything but 'noxious weeds,' being extremely interesting in themselves and pointing directly to further researches and a further strengthening of the 'solid framework.'

On his return from a visit to Ceylon and Southern India in 1889 and 1890 Sir John Farmer gave at Oxford a most interesting lecture on his experiences. I recall two of his observations which have always seemed to me most illuminating. One concerned a *Loranthus*, which is so successful that it threatens the very existence of certain introduced trees. It possesses a viscid fruit which adheres to stem and leaves; then from the seed the embryo puts out a sucker borne at the end of a rather thick stalk which curls down and fixes itself to anything it touches. The stalk then straightens and the fruit, containing the germinating seed, is borne aloft. If, however, as he believed, the sucker becomes attached to an unsuitable surface, the stalk bends over again and makes another attempt to reach a living structure which can be penetrated; and if this fails the process continues, causing the fruit to travel in search of favourable opportunities, naturally denied to those which he often saw thickly covering the telegraph wires in the Nilgiri Hills.[13]

The second observation was made upon flowering plants which depend for cross-fertilisation on insect-visitors and the honey which attracts them. Such flowers are well known to be robbed by insects which bite their way in and steal the honey without doing their work. Now Sir John Farmer observed that in certain species this difficulty was met by the development, on the outside of the flower, of special glands attractive to a bodyguard of ants so that the lazy visitors would be compelled to seek the proper entrance and the thieves driven away. This observation has always seemed to me especially significant, as showing how the simple operation of Natural Selection may simulate a rather elaborate process of reasoning. We may wonder whether it would have satisfied the zoologist of whom Darwin wrote to Lyell: 'Dr. Gray of the British Museum remarked to me that "*selection* was obviously impossible with plants! No one could tell him how it could be possible!" And he may now add that the author did not attempt it to him!'

But if either or both of these interpretations should be disproved, if the ants in these and other analogous associations should be shown, as some believe, to be parasites doing no useful work for the plant—what then? Well, once again hypothesis will have played a fruitful part in stimulating and guiding research.

[13] My friend has kindly refreshed my memory on some of the details.

An often repeated objection to Natural Selection is the difficulty or impossibility of accounting for the earliest stages of useful structures. It is, of course, unwise to attempt an explanation of an unknown origin. We can only await further discoveries and oftentimes admit that there is little hope of success. But the difficulty is frequently completely met by Anton Dohrn's principle of ' change of function.' A new function is often taken over by an organ adapted to perform another, the two at first overlapping and the younger gradually supplanting the older. The various uses of Vertebrate limbs supply a good illustration.

Another valuable principle, working in association with Anton Dohrn's, is the ' Organic Selection ' of Mark Baldwin, Lloyd Morgan and Fairfield Osborn. The power of individual adaptability ' acts as the nurse by whose help the species . . . can live through times in which the needed inherent variations are not forthcoming.' But this power of adaptability is itself a product of selection. ' The external forces which awake response in an organism generally belong to its inorganic (physical or chemical) environment, while the usefulness of the response has relation to its organic environment (enemies, prey, &c.). Thus one set of forces supply the stimuli which evoke a response to another and very different set of forces.'[14]

What other theories of evolution have been offered to us by those who would reject or limit the power of Natural Selection ? Some of them have been mentioned by a writer in a recent number of *Nature*[15]—' orthogenic variations,' ' established organic architecture,' ' metabolic routine,' ' laws of growth ' and ' conditions of organic stability.' Others were named in Sir Peter Chalmers Mitchell's Huxley Memorial Lecture in 1927, and I agree with his description of them as a ' brood of imaginary vital forces, gods placed in machines to account for modes of working we do not understand ' ; although, in many instances, some supposed manifestation of an internal developmental force receives a ready explanation along the lines suggested by H. W. Bates in his classical paper :—[16]

' The operation of selecting agents, gradually and steadily bringing about the deceptive resemblance of a species to some other definite object, produces the impression of there being some innate principle in species which causes an advance of organisation in a special direction. It seems as though the proper variation always arose in the species, and the mimicry were a predestined goal.' Then, after mentioning other suggested hypotheses, he concludes that all are ' untenable, and the appearances which suggest them illusory. Those who earnestly desire a rational explanation, must, I think, arrive at the conclusion that these apparently miraculous, but always beautiful and wonderful, mimetic resemblances, *and therefore probably every other kind of adaptation in beings*,[17] are brought about by agencies similar to those we have here discussed.'

The writer in *Nature* who marshalled his array of supposed developmental forces, contrasted with them ' the old nightmare view of evolution

[14] Poulton in *Proc. American Assoc. for Adv. Sci.*, vol. xlvi., 1897, p. 241.
[15] Vol. 127, March 28, 1931, p. 479.
[16] *Trans. Linn. Soc. Lond.*, vol. xxiii. (1862), Pt. III (1862), Mem. XXXII, p. 514.
[17] Italicised for the purpose of this address.

as a chapter of accidents.' Well, a nightmare is not uncommon as a result of imperfect digestion!

The concluding section of the address will be almost entirely devoted to recent work with a direct bearing on Darwinian evolution—the researches upon Mimicry and allied subjects undertaken by a band of brother naturalists widely scattered over the world. My greatest scientific interest and delight have been found in this work, and to it for nearly fifty years all available time has been given. The preparation lies far back in childhood, for my earliest memories are of living insects; and then at a fortunate period I read Prof. Raphael Meldola's translation, with his valuable notes, of Weismann's *Studies in the Theory of Descent.* He soon became my dear friend, and for nearly a quarter of a century I relied 'probably even more than I am myself aware upon his sympathy and help.'[18]

I would ask any naturalist who feels inclined to criticise the amount of space given to insect-mimicry in this address, to remember the words of H. W. Bates—'The process by which a mimetic analogy is brought about in nature is a problem which involves that of the origin of all species and all adaptations.'[19]

The evidence for evolution by Natural Selection to be briefly described is in large part associated with the name of Fritz Müller, the illustrious German naturalist of whom Sir Francis Darwin wrote—'The correspondence with Müller, which continued to the close of my father's life, was a source of very great pleasure to him. My impression is that of all his unseen friends, Fritz Müller was the one for whom he had the strongest regard.'[20] These words enable us to realise the special value and interest of Darwin's letters to Fritz Müller, the noble gift which the British Association has received within the last few months from Prof. Fairfield Osborn.

Many of Fritz Müller's letters on insect mimicry and allied subjects were sent by Darwin to Prof. Meldola, who communicated the observations to the Entomological Society of London,[21] of which he was an Honorary Secretary.

Whenever I have brought some striking example of insect mimicry to Sir Ray Lankester, my dear friend and the friend of many here, his comment was always the same—that it was a convincing proof of evolution by Natural Selection, and that he was unable to understand how any naturalist could come to a different conclusion. And yet, as we know, many have done so and probably many do so still. I hope, therefore, that it may be interesting and perhaps convincing to some unbelievers,

[18] *Essays on Evolution*, p. ix. This work is dedicated to Raphael Meldola.
[19] *Ibid.*, p. 511.
[20] *Life and Letters of Charles Darwin*, London, 1887, vol. iii, p. 37.
[21] Darwin's letters to Meldola, including ten referring to Fritz Müller, are printed in *Charles Darwin and the Theory of Natural Selection*, Poulton, London, 1896. The originals, with many of F. Müller's letters, were presented by Prof. Meldola to the Hope Library, Oxford Univ. Museum.

to reproduce, with the kind permission of the Entomological Society, two plates recently published in the Transactions.[22]

The moth model and butterfly mimic, beautifully illustrated on plate I, really speak for themselves; but it must be explained that the resemblance between the patterns is much closer on the upper surface of the wings than on the under; that the orange patch evidently becomes a conspicuous warning mark (aposeme) in the position of rest when the insects hang with drooping wings and the under side of the body is uppermost; that the position of the mimic's patch on the parts of the wings which cover the body and not on the body itself, as in the model, is evidence of selective elimination guided by the sense of sight; also that model and mimic fly together round the tops of trees, the former being much the commoner. I owe this most interesting example to my kind friend, Dr. Karl Jordan, of the Tring Zoological Museum.

The second example, shown on plate II, is of a very different kind, but I think equally interesting and convincing. The oval yellow masses of silk spun on the outside of their cocoons by the caterpillars of the W. African Bombycid moth, *Norasuma kolga*, closely resemble the cocoons constructed by Braconid parasites which have devoured a larva or pupa. The appearance is, I believe, well known to nearly everyone and is especially common in the autumn, when the dead or dying caterpillars of the Large Garden White butterfly may be seen on walls and fences, bearing the yellow cocoons of the parasitic larvæ which have destroyed them. It has sometimes been thought that the object of the pretended Braconid cocoons is to deceive the female Bracon in her search for caterpillars in which to deposit her eggs, but this is most improbable because these parasites are guided by other delicate senses in addition to sight, which perhaps is not employed for this purpose; above all, because the eggs which are the ultimate cause of parasitic cocoons like the pretended ones, would have been laid far back in the life of the victim. It is probable that the conspicuous yellow colour is advantageous to the parasites, for the small cocoons are very tough and contain but a small amount of food. A few experiments, perhaps a single one, would teach a bird that a cocoon bearing these yellow masses contains only a shrunken skin, and also that the yellow cases themselves are not worth opening. The yellow warning colour is advantageous to the parasites 'because enemies are all the more readily discouraged from making attempts which would incidentally lead to the destruction of some of them. Hence the obvious advantages conferred by false cocoons of parasites when mistaken for real ones.'[23] This interesting adaptation was discovered by my old friend, Dr. W. A. Lamborn, O.B.E., who, a little earlier, had found another example in which the same deceptive resemblance was brought about in a totally different way. The cocoon of another West African moth (*Deilemera antinorii*) he observed to be covered with little yellowish spheres so very like Braconid cocoons that he kept them and watched for the parasites to

[22] *Trans. Ent. Soc. Lond.*, vol. lxxix., 1931, pls. xiv and xv. The cost of reproduction has been borne by the Fund for Promoting the Study of Organic and Social Evolution, presented to Oxford University by my dear friend Prof. James Mark Baldwin.

[23] *Trans. Ent. Soc. Lond.*, vol. lxxix., 1931, p. 397. This paper gives full references to all the observations here referred to in the description of pls. I and II, as well as others necessarily omitted.

O. F. Tassart, pinx. Natural size.

(1) Underside of Model, the Uraniid moth *Alcidis agathyrsus*, with orange patch on abdomen. (2) Underside of Mimic, *Papilio laglaizei*, with two orange marks forming a single patch over the abdomen when the hind wings come together (3).
Both model and mimic fly together in New Guinea.

O. F. Tassart, pinx. Natural size.

Underside of leaf with 8 cocoons of Bombycid moth *Norasuma kolga* (1). Yellow masses, spun on a loose net over the reddish cocoons, resemble cocoons of Braconid parasites. The female moth (2) and males (3, 5, 6) emerged from 4 red cocoons, an Ichneumonid (not Braconid) parasite (4) from another. W. A. Lamborn, Lagos District, 1912.

emerge. It was finally discovered that the 'cocoons' are spheres of hardened froth evacuated by the *Deilemera* caterpillar and then attached with silk to the outside of its cocoon. The late Mr. G. F. Leigh, of Durban, was similarly deceived by an allied East African species and threw away three or four cocoons, thinking they had been parasitised.

What interpretation can be suggested for adaptations such as these, except the selection and accumulation of small variations? And it is to be remembered that even in the mimetic butterfly of Plate I the associated instinct—the attitude assumed at rest—is an essential element in the resemblance, while in the construction of the false cocoons shown on Plate II, the instinctive actions are nearly everything. It is also to be remembered that these actions are prophetic, destined for the protection of a future pupal stage. This fact is so interesting and significant in its bearing on theories of evolution that I venture to bring before you two other especially striking examples, although, of course, prophetic activities are displayed by every caterpillar in spinning its cocoon or otherwise preparing for pupation.

The larva of an African Tabanid fly (*T. biguttatus*) lives and becomes a pupa in mud which, in the dry season, is traversed by cracks so wide that they would often expose the insect in its most helpless stage. But Dr. Lamborn discovered that the maggot has prepared for this danger. It carves out a cylinder from the surrounding mud, making a line of weakness by means of a close spiral tunnel; then it enters just below the top of the cylinder and pupates in its centre. The pupa when mature bores its way through the hard mud covering and the fly emerges. Dr. Lamborn was led to his discovery by observing the tops of the cylinders, of about the size of a penny, often with the pupal shell protruding from the centre; also by noticing that the cracks running in all directions stopped short when they reached the cylinders. I feel sure that you will agree with the words written by Prof. J. M. Baldwin when he read the account of this instinctive behaviour—'As to the discovery of Lamborn, it seems *complete*—one of those rare cases of a single experience being sufficient to establish both a fact and a reason for the fact! It is beautiful.'[24]

The other observation is also of especial interest, being an arresting example of the attainment of the same end by a different and unusual means. In leaving their cocoons some insects gnaw their way out, others make use of holes drilled by pupal spines, as in the last-mentioned Tabanid fly. The well-known 'Puss-moth' (*Dicranura vinula*) has been shown by O. H. Latter to soften the hard cocoon with a secretion of caustic potash. Many caterpillars in spinning their cocoons make special provision for easy emergence and difficult entrance, on the reversed principle of the lobster-pot, a beautiful example being our own 'Emperor Moth' (*Saturnia pavonia*). Now these preparations are made in spinning the cocoon, but the caterpillar of an Indian moth allied to our 'Lappet Moth' first nearly finishes its cocoon and then deliberately bites two slits in it. As Lt.-Col. F. P. Connor[25] has written: 'It was a striking fact to observe how the larva,

[24] *Proc. Ent. Soc. Lond.*, vol. v., 1930, p. 14. Lamborn's discovery is published in *Proc. Roy. Soc.*, B., vol. 106, 1930, p. 83, pl. v. As this address was being written a letter arrived from my friend at Fort Johnston, Nyasaland, telling me that he has just bred another Tabanid fly, at present undetermined, from a mud cylinder like that of *T. biguttatus*.

[25] *Journ. Bombay Nat. Hist. Soc.*, vol. xxvi., 1919, p. 691.

after all but completing the cocoon, always "remembered" to destroy part of its laboriously built home by biting out two deep clefts at one end, and how the valve-like door thus made was patiently tested several times to make certain of its being of the right size, and then carefully closed on the inside with a little soft silk which would not interfere with the emergence of the imago.' In testing the opening the caterpillar extended 'half its body out of the cocoon to assure itself that the vent was large enough.' How is it possible to apply any Lamarckian theory of inherited experience, or of effort and improvement following from experience, to examples like these? The experience of ease or difficulty in emergence in the last example, of failure or success in evading enemies in the others, will come, not in the stage which made the preparation but in a later one, and should it come, the chances of handing on its lessons would be negligible. 'The prime necessity for an insect, as for all animals which cannot in any real sense contend with their foes, is to avoid experience of them altogether.'[26] And the cocoon-making activities described above are preparations, made long beforehand, for the avoidance of experience.

I propose now to refer briefly to some of the objections which have been raised against the opinion that Protective and Mimetic Resemblances have arisen by Natural Selection, and to consider alternative suggestions. Dr. Paul Vignon, in his fine and beautifully illustrated monograph[27] on the leaf-like Long-horned Grasshoppers (Tettigoniidæ) of tropical America, comes to the conclusion that the detailed resemblance to decayed leaves or leaves apparently mined or eaten by caterpillars, is useless, his reason being that other species with the much simpler likeness to uninjured leaves are able to hold their own in the struggle with greater success, as shown by their comparative abundance. Therefore he considers the details as a 'decoration' unnecessary in the life of the insect, agreeing with Brunner's theory of 'Hypertely.'[28] I believe, on the contrary, that the

[26] The arguments in this paragraph were brought forward in the unpublished discussion 'Are Acquired Characters Hereditary?' at the Manchester Meeting, September 5, 1887 (*Report*, p. 755). The later occasions on which they were developed and recorded are mentioned on p. 155, n. 1, of *Essays on Evolution*, where they are reprinted (pp. 117, 118, 154–160).

[27] *Arch. du Mus.*, 6, V, p. 57, 1931. See also his *Introduction à la Biologie Expérimentale. Les êtres organisés. Activités, instincts, structures.* Encyclopédie Biologique, VIII, Paris, 1930.

[28] Prof. J. M. Baldwin has kindly written the following note on a subject (recalled by Dr. Vignon's memoir) we had discussed together :—

'The continued lack of enthusiasm for Natural Selection in France seems at first glance remarkable. It seems inconsistent with the French love of logical "clearness and distinctness" given as the criteria of truth by the French philosopher Descartes, for whom his countrymen have the greatest veneration. But the tendencies shown in the work of Delage and Giard in the last generation appear still in the publications of such thinkers as Le Roy and Brunschweig. Naturally I take no account of special researches of younger biologists with which I am not familiar. The philosophical writers, at least, retain a diluted Lamarckism, somewhat hesitant, it is true, and always on the defensive. It is part of the vitalism expressed by Bergson in the terms "élan vital" and "évolution créatrice." The Positivism of Auguste Comte is still completely demoded, except in the sociological work of Durkheim and Lévy Bruhl, in which the question of the method of biological evolution has no place. The revolt against Bergsonian vitalism in the intellectual world has been directed against its mysticism, but has not extended itself to questions of biology.'

detailed resemblance to one out of many different appearances which the same object may present—*e.g.* to a leaf gnawed into a particular shape by a caterpillar—would often mean safety to a rare, hard-pressed species but great danger to a common one; for the sharp senses of enemies would quickly detect the meaning of that one shape, and then a special search would be made for it.[29] I am sure, however, that everyone will share the author's hopes for further observations on the living insects in their natural surroundings.

On the subject of the Protective Resemblance to leaves I cannot resist the temptation to say a few words about W. J. Kaye's discovery of the part played by the dead-leaf-like under surface of the tropical American butterfly *Protogonius*.[30] The upper side of this butterfly roughly resembles the conspicuous warning pattern of the predominant mimetic association of its locality, changing when the pattern changes as we pass from one area to another—always a mimic although always a poor one. At rest, with folded wings, the resemblance to a dead leaf is perfect. Now Kaye observed that when the open wings of these butterflies were seen from below against the sky the appearance was that of the upper surface, so that at first he thought they must be flying upside down. When, however, he examined them he found that the apparently opaque dead-leaf-like under side was completely overwhelmed by the stronger contrasts of the upper surface. The wings of *Protogonius* were shown in this Section at Liverpool in 1923, when a friend who does not greatly favour an interpretation based on Natural Selection, pointed out rather triumphantly that the dark and the light parts of the two patterns correspond respectively. But this is precisely the kind of result which affords proof of evolution by selection. The two patterns certainly have a common plan, but by stippling here, softening there, and the addition of delicate tints in streaks and washes, the conspicuous, strongly contrasted mimetic pattern of the upper surface is replaced on the under by a beautiful and detailed likeness to a dead leaf.

Before considering the objections to the theory of mimicry it is necessary to devote a little time to Fritz Müller's interpretation of the resemblances which Bates was unable to explain. His difficulty was caused by the remarkably detailed likeness between many species in the two groups which he called *Danaoid* and *Acræoid Heliconidæ*, groups really widely separated and now known respectively as the *Ithomiinæ* and the *Heliconinæ*, both conspicuous and distasteful, and providing models for other butterflies and moths, yet often mimicking each other, the *Heliconinæ* being commonly mimetic, the *Ithomiinæ* rarely. Bates was referring to these resemblances in the following sentence: 'Not only, however, are *Heliconidæ* [viz. both the Danaoid and Acræoid groups] the objects selected for imitation; some of them are themselves the imitators; in other words, they counterfeit each other, and this to a considerable extent.'[31] The theory of mimicry which bears Fritz Müller's name was

[29] *Proc. Ent. Soc. Lond.*, 1924, p. cxlv.

[30] *Ibid.*, 1922, p. xcviii; 1923, p. xxxvii. See also p. xl for Lord Rayleigh's notes on the optical interpretation.

[31] *Ibid.*, p. 507.

suggested by him in 1879.[32] Briefly, the theory rests on the advantage of a combined advertisement in saving lives that would have been lost in the experimental attacks of enemies. Batesian Mimicry, on the other hand, rests on the advantage of a false advertisement, leading a palatable insect to escape because mistaken for a distasteful one. Much controversy has arisen over the mathematical aspect of the problem, but this cannot be considered on the present occasion. I have been led to believe that Müllerian Mimicry is more important than Batesian because models and mimics are so commonly found in the same presumably distasteful group, and because the resemblances which were not explained by Bates' theory are so much commoner than he supposed. Thus the distasteful Heliconine butterflies, among which he recognised mimics of the Ithomiines, are also themselves divided into groups, of which one mimics the other so perfectly that the real difference was for many years unsuspected.[33] And this is equally true but far more striking in the Heliconine mimics of Ithomiines because the patterns are very elaborate, so much so indeed that these mimics are among the most remarkable in the world.

One or two more examples which suggest the prevalence of Müllerian Mimicry may be mentioned. The intricacies of systematics being unnecessary for the appreciation of the argument, I propose to reduce them to a minimum. The 'White Admirals' of the Northern Belt have been separated into different genera, but they are all nearly related with very similar life-histories. They are, except when modified by mimicry, dark butterflies with conspicuous white markings displayed in a sailing flight. In Europe they are mimicked by the female of our 'Purple Emperor' and other butterflies, including a black-and-white invader (*Neptis*) from the south, this latter butterfly belonging to a group which itself provides models for mimicry. Any doubt about the mimetic resemblance of the female Emperor is dispelled when we remember that numerous allies of these Admirals in tropical America (*Adelpha*) are there mimicked by females of butterflies allied to the Emperors (*Chlorippe*). In North America some of the White Admirals possess the black-and-white pattern, one (*astyanax*) is a mimic of a distasteful Swallowtail (*P. philenor*), but at the same time is considered by Scudder to be the model for a female Fritillary. Others are beautiful imitations of Danaine invaders from the Old World, and the mimicry is so recent that one of these (*archippus*) and also *astyanax* can breed with their black-and-white ancestor (*arthemis*) and produce intermediate offspring.[34] There is finally a species (*lorquini*) on the Pacific Coast which is a mimic of a southern invader (*californica*) closely related to the tropical American Adelphas. This last, too, is of especial interest because the mimicry is only developed where the two butterflies

[32] *Kosmos*. The paper was at once translated by Meldola and published in the *Proc. Ent. Soc. Lond.*, 1879, p. xx. A preliminary paper containing everything essential to his theory of mimicry was published by Fritz Müller in *Zool. Anzeiger* (Carus), I (1878), pp. 54, 55. Translation by E. A. Elliott in *Proc. Ent. Soc. Lond.*, 1915, p. xxii.

[33] W. J. Kaye in *Proc. Ent. Soc. Lond.*, 1907, p. xiv.; H. Eltringham in *Ibid.*, *Trans.*, 1916, p. 101, pls. XI–XVII.

[34] *Proc. Ent. Soc. Lond.*, 1916, p. xciv. Abstract of W. L. W. Field's three valuable papers in *Psyche*.

overlap, and dies away to the north and east where *lorquini* spreads beyond the range of its model.[35]

We have seen that the Adelphas of the New World are models, but the corresponding African representatives of the White Admirals, the Pseudacræas, are, with one or two exceptions, mimics, resembling certain Acræine butterflies conspicuous in their localities, and in two instances Danaines, one of the models being *D. chrysippus*.

These tangled relationships of models and mimics in the great group of 'White Admirals' and their allies are in my opinion impossible to reconcile with the Batesian theory, but in every way consistent with the Müllerian. It will be necessary to return to the African Pseudacræas a little later, but I will first mention one more example which, I believe, supports the same conclusion.

During the meeting of the Association at Toronto in 1897, I met Dr. Gustav Gilson of Brussels who was about to visit Fiji and very kindly promised to collect butterflies for me. Among the specimens received were two species of *Euplœa*, one of which had obviously been modified in mimicry of the other. Now the Euplœas are among the most distasteful and most commonly mimicked butterflies in the world, and I became extremely anxious to obtain more specimens from different islands of the Fijian and other groups. Finally, after waiting more than twenty years I received a very kind letter from Mr. Hubert W. Simmonds, who had heard of my wants, which he then proceeded to supply most generously and efficiently, enabling me to study this and other equally interesting problems. There is not now the possibility of describing the results,[36] but I will mention, as bearing on the Müllerian theory, that the mimicking *Euplœa* of Fiji is found to be a model on Wallis Island, and the model of Fiji its mimic; while on Fotuna Island, 150 miles away, the Wallis model is absent, while the other *Euplœa* is present, but unmodified by mimicry.

The year before Fritz Müller proposed his theory of mimicry in 1878, he published a paper which was probably the preparation for it—the paper in which he explained the meaning of the gregarious habit in certain distasteful insects. Thus, writing of the dull brown caterpillars of two American butterflies, he suggested that the social habits 'which lead them to congregate in large numbers make up for their want of colour, since their offensive odour then gives timely warning to an approaching enemy.'[37] This interpretation has recently been adopted for the interesting and hitherto puzzling habits of *Heliconius charithonia*, which

[35] *Trans. Ent. Soc. Lond.*, 1908, p. 447. *Lorquini* and its model are represented on pl. XXV, which also shows a reciprocal approach of the latter towards its mimic. Owing to the kindness of Commander C. M. Dammers I have been provided with Mimicry in the N. American 'White Admirals,' here very briefly summarised, is the opportunity of renewing this investigation with far more extensive material. considered in detail in the above paper and in *Proc. Acad. Nat. Sci. Philadelphia*, January 1914, p. 161.

[36] A full description appears in *Trans. Ent. Soc. Lond.*, 1923, p. 564, pls. XXIX–LIII.

[37] *Kosmos*, December 1877. Translation by Prof. R. Meldola in *Proc. Ent. Soc. Lond.*, 1878, pp. vi., vii.

collects into crowded groups on bare twigs in the evening, as was first recorded by Philip Gosse in Jamaica in 1851 and since then by numerous observers. *H. charithonia*, which belongs to the distasteful Heliconines referred to on pp. 16, 17, and is itself mimicked by other butterflies,[38] has been carefully studied by Dr. F. M. Jones, and its gregarious habits described in detail in his paper 'The Sleeping Heliconias of Florida.'[39] He here suggests that the warning characters may be rendered more effective at night 'by the close proximity of large numbers, under these conditions readily recognisable by form, colour, or *scent*, as identical in kind and inedible; for thus the injury or destruction of one of the group might conceivably work for the protection of the many.' It may be added that the choice of leafless twigs for a resting-place obviously enhances the conspicuousness of the assemblage.

We must now return to one of the African Pseudacræas, a wide-ranging species (the Linnean *eurytus*) which subdivides into a number of local forms mimicking the local Acræine models. This species is represented in Uganda by a race (*hobleyi*) so significant in its bearing on evolution by selection that it is necessary to give a little time to it. *Eurytus hobleyi* appears in three forms—two, with male and female alike, mimicking two Acræine butterflies (*Planema*) differing in colour but also with male and female alike. The third, with male and female very different, mimics a third *Planema*, the sexes resembling the corresponding sexes of the model. Now these four mimetic forms—for the male and female of the last were believed to be of different species—have all been described and named as distinct, and there was great astonishment and even some incredulity when Dr. Karl Jordan, relying on structural features, pronounced them to be one. After many efforts to test this conclusion by breeding, a cable was received from Dr. Hale Carpenter on Bugalla Island (N.W. Victoria Nyanza), giving the information which proved that Dr. Jordan was right.

Many other families were then bred by Dr. Carpenter, and these, with his captured specimens, showed that, in the islands, the three forms run into each other, being connected by an abundance of transitional varieties which are extremely rare on the adjacent mainland of Uganda. The significance of this is obvious when it is realised that the models are for some unknown reason comparatively scarce on the islands.[40]

The same conclusion is enforced by the wonderful families of *Papilio dardanus*, bred by Dr. V. G. L. van Someren and Canon K. St. Aubyn Rogers from localities near Nairobi. Now in the families of this butterfly that have been bred in other parts of Africa—by Carpenter in Uganda, by Lamborn on the W. and E. coasts, by Swynnerton in S.E. Rhodesia, and by Leigh in Natal, the mimetic forms of the females are sharply separated—a fact which led to the mistaken conclusion that these patterns appeared fully formed and complete, each as a single variation. But in the Nairobi families, as in the Pseudacræas of the Uganda islands, all kinds of transi-

38 W. J. Kaye in *Proc. Ent. Soc. Lond.*, vol. v., 1930, p. 89.

39 'Natural History,' *Journ. American Mus. Nat. Hist.*, vol. xxx., p. 635. A full abstract, with references to other observations, in *Proc. Ent. Soc. Lond.*, vol. vi., 1931, p. 4.

40 *Trans. Ent. Soc. Lond.*, 1912, p. 706; 1913, p. 606; 1920, p. 84; 1923, p. 469.

tional forms appear and, most striking of all, the *trophonius* female mimicking the Danaine *D. chrysippus* has not been bred but only its primitive ancestor *lamborni*, and this has appeared often, although very rare in other localities. Here, too, the same explanation holds, for Dr. van Someren and Canon Rogers have observed that for some cause, perhaps the elevation, the Danaine models are much scarcer than their mimics, and cannot be supposed to influence the selective elimination as in other parts.

These two striking examples offer, I believe, convincing evidence of the power of selection in the evolution and preservation of mimetic patterns; also that the evolution was by small variational steps. The remarkable families of *Hypolimnas bolina*, bred by Mr. H. W. Simmonds in Fiji, supply further evidence in favour of this last conclusion.[41]

Admitting, as claimed and, I believe, proved above, that selection is essential for the evolution of mimicry, nevertheless the abundance of mimetic forms when their models are rare, and still more when they are absent altogether, does make it difficult to feel confident that *Natural* Selection, in its accepted sense of survival of the fittest, has always been the cause. This doubt was first raised in my mind by the consideration of the Oriental butterfly, *Papilio polytes*, and led to the belief that in this and probably other predominant species the absence of the model finally leads to the disappearance of the mimetic pattern, 'although the species that bore it remains as abundant as before. The survival or extinction of the species is not affected: all that has happened is the survival or extinction of a pattern borne by a certain proportion of the individuals of the species. When these disappear, other individuals with another pattern take their place.'[42] For this process Prof. Julian Huxley has suggested the term 'Intraspecific Selection,' to be contrasted with Natural Selection which ensures the survival of the species in its organic environment and, therefore, in a struggle which is *inter*specific. Mr. A. J. Nicholson[43] has independently proposed a similar hypothesis but seeks to carry it much further, so as to cover all examples of Mimicry and Protective Resemblance. My reasons for disagreeing with this opinion are given in the above-mentioned paper on Intraspecific Selection.

Certain criticisms which have been brought against the theory of mimicry have followed from the erroneous assumption that the Warning Colours of the models imply complete immunity from attack, even by parasites, an assumption unfortunately made by Haase in his important and valuable work.[44] Of course no species enjoys absolute immunity, and if it did so the enjoyment would be brief, for it would rapidly destroy its own means of existence. Furthermore we know, as my friend Dr. Hale

[41] *Trans. Ent. Soc. Lond.*, 1923, pls. XLV–LIII.

[42] Poulton in *Proc. Zool. Soc. Lond.*, 1928, p. 1037. The term 'Intraspecific Selection' was introduced in this paper, which also quotes the essential passages from the paper (*Bedrock*, vol. ii., No. 3, October 1913, p. 295) in which the hypothesis was first suggested.

[43] 'A New Theory of Mimicry in Insects,' *Australian Zoologist*, vol. v., pt. 1, November 1927, p. 10, pls. I–XIV.

[44] *Untersuchungen über die Mimicry*, Stuttgart, 1891–3.

Carpenter showed to this Section at Birmingham in 1913, that the species distasteful to insectivorous animals (although not by any means entirely free from this danger) are *specially subject* to parasitic attack. At the same city, in 1886, I brought before this Section the theory of a compensating principle[45] which would check the increase of distasteful insects; for when other food became scarce they too would be devoured, and then their conspicuous appearance and slow movements would lead to their easy capture. This theory was supported by experiments which proved that insectivorous animals, when they are sufficiently hungry, will in fact eat the distasteful species, although often with signs of disgust. The experimental method, necessarily employed in testing the above-mentioned hypothesis, and also of much value when other evidence is wanting, was criticised by W. L. McAtee in a paper published in 1912.[46] I was probably mistaken in not at once writing a detailed reply to these criticisms, which were not only directed against the conclusions drawn from experimental feeding, but also against other conclusions on which the theory of mimicry is founded. On the other hand, there was much to be said for waiting until far more evidence had been collected, and now, after nearly twenty years, it may be fairly maintained that such evidence has been forthcoming.

In the first place it may be granted that, apart from its special value as a test, the experimental method is, in this investigation, very inferior to the direct observation of attacks made upon insects by birds and other enemies in their natural surroundings and undisturbed. It is impossible on this occasion to attempt to give any account of the great number of such records which have accumulated since the appearance of McAtee's criticisms. I will, however, mention two sets of observations. In 1927 Dr. Hale Carpenter kindly sent me the wings of Uganda hawkmoths—twenty-one specimens and seven species—found on the floor of a rest-house where they had been dropped by bats hanging in the roof. This interesting observation suggested an examination of moths' wings dropped by British bats—an ideal means for discovering their true preferences. Wings representing 1,328 moths were collected in sheltered places frequented by bats—probably always by the Long-eared Bat (*Plecotus auritus*). All the specimens except sixteen belonged to species with protective (Procryptic) colours and habits. The exceptions included relatively conspicuous species shown by experiments on other animals to be rather distasteful (sometimes accepted, sometimes refused), also species of which the palatability is unknown. Not a single specimen with a striking warning pattern was present.[47]

[45] Considered in detail in a paper published in the following year: *Proc. Zool. Soc. Lond.*, 1887, p. 191.

[46] 'The experimental method of testing the efficiency of warning and cryptic coloration in protecting animals from their enemies.' *Proc. Acad. Nat. Sci. Philadelphia*, 1912, pp. 281–364.

[47] *Proc. Zool. Soc. Lond.*, Pt. 2, 1929, p. 277. The interesting plates I–III in *Proc. Ent. Soc. Lond.*, vol. vi., 1931, provide evidence of the same value as that furnished by the rejected wings. They show young cuckoos being fed by fosterers with 'Small Garden White' butterflies, in Sussex, photographed by Mr. H. F. Chittenden, and Cumberland, by Mr. A. G. Britten. Dr. J. G. Myers' observations on the insect food of the Coati (*Nasua*) were also in large part made under natural conditions (*Ibid.*, vol. v., 1930, p. 69). See also Capt. C. R. S. Pitman's experiments on an African Lemur (*Perodicticus*) on p. 91, and in vol. iv., 1929, p. 90.

The second series of observations is now being undertaken at Vineyard Haven, Massachusetts, by Dr. Frank Morton Jones, who has kindly written to me, explaining the details of the excellent methods he is employing. Insects, chiefly Lepidoptera and Coleoptera, of known species are exposed on a feeding-tray in a favourable locality and the visits of birds watched at a distance through field-glasses. Thus on June 27 last, of 63 beetles belonging to 9 species placed on the tray, there remained in 30 minutes, after 22 bird-visits (3 species), 15 beetles of one red-and-black species. Thus 48 beetles of 8 species were taken and *all* the 15 of the ninth species were untouched. Dr. F. M. Jones is also attempting to form a scale of distastefulness by observing the reactions of a common species of ant to the insects employed in the experiments.

One of the chief criticisms made by McAtee and made in this country also, was the insufficiency of the evidence that butterflies are commonly attacked by birds, the enemies believed to be the selective agents in the evolution of mimicry. McAtee, in support of this objection, quoted the results of an American agricultural investigation in which an enormous number of birds' stomachs had been examined and remains of butterflies found in only an insignificant proportion. This criticism had been in great part met beforehand in a paper[48] published by Sir Guy Marshall in 1909; and more recently C. F. M. Swynnerton[49] and W. A. Lamborn[50] have conclusively shown that butterflies are rapidly reduced to such minute fragments in a bird's digestive tract that examination with the compound microscope is necessary in order to obtain trustworthy results. Furthermore, it is only in recent years that the imprint of a bird's beak on a butterfly's wing has been noticed ; but now that attention has been directed to this evidence it is found to be quite common—a good example of the fertile but, for the uncritical, the dangerous principle that an observer only finds what he looks for.

It is possible that the mistaken assumption of the immunity of models has played a part in prompting Dr. Bequaert's interesting paper on the enemies of ants.[51] Admitting the existence of these enemies and the certainty that the list will be immensely lengthened, it still remains that ants are ' the most powerful of insects, ever-present and aggressive in all habitable parts of the earth.'[52] And it is difficult to reconcile with Dr. Bequaert's opinion that they are valueless as models, the fact that my friend Mr. H. St. J. K. Donisthorpe has, since 1891, discovered, in the nests of British ants, ' 204 species of insects, spiders and mites new to the country, including 74 new to science. Of these guests 28 are mimics of ants. . . . He has also recorded 34 mimics living independently of ants.'[53] I believe that most naturalists will conclude from these discoveries in Great Britain and Ireland, and from the remarkable profusion of ant-mimics and ant-

[48] *Trans. Ent. Soc. Lond.*, 1909, p. 329. See especially pp. 336, 337.

[49] *Linn. Soc. Journ. Zool.*, xxiii., 1919, p. 203. Abstract in *Proc. Ent. Soc. Lond.*, 1915, p. xxxii.

[50] *Proc. Ent. Soc. Lond.*, 1920, p. xxvi.

[51] *Bull. Am. Mus. Nat. Hist.*, vol. xlv., p. 271, New York, 1922.

[52] *Zoolog. Anz.* (Wasmann-Festband), 1929, p. 86.

[53] *Ibid.*, p. 84. Quoted from ' Guests of British Ants,' Donisthorpe, London, 1927. The numbers have been brought up to date with the kind help of the author.

associates in the tropics, that in spite of all attacks, these insects possess in the highest degree the qualities which render them valuable as models.

I now propose to direct your attention to certain experiments and observations which throw light on the brain and senses of Vertebrate enemies of insects. Although the experiments brought before this Section at Manchester in 1887 were few and single, I believed, and I still believe, they were crucial, and proved beyond doubt that the mind and memory of even a Reptilian enemy—and of course far more probably an Avian enemy—are such as we should expect to find in the selective agents which have brought about the evolution of mimicry in insects. I refer to the chameleon, which, after rejecting a bee which it had captured the moment after its introduction into the cage—after this single experience—would never touch another, although offered from time to time during many months ; also to the lizard, which approached a hornet-like Clearwing Moth with the utmost circumspection, and in finally seizing it kept as far as possible away from the supposed sting, and then, evidently realising from the texture that the insect was not a wasp or hornet, proceeded to eat it without further caution, and a few days later recognised another at sight and instantly devoured it.

It is not to be hoped that these experiments will carry the same conviction to those who only hear of the results and did not see them ; but in recent years other evidence throwing much light on the faculties and behaviour of birds has been steadily accumulating.[54]

The conclusions of the distinguished ornithologists, E. C. Stuart Baker and Rev. F. C. R. Jourdain, that the resemblance of Cuckoos' eggs to those of the fosterers has been evolved through the selective destruction of the less like by the birds which would otherwise have been victimised, obviously bears closely on the development of a mimetic pattern in insects. The similarity between the two selective processes, both leading to a superficial likeness which changes with the geographical changes of the models, was made the subject of the Address to the Entomological Society in January 1926, and led to the last words addressed to me, although indirectly, by William Bateson, the distinguished ex-President of this Section and of the Association, whose loss we all deplore. Not many days before his death he was present at the meeting and told a mutual friend that he was much interested in the observations and that they were quite new to him.

Evidence of a different kind, but probably very significant, is provided by the well-known African Honey Guide (*Indicator*) which directs man to a bee's nest and is repaid by a meal on the scattered larvæ. My friend, Dr. Neave, has told me that this bird, when insufficient attention is paid to its directions, becomes so noisy that game is disturbed, and he found it necessary, on hunting expeditions, to detail a couple of natives to follow the Guide and keep it quiet. How far the behaviour of the bird is instinctive and how far intelligent is, I believe, unknown, but it is impossible to imagine a more fascinating subject for investigation.

[54] Nearly all these observations are recorded or quoted with full references in the Proceedings of the Entomological Society of London in recent years, and will be easily traced.

Of more importance, because common to many species and known to exist in all the great tropical areas, is the nesting association between birds and the most formidable of insects—wasps and hornets; also with ants and termites. In the association with wasps naturalists have definitely stated that the birds begin to build close to the already constructed pendent comb of a wasp, while their nests are actually excavated in the termite-mounds and in the huge nests of tree-ants. This most interesting and significant behaviour has been summarised for tropical America by J. G. Myers, who has confirmed the older records by his own observations and has also been led to the startling conclusion that at least one wasp tends regularly to make its nest beside the colonies of a tree-ant (*Azteca*). Notes on these associations in Africa have been written by A. Loveridge and V. G. L. van Someren; in India by E. C. Stuart Baker; in Australia by W. B. Alexander.[55]

The behaviour briefly described in the last two paragraphs proves, I believe, that birds possess a brain and sense-organs such as would lead them, in seeking their food, to associate the qualities, favourable or unfavourable, with the appearance, and to remember and apply their experience, in fact precisely the powers required by a selective agent in building up a mimetic pattern.

To the above evidence may be added two examples of bird behaviour in our own country. The cocoon of the common 'Lackey Moth' is thick on the exposed surface but thin where it is spun on to a leaf. Birds have discovered this and peck a hole through the leaf and thin wall in order to abstract the chrysalis.[56] Many naturalists have observed that birds, although they frequently peck their way into the centre of 'bullet-galls' (often but erroneously called 'oak-apples'), never do so when the enclosed insect has emerged, being doubtless guided by the sight of the small round hole or by tapping with the bill.[57]

What other hypotheses have been suggested by those who reject evolution by Natural Selection as the explanation of mimicry and allied adaptations? Some naturalists believe that the resemblances in question are accidental and of no biological significance. This opinion, although defended by such an eminent entomologist as Prof. Handlirsch,[58] is not likely to be held by anyone who has seriously considered examples such as those brought before you to-day, or has studied the geographical distribution of mimetic associations. Chance resemblances are, of course, bound to occur among the immense number of butterfly patterns throughout the world, but these will be as frequently found between the species of different countries as between those of the same country. Such truly chance likenesses in patterns have been examined by my friend, Dr. F. A. Dixey,[59] and have been shown to be relatively few and only to exist at all

[55] *Proc. Ent. Soc. Lond.*, vol. iv., 1929, p. 80 (America); p. 88 (Africa); p. 89 (India); vol. v., 1930, p. 111 (Australia); vol. vi., 1931, p. 34 (India).

[56] Observed by A. H. Hamm. *Ibid.*, 1902, p. xv.

[57] *Ibid.*, vol. iii., 1928, p. 50; vol. iv., 1929, p. 10.

[58] *Handbuch der Entomologie.*

[59] *Proc. Ent. Soc. Lond.*, 1913, p. lx. As regards chance likeness in form, Bates wrote in his great paper (p. 514 n.): 'Some orders of insects contain an almost infinite variety of forms, and it will not be wonderful, therefore, if species here and

when the patterns are relatively simple. The distinguished mathematician, the late Prof. Study, of Bonn, who was deeply interested in mimicry, has shown, in two of his last papers, the impossibility of an explanation based upon chance resemblance, and I believe that the same conclusion will be reached by anyone who reads the chapter on Mimicry in Dr. R. A. Fisher's recent work.

The view has sometimes been held that mimetic resemblances are due to model and mimic independently passing through the same stage of evolution, either as a whole or in the mimetic features only; or, as Darwin once suggested, 'that the process probably commenced long ago between forms not widely dissimilar in colour'.[60] I remember, at the Leeds Meeting in 1890, when Prof. Patrick Geddes suggested the former interpretation, that the late Lord Rayleigh remarked, 'How would you apply your explanation to the resemblance of insects to bark, or twigs, or leaves?'[61] It is strange that this fatal objection did not occur to Darwin, for Bates himself in the great paper had written: 'I believe . . . that the specific mimetic analogies exhibited in connexion with the *Heliconidæ* are adaptations—phenomena of precisely the same nature as those in which insects and other beings are assimilated in superficial appearance to the vegetable or inorganic substance on which, or amongst which, they live. The likeness of a Beetle or a Lizard to the bark of the tree on which it crawls cannot be explained as an identical result produced by a common cause acting on the tree and the animal.'[62]

Before concluding, a few lines must be devoted to recent work on Sexual Selection, first briefly introduced as a factor in evolution by Darwin in the Joint Essay. Nothing would have interested and pleased him more than discoveries which, following the splendid pioneer work of Fritz Müller, have been made in the epigamic structures and behaviour of insects—the extensive observations on the scents of male butterflies, by Dixey and Longstaff, and on their scent-scales by Dixey; the structure and use in courtship of the scent-brushes of male Danaine butterflies, by Eltringham, Lamborn, and Hale Carpenter; the extraordinary brushes protruded from the back of the head by the males of *Hydroptila* (Trichoptera), by M. E. Mosely and Eltringham; the courtship of Empid flies, including the spinning of a cocoon as a wedding gift by the male *Hilara*, by Hamm and Eltringham;[63] the fertilisation of orchids (*Ophrys*) by male bees (*Andrena*) which, emerging before the other sex, are attracted by female-like appearances, and probably scent, of the flowers, by Pouyanne, confirmed by M. J. Godfery, and by Mrs. Coleman, who has observed the

there be found to resemble each other, although inhabiting opposite parts of the earth, and belonging to widely different families. Such analogies are accidental, and can have nothing at all to do with the evidently intentional system of resemblances, carried on from place to place, which I have discussed.'

60 *Essays on Evolution*, p. 233 n.

61 *Proc. Ent. Soc. Lond.*, 1925–26, p. xcv.

62 *Ibid.*, p. 508.

63 *Ent. Monthly Mag.*, 1913, p. 177; *Proc. Roy. Soc.*, B., vol. cii., 1928, p. 327. All the other observations are recorded, with full references to earlier publications, in the *Trans.* or *Proc. Ent. Soc. Lond.*

fertilisation of an Australian Orchid (*Cryptostylis*) by a male Ichneumonid (*Lissopimpla*), similarly attracted to the flower.

On two occasions I have been present when the late Lord Balfour expressed his opinion on the theories of evolution we have been considering to-day, and I am sure that naturalists will be glad to hear the conclusions reached by his keen and penetrating intellect on subjects in which, although without the time, or indeed the inclination, to probe far into details, he took the keenest interest. We know that, even before he went to Cambridge in 1866, he had read and admired the *Origin*, and we have been told by his nephew, Lord Rayleigh, of his ' extraordinary faculty for getting hold of the essentials of a subject without apparently feeling the need for systematic study.'[64]

Over forty years ago, when the results of Weismann's researches were extinguishing the Lamarckian element which had been added to the Darwin-Wallace theory, I heard Lord Balfour say that to him, as a student of philosophy, the new teachings on the scope of heredity were more interesting than the old. Again, in 1927, a few months before his eightieth birthday and before he began to dictate the charming but too brief *Chapters of an Autobiography*, he said that, looking back, he was impressed by the fact that nothing suggested in later years had replaced or modified the Darwinian theory of evolution.

And now in conclusion, speaking at the close of the second half-century of our Society's life, and speaking as one who owes more than he can express to the kindness and help received at these meetings, I cannot do better than remind you of prophetic words spoken at Oxford in 1832. Prof. Adam Sedgwick, responding after his nomination as president at Cambridge in the following year, said that the work of the Association at the meeting which had just been held could not but tend ' to engender mutual friendship, mutual forbearance, mutual kindness and confidence ' ; and, for the future, ' he looked forward with full assurance to the happy results of this union between men of similar sentiments and similar pursuits, who possess one common object—the improvement of mankind by the promotion of truth.'

[64] *Proc. Roy. Soc.*, B., vol. 107, 1930, p. viii.

SECTION E.—GEOGRAPHY.

THE HUMAN HABITAT.

ADDRESS BY

THE RIGHT HON. SIR HALFORD J. MACKINDER, P.C.

PRESIDENT OF THE SECTION.

In this Centenary Year it would be natural that a Sectional President should look back along the path that has been travelled since the time of William the Fourth. In the case of the Geographical Section, however, our history has been intertwined with that of the Royal Geographical Society, and it was obviously appropriate that the distinguished historian of that Society, Dr. Hugh Robert Mill, should complete his work by adding to it the annals of the Section. That task, at my invitation, he fulfilled for us yesterday with characteristic humour. My duty, therefore, lies in another direction : I have a Jubilee to celebrate.

About half a century has elapsed since the Council of the Royal Geographical Society came gradually and with some controversy to the conclusion that if it would succeed in reforming geographical education it must transfer its attention from the Schools to the Universities. It was at Oxford, our Senior University, that a beginning was made. My memory goes back to my first lecture as University Reader there forty-four years ago. I had an audience of three, one man and two women. The man was a Don who told me he knew the geography of Switzerland, for he had just read Baedeker through from cover to cover. The two ladies brought their knitting. The University of Oxford is now at long last to complete the work of my successors by electing a Professor of the subject with full status and emoluments. In the interval every University in the country has set going the teaching of Geography. Thus, the decision to establish the Oxford Chair comes as the endorsement of a general movement and crowns a national development. Oxford is the right place for that crowning. Richard Hakluyt, the Elizabethan, was the first Oxford Reader in Geography ; I was only the second. Is it too late to suggest that the new Chair should be called the Hakluyt Professorship ? In the moment of triumph that would be a graceful gesture towards our elder sister, History.

At the same time that Oxford decided to establish the new Professorship, it was announced that in due course an Honour School in Geography would be set up. Inevitably this will open once more the question of the content of the subject, for I do not imagine that Oxford will be satisfied by merely following alien models. Those of us who look back to the beginnings of the movement will remember the barren and wearisome discussions which turned round the attempt to obtain agreement on a formal definition of Geography. Probably none of us are to-day much

interested in the definition and delimitation of the Sciences. The most notable of recent advances have been made along their frontiers and, indeed, in the interstices between them. But a curriculum which is to guide the studies of Honour Students should be penetrated, although not defined, by some principle of unity. Such a thread of unity is, it seems to me, wanting in some of our existing curricula.

The change which has come about in the higher study and teaching of geography is not wholly due to the initiative of learned societies and universities; in no small degree it is in response to a public demand. Historical studies have, perhaps, unduly biased our tradition of a liberal education, with the result that the business world has doubted the executive value of University-trained men. The Universities are now sending some of their best students into general business, and not only as technical experts. Such men must be guided to philosophical powers of thought and expression without losing their grip on actuality. Prestige has long clung to the study of Humane Letters, to the detached exercise of the intellect on the languages, history, and thought of past centuries. No cultured Englishman will undervalue history. Have we not a law based on precedents and an unwritten constitution? Is not respect for tradition the very foundation of our national character? But it is the modern thesis that space and time are a continuum; our ancestors said the same thing when they spoke of God as both Almighty and Everlasting. Who shall say which is the higher of the divine attributes—the Almighty or the Everlasting? To descend to the mundane—why should you not find philosophy in Geography as well as in History?

Dr. Mill has told us how that a Section of this Association was founded in the 'thirties of last century for geology and geography, how that its scope was subsequently narrowed to geology and physical geography, and how that in the 'fifties a new Section had to be formed for what remained of geography. That is the terse history of a skilful feat of diplomacy, or in plainer language of a theft. It was not until the 'eighties that under the lead of such men as Francis Galton, James Bryce, and Douglas Freshfield our Alsace was reconquered, with what example and help from other countries I must not now stop to detail. The worst of it was that not a few geographers had become content with the limited boundaries within which they had been driven; I well remember the *obbligato* of seafaring language which came *sotto voce* from a worthy admiral, a member of the Council of the Royal Geographical Society, who sat in the front row of my audience when in 1887 as a young revolutionary I read my paper on the Scope and Methods of Geography. At Oxford, in the later 'nineties, Herbertson and I pressed what we described as the regional, the synthetic idea. Herbertson published his paper on the Major Natural Regions, and I edited my Regions of the World Series. In the first decade of the present century there was a reaction, and curricula were drafted which tended once more to break the subject up into a group of fragments from other sciences. Since the War, the earlier trend has happily, as I think, won a fresh impetus.

In asking for a principle which shall give unity of purpose to geographers, let it not be thought that by special pleading I am seeking for some idea which shall suffice to justify the institution of a geographical discipline

by bracketing together, formally and superficially, really incongruous elements. A house that is builded upon the sands cannot stand. But we all feel, we who have the love of maps, that somewhere in geography there is a fundamental unity which eludes us. Is not our difficulty how to weld together the geological and the human aspects of the subject? To have stated your problem is to have gone a long way towards solving it. Is it not perhaps the lure of geomorphology which has been misleading us? I am not prepared to go quite all the way with Prof. Douglas Johnson of Columbia University, who would wholly exclude geomorphology from geography, but I am ready to regard it as a secondary and not the primary factor. Geomorphology, as it has now developed, has internal coherence and a consistent philosophy, and in their hunger for these joys many of our geographers, it seems to me, have blinded themselves to the fact that as geomorphologists they are not in the centre but on the margin of geography. I had almost said that in escaping from servitude they had robbed the Egyptians, the geologists, and had been cursed for the possession of ill-gotten goods by a generation spent in the wilderness.

What is it that really gives a unique interest to the surface of this earth? Surely not its dead features; there are mountains also on the moon, ruins from a live past. Is it not the fluid envelopes, the water and the air, which by their circulations, their physical and chemical reactions, and their relation to life, impart to the earth's surface an activity almost akin to life itself? Which is the fundamental—the living, palpitating being or the dead skeleton which it shapes and leaves behind as a monument? Which is the prior—function or form? I admit that in my earlier writings I myself went often astray, attracted by the antithesis which Archibald Geikie drew in his Textbook of Geology between the laying down of the rocks and the shaping from those rocks of the existing surface. It seemed that the former was geology and the latter geography. It seems to me to-day that it is in the water rather than in the rocks that we must look for our salvation.

Let me here interpose an idea which I accept from the astronomers. I quote from Jeans in his 'Universe Around Us,' but have taken the liberty of slightly rearranging the order of the selected sentences so as to present an epitome of the argument while retaining authoritative wording:—

> 'The old view that every point of light in the sky represented a possible home for life is quite foreign to modern astronomy. By far the greater part of the matter of the Universe is at a temperature of millions of degrees. There can be no life where atoms change their make-up millions of times a second and no pair of atoms can ever stay joined together, for the very concept of life implies duration in time. It also implies a certain mobility in space, and these two implications restrict life to the small range of physical conditions in which the liquid state is possible. We know of no type of astronomical body in which the conditions can be favourable to life except planets like our own revolving round a sun. Of the rare plantary systems in the sky, many must be entirely lifeless, and in others life, if it exists at all, is probably limited to a few planets. If life is to obtain a footing, the planets must not be too hot or too cold. We cannot

imagine life existing on Mercury or on Neptune; liquids boil on the former and freeze hard on the latter. Millions of millions of stars exist which support no life. At the best, life must be limited to a tiny fraction of the Universe.'

In the face of such utterances I think that geographers should be rid of that inferiority complex which the historians on the one hand, and the geologists on the other, have too frequently managed to impose upon them. *A planet with a hydrosphere is a unique object of study.* This earth is important not merely subjectively and because we men who inhabit it are the students; in the university of the impartial angels it would be an object of intense interest and speculation. May we not compare it with radium among the elements? Just as radium, almost infinitesimal in quantity, has by its activity revealed the energy locked up in the commoner atoms, so may not the hydrosphere of our earth present the infinitesimally rare conditions under which life becomes active which elsewhere through the Universe is immanent, but potential and not active?

Three years ago, in an address to the International Geographical Congress at Cambridge, I described the hydrosphere in words which I will ask your leave to use again:—

'. . . the hydrosphere, a term invented to cover the totality of water on the earth whether gathered together in the ocean, or invisible in the air, or condensed in the clouds, or falling as rain or snow, or creeping down in the glaciers, or coursing down in the rivers, or percolating underground, or rising in the sap of plants or circulating in the arteries and veins of animals. There is probably no complete lacuna in the hydrosphere, though there are thin places over and in the deserts. It would be a vast bubble, if we imagine all else dissolved away. Moreover, the hydrosphere is functionally one, for given sufficient time and every drop might successively take the place of every other drop, passing from the ocean back into the ocean. Obviously life, and not least human life, is possible only within the bounds of the hydrosphere. In purely physical geography nine-tenths of the processes investigated are dependent on the physical properties of water. It is a remarkable fact that within the short range of temperatures on the earth's surface lies the whole gamut of the changes of state in water, with all the consequences which flow from a high specific heat and the liberation and absorption of great latent heats. The atmosphere exhibits climatic contrasts chiefly by reason of its contained moisture. Propelled by the sun's energy from without, water is the chief sculptor of the solid forms upheaved by the earth's energy from within. Without water there would be no agriculture, nor would coal and iron have been deposited for our mines. Even man must earn his living by the sweat of his brow.'

Let me hasten to add that in this advocacy of the claims of the hydrosphere to be considered as the central theme of geography, I am not forgetful of the just rights of the lithosphere, which has of late been dominant in our geographical argument; nor yet do I forget the part played by the winds and the oxygen of the atmosphere. We will, if you like, define the object of geographical study as extending to those parts

of the lithosphere and of the atmosphere which are interpenetrated by the hydrosphere. It is obvious that a dry lithosphere and a dry atmosphere would present a landscape of merely naked rocks, not even rusted, and seas of sterile sand with perhaps endless lines of wind-driven dune. When we reflect that the millions of stars 'have surface temperatures of anything from 1650° to 30,000°' (Jeans), what an astonishing thing it is that through the long geological ages the surface of our earth has enjoyed that short range of temperatures which will permit of the liquidity of water.

I turn to biogeography, for it is there that our difficulty lies. The fundamental unit of biogeography is the natural region. Apart from the idea of habitat or environment the natural region would have no meaning. What gives to it significance is the fact that, whether you accept the inheritance of acquired characteristics, or believe in natural selection alone, species originate as local varieties. The African elephant is no doubt a single interbreeding species, but the elephant of the western tropical forests is of a different variety from the elephant of the eastern uplands. The same is no doubt true of the mountain and the forest gorillas. The same again is the case to an almost infinite extent with the varieties of the many species of butterfly. Now the basis of the fixation of a local variety into a species is that the variety shall consist not of so many individuals but of a single blood. Forgive me if for convenience I speak in what follows of blood instead of protoplasm.

Let us go for an example to the head of the list: to Man himself. John Bull is a local variety of the genus and species, *homo sapiens*. There are to-day some forty millions of us inhabiting a natural region which we may describe as the English Plain, for there are few who live much over 600 feet above the sea. The fact that each of us is the offspring of two parents results in this: that if you go back to the time of the Norman Conquest each of us now living had more than sixty million ancestors. But in the England of that time was a population of only two millions. If we assume a fluid marriage market, then *each* of us living would be descended from *each* of the two million people of the time of the Conqueror by thirty different pedigrees. The assumption of fluidity in marriage opportunities is, of course, not quite true, but although in the past Lincolnshire may rarely have married Hampshire, yet Lincolnshire married Northamptonshire, and Northamptonshire—Oxfordshire, and Oxfordshire—Berkshire, and Berkshire—Hampshire. There was a social continuity from the Humber to Southampton Water. Thus in literal truth there is to-day in England a single blood, although with some provincial thickenings. When, however, we come to the shore of the English Channel or of the North Sea, we look across to other bloods in France and the Netherlands from which a few drops have splashed across during the centuries. A different kind of boundary delimits the English Plain along the Welsh Border. When Simon de Montfort marched in 1265 along that border to meet his fate at the Battle of Evesham, his English soldiers, accustomed to living on bread, became mutinous because their allies in the Civil War, the Welsh mountaineers, could give them to eat nothing but the meat and milk of the upland (Oman). Some centuries earlier the English blood, and with it, as it happened, also the English

speech, had spread gradually across the arable English Plain until it was brought up sharp against the edge of the Welsh upland. That halt was imposed not merely by hill tribes on a defensible barrier; but no less by the sudden change from agricultural to pastoral habits of life. No doubt there has since been some transfusion of blood across the frontier, but, apart from the original mingling of stocks as the tide of Englishry advanced over the plain, surprisingly little. The boundary between the races is quite definite to-day. You remember the heart cry of Shakespeare's Mortimer: 'This is the deadly spite that angers me—my wife can speak no English, I no Welsh.' But that was a quasi-royal marriage, and royal blood is a precious fluid, exceptionally mobile!

Thus, in the English Plain we have a typical natural region, so far uniform in climate and soil as to favour social continuity within, but engirt by such physical features as suffice to break social continuity around by preventing or greatly impeding intermarriage. Within this natural region we have the English blood, one fluid, the same down through the centuries, on loan for the moment in the forty million bodies of the present generation. John Bull in his insularity is the exemplar of the myriad separate bloods and saps, each the fluid essence of a local variety or species of animal or plant. The climates and soils which by *their* continuities and discontinuities thus control the origin of species owe their power to their moisture; they, no less than the bloods and saps which react to them, are phenomena of the hydrosphere.

The natural region is no mere convenient generalisation; both by origin and effect it is a fundamental fact. Consider first its origin. Whether low-lying or high-lying it has a certain area or spread, and the ultimate reason of this is that the surface of liquid water is level. The technical terms of geomorphology, the concept of the morphological cycle, are based on the assumption that all landscapes are in process of grading down to level. In the case of plains, whether due to superficial denudation, or to marine erosion, or, again, to the deposit of sediment, the aqueous origin of the spread is obvious. But the boundaries which give relative isolation to natural regions were not so long ago almost universally attributed, in so far as consisting of features of land relief, to the shrinkage of the earth's size. The trend of hypothesis is now in a new direction. The vast accumulations of strata recorded in the foundations of the mountains, and the study of isostasy by observations for gravity, point to the aqueous origin of the upraising no less than of the degrading of land masses. The continents are apparently floating granitic rafts, which carry cargo, shifting cargo. They float on a heavier, basaltic layer which, except for certain local upwellings, comes to the surface of the lithosphere only in the bed of the deep ocean. The descending waters remove the uplands and distribute the spoil over wide lowlands and over the shallow sea bottom of the continental shelf. Thus, great breadths of the continental rafts are subjected to differential stresses—up-floating and down-sinking. In some cases the structure bends and there are slow readjustments of the sea level along the coasts; elsewhere the processes of mountain making—folding, faulting, shearing—are set up along certain belts which are axial as between the up-floating and down-sinking areas. Whatever be the details of observation and speculation along these lines, it seems

to be growing clear that the upraising no less than the destruction of land masses, the boundaries no less than the spread of natural regions, are in their origin, at any rate in the present and recent geological epochs, mainly due to the hydrosphere. In this discussion I have not dealt with the special features of the marine natural regions; they are obviously phenomena of the hydrosphere.

Consider now the results that follow from the existence of natural regions. We can imagine a world without definite natural regions, all sown over, let us say, with isolated peaks, like volcanic cones, and the liquid water gathered into innumerable small lakes, so that the lowland became a world-wide net of connected fertile strips with peaks and lakes in the meshes. In such a world it is difficult to see how a vast number of contrasted species could arise and flourish, for there would be no breaks in blood continuity behind which local varieties could be isolated and fixed, and no broad areas of uniformity where mature species could become dominant. Some day, in the fulness of knowledge, a biologist and a geographer will perhaps collaborate to show that our actual catalogue of species, in their grouped families and orders, is correlated with a world in which you have a single ocean, one great and three minor continents, a few vast plains and plateaux, a few high dividing ranges of mountain, and many small islands, peninsulas, valleys, lakes, oases. The effect of momentum from past geographies would, of course, have to be taken into account.

There are two essential facts to be remembered in regard to the hydrosphere. The first is that it conveys and stores energy and is not a source of energy. The energy which works within it comes for the most part either direct from the sun or is controlled by life. (Will you, for shortness, allow me to bracket the moon's tidal pull with the sun's energy?) The geographical significance of Life lies in its action in mass. That fact is obvious in the cases of rocks made of coral and coal made of trees. It is true no less where the life is carried in a multitude of mobile individuals. The higher beings act gregariously by impulse. They are attracted or repelled by ideas which they can communicate to one another. No one who has watched a flock of birds in flight but must agree to that statement. The Gadarene swine were another case in point. In human affairs the run on a bank, or the march of an army, or the digging of a canal is comparable.

A second characteristic of the hydrosphere is that it is a closed system. This follows from the facts that it is a sphere, that it is liquid or ever returning to liquid, and that, so far as we know, the amount of water is not appreciably increasing or diminishing. It is the medium, therefore, of a single dynamic system. Nothing can happen at any point within it which has not repercussions at every other point, although the internal elasticity of the system is fortunately mitigated by the rising and falling of the liquid surfaces and the passage of the liquid into the gaseous and solid states. Theoretically it is true that no cape is worn into a new form owing to a change in the impact of a sea current but a set of changes, nine-pin-like, is started which goes the round of all the coasts of the world, current changing shape and shape reacting on current. This statement carries with it the implication that every natural region is part of the

environment of every other natural region, how remote soever on the earth's surface, and from this it follows that the supreme vision of the geographically trained mind is of the world whole. The geographer takes over from the astronomer, physicist, chemist, geologist, biologist, historian, economist, and strategist certain results of their special studies, combines them into his own vision of a dynamic system, builds up his natural regions, and finally groups these into his world conception.

When I was in Africa I remember seeing before me a great billowing slope, clothed with dense forest, dark green and burnished in the sunshine. I entered and traversed that forest for a long day. When I emerged and looked back there was the same forest, and yet to my vision it was not the same, for I could now appreciate its texture; I had not merely sight of it, but insight. So it is with the trained geographer, he starts on the shoulders of the scientific specialists, he traverses his natural regions, and emerges with a new grasp and insight of the world as a whole. This, if I mistake not, will be his essential contribution to the shaping of our human destiny in the not far distant future.

In the world view of the geographer what are the major features of Humanity and the Human Habitat? Surely they are two, the East and the West. Let me try to set in perspective some salient facts.

The monsoon winds sweep into and out of Asia because that vast land lies wholly north of the equator and is, therefore, as a whole, subject to an alternation of seasons. Over an area of some five million square miles in the south and east of Asia, from India to Manchuria, and in the great adjacent islands, the monsoon drops annually a rainfall amounting on the average of years to some 18 million millions of tons. Half of mankind, 900 million people, live in the natural regions of this area; about 180 to the square mile. The rainfall is, therefore, of the order of some 20 thousand tons annually for each inhabitant. There is a considerable traffic between the regions of the group, and there are the fisheries; in order to see it whole let us add three million more square miles for the marginal and landlocked seas. Then we shall have a total of eight million square miles, or 4 per cent. of the globe surface, carrying 50 per cent. of the human race. The annual increase of population may amount to some seven or eight millions, and as compared with this figure both emigration and immigration into and from the outer world are small. In the main we have here vast stable peasantries, 'ascript to the glebe,' if we may use a mediæval legal expression; tied to the soil; a tremendous fact of rain, sap, and blood. That is the East.

The West lies in Europe, south and west of the Volga, and in that eastern third of North America which includes the main stream of the Mississippi and the basin of the St. Lawrence. Europe within the Volga boundary measures some three million square miles, and eastern North America some two million square miles. The two together are, therefore, equivalent in area of land to the group of regions which constitutes the East. If we add three million square miles for the fisheries and the oceanic belt which contains the 'shipping lanes' between Europe and North America, we shall again have a total of 4 per cent. of the globe surface, and that is the main geographical habitat of western civilisation. Within this area are 600 million people, or 120 to the square mile of land. Not-

withstanding the oceanic break it may be regarded as a single area, for the distance from E.N.E. to W.S.W., from the Volga to the Mississippi, measures only some seven thousand miles, or little more than a quarter way round the globe along the Great Circle. The rainfall on the land is drawn from the same source both in Europe and eastern North America; it comes mainly from the south, from the Atlantic, and is of the order of 12 thousand tons per human inhabitant per annum. There is an annual net increase of population of some four or five millions and, as compared with this, emigration to the outer world is small, for the movement of a million migrants a year from Europe to North America in the dozen years at the commencement of this century was, of course, internal to the area.

Thus we have two areas, measuring together less than 10 per cent. of the world's surface, but containing more than 80 per cent. of the world's population. Outside is some 90 per cent. of the world's surface, but only 20 per cent. of the population. On some 40 million square miles of land, outside the East and the West, you have an average density of population of only ten to the square mile, as contrasted with 120 on the five million square miles of the West, and 180 on the five million square miles of the East. The moisture upon the land areas, outside the Western and Eastern rain zones, varies from Saharan drought to Amazon and Congo deluge, but it is a remarkable fact that South America, with all its fertility, has upon its 6½ million square miles a population of only ten to the square mile, or the average for the world outside West and East. This fertile vacancy of South America may be regarded perhaps as a third great feature of the Habitat of Man; it must be set alongside the extraordinary and persistent self-containedness of the East and the West. The increase in the world's population, some 12 millions a year, is mainly retained in its native East and West, and the growth of the outer populations, even though reinforced by some immigration, is relatively insignificant. The main growths, the spread of the sheets of human blood, have been merely overflows from the anciently occupied regions into adjacent areas —into North and North-Eastern Europe, into Eastern North America, and into Manchuria, and in each case the natural frontiers of drought and frost have now been approached, except for relatively narrow outlets along the wheat belts of North America and Siberia. Even in North America the centre of population has ceased to move appreciably westward.

In this continued growth of population in the East and the West in far greater actual number than in the rest of the world, notwithstanding the abundant rainfalls in several large regions elsewhere, we have an instance of geographical momentum. That momentum, though issuing from the past, is a fact of the present, an element in the dynamic system of to-day's geography. I repeat an analogy which I have used elsewhere. If I stand on a mountain-top there are two answers to the question why I am there. The first is that the rocks hold me up—that is the dynamic answer; the second is that I climbed there—that is the historical or genetic answer. Whether we look backward or forward, our genetic studies should start from a firm hold on the dynamic system of the present. The trained geographer may restore imaginatively the geographies of the past and so contribute to geology, archæology, and history; in a word,

he may study the historic present; but whether he study present or past, he is primarily concerned with space, and with time only in the sense that everything has momentum. The present consists of a coming out of the past and a going into the future; there is a complex dynamic present in the sense of a balance of forces, all severally waxing and waning in different degrees. The student of the hydrosphere is concerned with water, sap, and blood, moving under sun power and life initiative. Some of the shapes governed by his circulations are relatively stable, such as land forms, but not much more so than the forms, say, of his sea currents or of the average distribution of rainfall as depicted on his maps. Even the distribution of human population is, as we have just seen, subject to a momentum which overrides the attraction of great physical opportunities.

Let us now turn for a moment to a not improbable relation of the hydrosphere to human—and perhaps all living—initiative. It is of the essence of life, whatever that may be, that it can oppose itself to the blind pressure of changing environment and, provided the change be not too violent, new species thus arise by survival of the fittest. Of all the changes of environment to which living beings are subject the most general and potent are undoubtedly due, directly and indirectly, to variations in the amount and mode of water supply. Let me cite one well-known fact by way of illustration. A newly-born babe has some prehensile power in its feet and some tendency to oppose its great toe to the other four. This is interpreted as indicative of a four-handed ancestry living in the trees. Sir Arthur Thompson has suggested that in some past geological epoch increasing drought slowly reduced the areas of forest and drove some of the over-crowded population to forsake their leafy homes, compelling them to become bipeds. The inference is obvious that in fighting against drought and frost terrestrial life is stimulated to initiative.

The same thing is true in regard to development of human society. There is one point, for example, in which the East and the West are alike; in large areas of both, owing to seasonal interruption of the rainfall and to winter frost, the growing period of vegetation is limited to certain months of the year. That is not the case with the tropical forests of South America and West Africa; and it is noteworthy that in the Malayan region of the East, where there is tropical continuity of heat and rainfall, the human population is sparse, except of late in Java under the western control of Holland. Here in the abundance of moisture humanity appears to lack the incentive to development.

The chief weapon in the fight for water against drought and frost is Capital. In its simplest form, Capital consists in the saving of food for the annual season of drought or frost. The biblical Joseph who laid up in the seven good years for the seven bad years was a Statesman who knew the value of so-called 'liquid' capital.

But if West and East have this in common, they differ greatly in their actual output of human energy. What has our study of the hydrosphere to say in that regard? Civilisation seems to have begun most remarkably at the geographical centre of the world: in the region around Suez, where the eastern and western oceans approach one another at the junction of Asia and Africa. You will remember that the mediæval monks

placed the centre of their wheel maps of the world in Jerusalem. Here in this region the nomad, living on the milk of his camels, migrates annually in pursuit of the sparse rainfall which gives steppe fodder for his herds. In this region also, the Nile and the Euphrates offer to the primitive agriculturalist the combination of year-long sunshine, absence of frost, and rivers overflowing and shrinking with the time of year. Thus, neighbouring peoples were, on the one hand, compelled to be ever mobile, and tied, on the other hand, to a sedentary existence. So the contrasted supplies of moisture drove men to that great clash between nomadism and agriculturalism which gave rise, in some degree, at any rate, to the energy, the discontents, and the conquests of the West. The regions of the East are relatively sheltered, and are exposed to nomad invasion only along two or three definite roads, whereas the West lies widely open to the desert and steppe and, therefore, to nomad pressure, save where shielded by the broader parts of the Mediterranean.

Of course there is more than this in the recent history of western civilisation. The command which the West has exercised in the last century over the vacant seas and almost vacant lands of the outer world, and the siege which it has laid to the East, have been intensified by a temporary cause; western men have led the way in the exploitation of the coal, oil, and iron-ore which were laid up by the hydrosphere under the powers of sun and life in past geographical conditions.

In the dynamic system which, it appears to me, should be the main subject of our geographical study, there are two elements which present a certain analogy and yet are different. Both of them are products of the past and yet both belong to the present. In thinking of coal the mind goes quickly back to long past forest scenes; but there is no need of geological history in order to appreciate the liberation of energy from coal in the process of combustion to-day. Similarly, the momentum which is characteristic of established circulations, as well in the human and economic as in the purely physical aspects of geography, may be studied according to origin and growth, in geology and history, but, on the other hand, as a fact of the present in our geographical dynamic system. Take, for example, a great human fact—London. To-day London is essentially a market, and there is no more persistent fact in human geography than a market; it is a nodal point not merely of roads but of circulations; its momentum is the product of a number of momenta interacting. In an established market men from near and far trust to find a competition of purchasers for the goods they have to sell, and a competition among salesmen of the goods they seek to buy. According to the recent census, London in the widest sense has some 12 million people, for those who serve the market require the aid of a port, industries, holiday resorts and suburban types of agriculture. To the merchant of to-day the origin of London in its hill village and tidal creeks is unimportant; at most, if he be a man of culture and imagination, it may be a fascinating romance for his hours of leisure and for the company of inquisitive children. But momentum is a practical fact of the present, which he will neglect in his business at his peril; for the many elements which enter into the momentum of London are all the time varying, some waxing and some waning. If he be a bit of a philosopher, as well as a romantic, he may

look ahead a couple of generations, and see his London grown to be half England, our so-called staple industries, though let us hope maintained, having need for fewer workers, and the momentum of the world market having increased. That 12 millions should grow to 24 millions in a single urban and suburban area seems an almost impossible destiny. But if you reflect that 12 millions are already ·7 of the world's population, and that 24 millions would only be 1·4, the miracle seems to shrink. As we have seen, the East and the West, as great natural regions and human communities, are not losing their relative importance in the world ; the growth of to-day seems to be rather intensive than extensive. With ever more centralised organisation, the managing centres of the world's economy will become more and not less essential. Though some functions may be delegated to lesser cities, it does not look as though the relative importance of the few great world centres—say London, Paris, Berlin, New York, and Chicago—will tend to diminish.

One more point on this matter of momentum. Time was when marshes and forests were most effective boundaries for the little natural regions inhabited by primitive agricultural tribes. Here in England and on the neighbouring continent such tribes settled upon the medium soils, for they had not the art of fertilising the light soils, nor because of the rudeness of their implements could they till the heavy soils. Gradually as the arts of life improved, the forests were cleared and the marshes were drained, and the lesser natural regions were fused into greater. It may perhaps be thought that with the continuance of this process all mankind will in the end be unified. The element of momentum must, however, not be forgotten. Take an illustration from the development of language. Modern printing, and now broadcasting, are bringing it about that the traditional peasant with his few hundred words is being replaced by whole populations with rich vocabularies of many thousands of words, and all manner of implications and shades of meaning in their phrases. A great national culture has an immense momentum which makes itself felt at the frontiers between the nations. An educated Englishman, though he may learn foreign languages and acquire an international outlook, is in fact far more intensely an Englishman than is the uncultured peasant of his land. The minor and less developed peoples may, in some cases, be absorbed, but the momentum of the greater nationalities is at present visibly increasing. Unless I mistake, it is the message of geography that international co-operation in any future that we need consider must be based on the federal idea. If our civilisation is not to go down in blind internecine conflict, there must be a development of world planning out of regional planning, just as regional planning has come out of town planning. The statesman of the future must know something of the geographical natural regions, if he is to build for stability. The peaceful readjustment of treaties to differential growth will postulate an informed and delicate geographical judgment.

In the formation of such a judgment, geomorphology cannot play the major part. Until within little more than a century ago, maps were essentially hydrographical documents ; they showed with any accuracy only coast lines and rivers, and the positions of towns on the coasts and the rivers. Precise representation of terrain is a modern addition. The

map of to-day, no less than the landscape itself, may be a thing of joy to the eye of an artistic geographer; hill forms and stream meanderings may present a poem of rhythm and harmonies to a Vaughan Cornish. Inevitably he will try to penetrate beneath the surface, for he sees questions and desires answers, but let him beware! What matters from his standpoint is whether the rocks are porous, or impervious, or soluble; how bedded and interbedded; at what angles they come to the surface, and with what jointing and faulting; how they weather and what soils they yield. I would have the young geographer practised in the use of an almost Ruskinian, purely descriptive language, with terms drawn from the quarryman, the stonemason, the farmer, the alpine climber, and the water-engineer. Of course, it would be pedantic to press such a revolt too far; there are ready-made stores of appropriate knowledge in geological literature, but they are expressed in an alien tongue, from a standpoint other than the geographical. The geographer is concerned with the dynamic relations of the atmosphere and hydrosphere with the lithosphere; as geographer he has little concern with geological horizons and epochs. None the less, the temptation to describe a region in terms of geological dates is besetting and insidious. It is a dangerous practice because it tends to lead the geographer away from his duty. Geology should be to the geographer what anatomy is to the artist; the subsidiary subject makes a good servant but an ill master.

My aim in this address has been to suggest that if you put the hydrosphere rather than the lithosphere in the forefront of your geography, there at once emerges a certain unity permeating both aspects of the subject. Water carries and stores energy, whether that energy emanate directly from the sun or be controlled by life. Two agents, the sun and life, work in the same medium, water, and must therefore obey the same conditions. Life canalises some of the movements due to the sun. Coral reefs and man-built breakwaters impede the action of waves; beavers and men dam the flow of rivers. Rising to greater co-operative efforts, man irrigates the desert, connects the oceans by artificial straits, and intercepts energy from the waterfalls. At the present time—as it were in a great parenthesis of history—man taps vast but exhaustible stores of potential energy which, in past ages, were piled up by the hydrosphere in deposits of coal and oil. Thus, the water-borne character of our civilisation is for the moment partially masked. But essentially both Physical and Human Geography are concerned with the carriage and storage of energy on the surface of this earth, and the vehicle is the Protean element, water. Even the lightning is incidental to the cloud, and broadcast music depends on steam or water-power.

There is not merely a philosophical consistency thus imported into geography—that in itself is of great value in an educational discipline—but there is also a complete change in practical outlook. When you treat the land forms as primary and think of the fluid movements as controlled by them, your mind inevitably looks back to the evolution of those forms, you think in terms of geology, of history; but when you treat the fluid circulations as primary and think of the land forms as incidental to them, your mind is concentrated on the present; you are thinking dynamically, you are ready for action. In the first case, the geographer is consorting

with the archæologist; in the second, with the architect and the engineer.

Of late geographers—I am not speaking of cartographers—have begun for the first time to see a practical and not only an educational outlet for their training. As I have said, regional planning has emerged from town planning, and we shall sooner or later come to world planning. During recent generations human affairs have moved with a tragic rhythm; first a great War, followed by exhaustion; then the relative peace and recuperation of a temporary equilibrium; finally, a period of increasing stress, when statesmen dare not act for fear of precipitating conflict between forces which have become unbalanced owing to varying rates of growth under differing natural and human conditions. We are now in the trough of our post-war exhaustion, but the younger generation should enjoy a mid-century opportunity, when—perturbed neither by exhaustion nor fear—men may in some small degree be masters of their destiny, always provided that they be masters of their minds. Then will be the opportunity of the geographer-statesman, for geography must underlie the strategy of peace if you would not have it subserve the strategy of war.

The saying is that in the beginning history is all geography; in other words, Man was once helpless in the grip of Nature. Can we not strive that in the end, also, history *shall be* all geography, but with a difference of implication? The first was a fatalistic utterance, the second is the utterance of the world-planner, the geographer in action. Heaven save us from falling into the hands of any expert, whether he be doctor or geographer, but they who hold up the arms of Moses have a share in his victory. While turning our faces thus resolutely to the future, let us not quite part company with our good friends, the archæologists, and our one-time rivals, the geologists; in becoming Realists let us not cease from being, in a reasonable degree, Romantics.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

THE CHANGED OUTLOOK IN REGARD TO POPULATION, 1831-1931.

ADDRESS BY

EMERITUS-PROFESSOR EDWIN CANNAN, M.A., Litt.D., LL.D.

PRESIDENT OF THE SECTION.

Times change, and economic theories change with them. We need no longer be ashamed of the fact, as we used to be inclined to be in the old days, when our colleagues in other Sections of the Association professed to despise us for disagreeing among ourselves and perpetually overthrowing conclusions arrived at by our predecessors. We hear less now of the certainty and finality of the other sciences, and can face their exponents unabashed, confident that theories may be useful for leading us on towards the truth without being immutable and exempt from revision.

I think that the biggest change made in economic theory during the last hundred years is to be found in the treatment of the subject of Population. In 1831, Malthus was still alive, and quite unrepentant for the shock he had given the public thirty-three years earlier by his *Essay on the Principle of Population as it affects the future Improvement of Society.* No one, it is true, any longer attached much importance to his doctrine of the inherent incompatibility of the ratios in which it was possible for population and food to increase, but the disfavour with which he regarded what he considered the natural tendency of population to increase was shared by most of the economists of the orthodox school, who had adopted the theory of diminishing returns to agriculture which was evolved in England from the local conditions of the very 'short period' of the Napoleonic war.

That theory, not as now taught in a form which makes it innocuous, but as taught in the early years of the nineteenth century, purported to show that the natural limitation of fertile and well-situated land must necessarily mean that the more numerous the people, the more difficult it must be for them to feed themselves. It was admitted that there were counteracting circumstances, summed up as 'the progress of civilisation,' which, in fact, had throughout history prevented the growing population of the civilised world from actually finding it more difficult to feed itself, but these circumstances were regarded as making only temporary headway against the general tendency, and not, like it, as being a law of nature. J. S. Mill, in his *Principles of Political Economy with some of their Applications to Social Philosophy,* which, though not made into a book until seventeen years after, was really thought out before 1831, and represents the ideas of 1800 to 1830 better than any other work, even ventured to

assert that though the people of his time were better off than the people of a thousand years before, they would have been still better off if the increase of population had been less.

The economic history of the hundred years has tended to bring about a very complete reversal of economists' view of this matter.

The hundred years began with developments which threw great discredit on the fundamental assumption of the old school that the extension of human occupation of land necessarily meant that less fertile and less well-situated land must be occupied as numbers grew. It was easy for men who saw arable cultivation creeping over barren hills in England and stony 'bogs' in Ireland to believe in that theory when Chicago was a collection of Indian huts, and Broadway, New York, a rough cart track to a farm, but the application of steam to ships and railways enabled mankind to extend easily over an immense area of land more fertile than much of what was occupied before. And as for situation, not only did the improvement in transport, coupled with the violations of natural geography involved in the cutting of the great ship canals, bring the 'more distant' lands nearer the 'market,' it also eventually brought 'the market' to the 'more distant' lands.

So we no longer think of the first cradle of the human race (or the first cradles of the human races if there are more than one) as the most fertile and well-situated spot (or spots) from which men have gradually been forced outwards. You all probably know the opinion of the British Army in Mesopotamia, expressed by the sergeant who was told by an officer that he was now on the very site of the Garden of Eden: 'Well, Sir, all I can say is that if this was the Garden of Eden it's no wonder the twelve apostles mutinied.' Though the sergeant was evidently not a well-read man, the change of view had reached even him.

Later in the hundred years scientific discovery in various directions has led to a complete change of emphasis in regard to the importance of what the old economists used to call 'improvements.' The old economists thought of hedges and ditches, drains, and a few other trifles of that kind which would enable corn to be more easily produced from European fields, and just a little of better breeding of cattle and sheep. These were things which might, they believed, interrupt for a time, now and then, the general downward drift of the returns to agricultural industry, but could not do more than that. Modern science has changed our outlook. We set no bounds to the possibilities of improvement. We expect to make unwholesome areas healthy, and to modify vegetable as well as animal products so that they will better serve our needs. Primitive mankind presumably fought and killed some of the now extinct carnivora; advanced mankind fights and will kill the locusts and the smaller insects which have hitherto prevented much use being made of some of the most fertile areas of the world. It was not an economist who, only a few years ago in the Presidential chair of the Association, foretold that very soon the world would be suffering from a shortage of wheat.

Thus, even if we still expected population to increase very rapidly, we should not believe, as J. S. Mill did, that it 'everywhere treads close on the heels of agricultural improvement, and effaces its effects as fast as they are produced' (*Principles*, Bk. IV, ch. iii, § 5). But in fact, Cotter

Morison's cry, made only a generation ago, that all would be well if only we could stop for a few years 'the devastating torrent of babies' now seems grotesque, for we do not now expect rapid increase of population to continue much longer, even if it becomes progressively easier to obtain subsistence.

The approach of reduction in the rate of growth of population began to show itself in England in the second half of the 1871–80 decade, when the annual number of births became nearly stationary after the rapid increase recorded down to 1876. But the public takes little notice of the supply of people furnished by the births. Just in the wooden way in which illiterate farmers and unbusinesslike old ladies look at their balances at the bank, so the public looks at the censuses. The census of 1881 showed an increase of 14·36 per cent. in the decade, which was higher than that shown by any of the censuses except those of 1821 and 1831, which were probably unduly swollen by the diminishing incompleteness of the enumerations. In 1881–91, in spite of high emigration, the rate of increase only dropped to 11·65 per cent, so rapid increase of population was still regarded as the normal thing which everyone should expect. The Royal Commission on the Water Supply of the Metropolis in 1893 deliberately rejected the reasonable suggestion that the rate of increase in Greater London might continue to fall as it had already begun to do, and relying on a continuance of observed increase, put the probable population in the present year, 1931, two and three-quarter millions more than the recent census has shown it to be.

But I had noticed that the old rapid increase in the annual number of births seemed to have come to an end, and working on the ages of the people as recorded in successive censuses, I put before this Section of the Association at its meeting in Ipswich in 1895, a paper (subsequently published in the *Economic Journal* for December in that year) in which I estimated the number of persons who would be living at each census up to that of 1951 on the assumptions that migration, mortality, and (not the rate, but the absolute number of births) remained stationary. I found that on these hypotheses the population of England and Wales would stop increasing during the present century, and would have only a trifling increase after 1941. The paper suggested that this was, at any rate, not improbable,

Hostile critics derided what they called my 'prophecy,' and for some time events were unfavourable to me. Emigration fell off enormously, mortality decreased, and the births increased slightly, so that the census of 1901 showed an increase of 12·17 per cent. in the decade, the absolute increase of three and a half millions being the largest recorded. But the situation was not fundamentally altered, since the increase of births was due entirely to the drop in emigration, which had caused a larger proportion of persons of parental age to remain in the country. In the *Fortnightly Review* of March 1902, I returned to the charge with an article on the 'Recent Decline of Natality in Great Britain,' in which, using a method of weighting the annual numbers of marriages by their proximity to the births recorded for each year—a method which seems to have been beneath the notice of the mathematical statisticians of that period—I was able to show, I think, conclusively, that the number of children resulting

from each marriage was falling steadily and rapidly, and insisted with more emphasis than before on the 'considerable probability of the disappearance of the natural increase of population—the excess of births over deaths—in Great Britain within the present century.'

The decade 1901–11 was indecisive; the ratio of increase was smaller than in any of its ten predecessors, but the absolute amount of increase just topped that of 1891–1901, and the number of births till 1908 or 1909 seemed to indicate some recovery of natality. But this was illusory. Even before the War the births had got down again to the level of 1876. The War sent them tumbling down to about three-quarters of that number, and now, after a wild but very short-lived recovery when the Army returned from abroad, they seem inclined to settle at the War figure—three-quarters of the number attained more than fifty years ago when the total population was twenty-six millions instead of forty millions, as it is now. The ratio of births, legitimate and illegitimate, to my weighted figure of marriages was just over $4\frac{1}{2}$ fifty years ago, fell gradually and steadily to $3\frac{1}{4}$ before the War caused it to collapse. (See the Appendix.)

It was commonly supposed by many of those to whom percentages serve rather to hide than to expose the facts on which they are based, that the diminution of births was being counterbalanced by the decline of infant mortality. It is true of course that diminution of infant mortality mitigates the effect of decline of natality, but the degree in which it can do so obviously decreases as the rate of infant mortality falls. When that rate is 500 per thousand, as it probably was here in the reign of Queen Anne, and may be still in great parts of Africa, a cutting down of births by 25 per cent. can be counteracted completely by a drop of one-third in the infantile mortality rate. But when the infant mortality rate is down to 100 per thousand, it would have to fall to nothing at all in order to counteract a decline of only 10 per cent. in the number of births. In fact, the rate has fallen in England and Wales from about 140 to about 70 in the fifty years from 1881, and this drop to one-half only balances about one-fifth of the decline in the number of births.

Though there were eminent dissentients only a few years ago, statisticians are now agreed that in the absence of some great and unexpected change, the increase of population in England and Wales will come to an end at a very early date. Even the lay public has been to some extent enlightened and rather shocked by the recent census announcements that the population of Scotland has actually decreased in the ten years, and that of England and Wales has increased only 2,061,000, as against 3,543,000 in the ten years from 1901–11, though the emigrants have been 324,000 less.

The same change is observable in some degree in other Western European countries and our own oversea offshoots. The cause of it—birth control—will doubtless in time affect the rest of the world, so that while we may expect considerable increase—even an increase much more rapid than at present owing to decrease of huge infant mortality—to take place among the more backward peoples for another half-century at least, there is no reason whatever for expecting the population of the world to 'tread close on the heels of subsistence' in the future, even if it may be correctly regarded as having done so in the past.

This change in our expectations involves many changes of emphasis, both in the theory of production and in that of distribution.

Two of them are perfectly obvious. First, the need, which J. S. Mill and most of his contemporaries and immediate predecessors felt so strongly, for insisting on the due restriction of population, has completely disappeared in the Western countries. Economists do not now require to talk as if the first duty of men and women was to refrain from propagating their race. Secondly, the need for insisting on the desirability of saving has become less pressing. A rapidly increasing population requires a rapidly increasing number of tools, machines, ships, houses, and other articles of material equipment in order merely to maintain without improving its economic condition, while at the same time the maintenance of a larger proportion of children renders it more difficult to make the required additions. To a stationary population saving will still be desirable for the improvement of conditions, but it need no longer be insisted on as necessary for the mere maintenance of the existing standard.

But there are other changes of equal importance which are more likely to be overlooked. One is in regard to the weight which we attach to the different kinds of production. In the middle of the eighteenth century 'subsistence,' and what we should consider a very coarse and inadequate subsistence probably seriously deficient in vitamins, appeared so much the most important economic good that the French *Économistes* insisted on calling all labour which did not get something out of the soil *stérile* or barren; and our own Adam Smith, with all his common sense, while admitting the manufacturing class into the ranks of 'productive' labourers, insisted on excluding domestic servants, physicians, guardians of law and order, and all other workers who did not make up material objects, or who were not employed for profit (he never was quite sure which criterion he meant to stand by). The great Christian philosopher, Paley, believed that nothing more than a 'healthy subsistence' was required for perfect happiness. Even Malthus and his immediate disciples, when they insisted on the desirability of the working-class having a high standard of comfort, seem to have done so more because this would prevent the 'misery' of semi-starvation for adults and absolute starvation for infants than because there is a direct advantage in being comfortable. Ricardo said 'the friends of humanity cannot but wish that in all countries the labouring classes should have a taste for comforts and enjoyments,' not apparently because comforts and enjoyments are good in themselves, but because 'there cannot be a better security against a superabundant population,' the population being superabundant, in his opinion, when it is subject to famine.

All this emphasis on food is now out of date. We no longer look forward to a future in which an increasing population will be forced by the operation of the law of diminishing returns to devote a larger and ever larger proportion of its whole labour force to the production of food. We know that even in the past, with a rapidly increasing population, the returns to agricultural industry have increased so much that civilised mankind has been able to feed itself better and better, while giving a smaller and ever smaller proportion of its whole labour force to the production of bare subsistence; and we can reasonably expect that the increase in the

productiveness of agricultural industry will be at least as great in the future, so that under the combined influence of the 'narrow capacity of the individual human stomach' and the stationary number of stomachs, not only a smaller and ever smaller proportion, but a smaller and smaller absolute number of workers will be able to raise food for the whole.

Even the politicians, who for the most part follow the economists with a sixty or seventy years' lag, are beginning to realise the change, and are losing their enthusiasm for schemes for 'settling more people on the land,' either in colonies or at home, and thereby increasing the already excessive depreciation of agricultural compared with manufactured products. The numerous subsidies which they still give to agriculture are mostly of an eleemosynary character intended to relieve distress, and the encouragement which they give to agricultural production is only an incidental effect, unintended and often deplored. They are defended, not on the ground that they increase food, but because they are supposed to increase employment.

The necessary change of emphasis applies not only as between food and other things, but as between most primary and most finishing industries. In face of rapidly growing knowledge and slowly growing or stationary population, it is inevitable that the 'staple' or 'heavy' industries which provide materials should decline relatively to those which provide finished goods and services. The demand for each of such things as pig-iron and yards of cloth is easily satiated; and so also, no doubt, is the demand for cricket-bats and chauffeurs. But the minor or 'lighter' industries are susceptible of an indefinite multiplication which makes the demand for their products, taken as a whole, insatiable. Increase a person's power of spending, and he will not increase his purchases in weight or bulk so much as in refinement of form, so that a richer people will devote a less proportion of their labour to producing things like pig-iron and bricks. Moreover, the mere fact of the disappearance of rapid increase of population tends to increase the proportion of demand which can be satisfied from scrap without fresh primary production. So, given a stationary population with rapidly increasing knowledge applied to production, we may expect the already observable tendency towards a less proportion of the whole labour-force being employed in the 'heavy industries' and a larger in the lighter industries to become more pronounced. Perhaps we see this even now in the slight drift of industrial population from the North to the South of England which appears to be taking place.

Another change of emphasis, of little importance on the Continent, where the West-Ricardian theory of rent never took real root, but of great importance in England and other English-speaking areas, is in respect of the landowners' share of the community's income. The disappearing bugbear of diminishing returns carries away with it the vampire rural landlord, who was supposed to prosper exceedingly when diminution of returns made food scarce and dear. You all know the famous passage in which J. S. Mill described the landlords as they appeared to him and the school which he, a little belatedly, represented:—

'The ordinary progress of a society which increases in wealth is at all times tending to augment the incomes of landlords; to give them both a greater amount and a greater proportion of the wealth of the

community, independently of any trouble or outlay incurred by themselves. They grow richer, as it were, in their sleep, without working, risking, or economising.' (*Principles*, Bk. V, ch. ii. § 5.)

Perhaps the disciple went a little beyond his master, Ricardo, in asserting so roundly that in a prosperous society the landlords must tend to get a larger and ever larger *proportion* of the whole income, but there can be no doubt that this was the impression which the Ricardian school conveyed to the public, and which formed the foundation for Henry George's scheme of land nationalisation and the agitation for land-value taxation. If the school had only meant to teach that the land became more valuable absolutely—in the sense of being worth a larger absolute amount of commodities rather than a larger proportion of all the commodities and services constituting the community's income—they could not have supposed land so peculiar, since it would share this characteristic with many other things—with anything which was more limited in supply than the generality.

To grasp the completeness of the change of view which has taken place in the last hundred years, we must notice that Mill and the whole school which he represented were thinking not of the few lucky landlords who have inherited land which has been selected by nature or accident as the site of a city, but of the ordinary rural agricultural landlords. So far have we moved that the land-value taxers of to-day quite cheerfully propose to exempt all ' purely agricultural value ' from the imposition which they advocate.

Envy of the happy owners of such urban land as rises in value more than enough to recoup what they and their predecessors in title paid in road making, sewering and other expenses of ' development ' plus loss, if any, in waiting for income, still plays a part in contemporary politics, but the economist foresees that there will be at any rate less of such rise of value when the adult population ceases to increase and the demand for additional houses and gardens consequently disappears. He realises that if any such rise continues, it will be due to the people being not only able, as they doubtless will be, to occupy a larger area with their houses and gardens, but also desirous of doing so. He will think this quite possible, but will not be confident about it, when he reflects that the vast spread of villadom may be only a temporary phenomenon, and that the married couples of the future, childless or with small families, may be more content with flats in towns and little bungalows with tiny curtilages right in the country.

The disappearance from economic theory of the picture of the vampire landlord taking an ever-increasing proportion of the whole produce of industry which was itself decreasing per head of workers, leaves the theoretic arena open for discussion of the sharing of the whole produce between earnings of work and income derived from possession of property of all kinds.

As to this, the economists of a hundred years ago had nothing to say. If they thought of the question at all, they mixed it up hopelessly with the rate of interest on capital, imagining property to receive a smaller proportion when the rate of interest fell, and *vice versa*. The Socialists, who followed them in fact the more closely the more they denounced them,

failed completely to clear up the confusion, and it dominates the mind of the lay public even now—much, I admit, to the discredit of the economists, who should have taught that public better.

While there are no statistics on the subject worth much, and none covering any considerable area either of place or time, past history is sufficiently known to assure us that increasing civilisation has, in fact, made the aggregate share of property grow faster than that of labour, the obvious cause of this being that useful things constituting property have grown faster than population, and so much faster that what decline of the rate of interest has taken place has not been sufficient to counteract the tendency. The most primitive people had scarcely any tools, and their buildings, if any, could be erected in a few hours. Ownership certainly did not then give a claim to about one-third of the whole income, as statistics suggest that it does in modern Western countries.

There is nothing to show that this tendency will be either reversed or intensified by a cessation of the growth of population. The cessation will, of course, tend to reduce the desirability of additional equipment; a large part of the additions of the past have been required simply to enable the additional people to be provided with tools, houses, and other instruments of production or enjoyment. But additions to equipment will be made with less sacrifice of immediate enjoyable income than before, so that the increase of quantity may be sufficient to counteract the decline in the value of the units. Moreover, it is quite impossible to say what the tendency of invention may be in the future—whether to enhance or to diminish the value of additional material equipment.

But the history of the last hundred years suggests that this question of the division of income between property and labour is losing whatever importance it possessed. The economists and socialists of a hundred years ago were little removed from the time when it was common to talk of 'the labouring poor,' as if society was pretty sharply divided into poor workers on the one side and rich owners of property on the other. There were, indeed, some members of the propertied classes who were poor, but they were offshoots of the wealthier families rather than members of the proletariat with a little property. How innocent the mass of the people were of the crime of owning anything you may realise if you recollect that none of the agencies with which we are familiar for enabling them to invest had then got beyond the embryo stage. Friendly societies, co-operative societies, building societies, savings banks, are all modern growths. Before their advent a worker could, of course, become a small master—never, I think, a small mistress—and from a small master grow to be a big master, but if, for any reason this was not open to him, what could he do with savings, supposing he was able to make any? Put them in a stocking, or the thatch, or under the garden soil, and if they happily escaped accident there, and accumulated sufficiently, give them to an attorney of doubtful honesty to be lent out on mortgage. I remember only about fifty years ago being told by a booking-clerk at a moorland station, about a hundred miles from London, how two old women had recently paid for return tickets to London in threepenny-bits, and by a solicitor that an old man from the same district had just brought him for investment on mortgage a large sum in gold which he had so far been

keeping in the thatch of his cottage. All this is now changed, and when property, as a whole, and not merely the large property-owners, is attacked, the great investing agencies of the 'working classes' become formidable opponents and are supported by the small direct investors who have been helped by them.

And while many of the working-class have become property owners, many of the propertied class have become the paid servants of public companies and other institutions, so that the old sharp distinction between the wage-earner and the capitalist is become a thing of the past, and the division of income between property and labour is no longer a division between two classes composed of different individuals, but a division between two sources of income largely possessed by the same individuals.

Thus, in Distribution, emphasis on the old categories of land, capital, and labour is rapidly becoming obsolete and is being replaced by emphasis on individual riches and poverty, however arising. No longer do we think of relieving poverty by improving the terms of the general bargain which theory conceives labour as making with capital; we are much more likely to meet with arguments that individual poverty is being caused by this general bargain being too much in favour of the wage-earners. It is no longer the lowness of standard earnings that worries the philanthropic economist, but the fact that so many people are unable to rank themselves among recipients of those wages. Emphasis is on unemployment.

Unemployment is not really a very modern phenomenon. The crowds of beggars who collected their daily dole in the Middle Ages from the monasteries and from private wayfarers and householders were, perhaps, as large a proportion of the population as the normal registered unemployed of to-day. The 'distresses' of the period just preceding a hundred years ago seem to have been accompanied by enormous unemployment, but we have no reliable statistics, and the loose statements, such as that in Birmingham in 1817 one-third of the workpeople were wholly unemployed and all the rest on half-time, do not help us much. But so far as I know, it has never been contended that history shows unemployment to be greater when population (or even population of working age) is rapidly increasing.

Yet it is common to talk of 'the difficulty of providing employment for a rapidly increasing population,' and some eminent authorities quite recently endeavoured to console the public by alleging that the coming decline in the growth of numbers will greatly alleviate the present situation in regard to unemployment.

I believe this to be a profound error, based on an elementary misconception of the origin of demand. The old proverb 'With every mouth God sends a pair of hands' is true and valuable, but no more so than its converse, 'With every pair of hands God sends a mouth.' The demand for the products of industry is not something outside and independent of the amount of products. The demand for each product depends on the supply of products offered in exchange for it, and the demand for all products depends on the supply of all products. Consequently, there is not the slightest danger of the working population ever becoming too great for the demand for its products taken as a whole.

Unemployment arises not from insufficient demand for the products of industry as a whole, but from the number of persons offering to work in particular branches of industry being in excess of the number admissible, having regard to the conditions and wages which are required to satisfy both the would-be workers who are unemployed and the persons already in employment. If the unemployed will not take what employers would offer them, the case is simple, and it is only a little more complicated if they are willing to take, and the employers are willing to give, something less than what is paid to the persons already employed; but the two parties are prevented from coming to terms on that basis by the fact that those already employed would go out on strike if the additional contingent was accepted at a lower rate than that which they themselves are receiving.

Now one of the commonest causes of such a situation is a falling off of demand for the products of a particular branch of industry. The fact that the demand for any product, let us say coal, for example, falls off, is a good reason for fewer persons being employed in that branch of industry and more in other branches. If the diminution of demand is very gradual, the necessary reduction in personnel can be effected by a cessation of recruiting. Many a branch of industry has gradually wilted away in this manner without much inconvenience or hardship to anyone. But if the diminution is more sudden, unemployment results owing to the natural reluctance of persons skilled, or at any rate experienced, in the particular branch of industry to leave it and try for employment in some other. The thoughtless outsider is apt to say that both the unemployed and those who are still employed in the branch should accept lower wages, and so by cheapening the product, extend the demand for it. As a temporary palliative this may sometimes be reasonable, but it is evidently never the best final solution of the difficulty. It is not reasonable that a trade should be continuously worse paid than others merely because the demand for its products was once bigger than it has become. What the diminution of demand calls for is a redistribution of labour force, fewer persons being allotted to the branch of industry of which the products are less in demand, and more persons to the other branches.

But when population is increasing, absolute diminutions of demand are likely to be somewhat fewer, and somewhat less acute when they do occur, than when population is stationary. If, for example, by the introduction of oil, or more economical consumption, the average person's demand for coal is reduced by one-tenth, in a stationary population the total demand for coal would be reduced by one-tenth ; but if the population in the same time increased 12 per cent., the total demand would be not reduced but slightly increased, and there would be no employment difficulty.

We ought therefore not to imagine that a stationary or declining population will rid us of the trouble of unemployment. It will provide more rather than less reason for promoting mobility of labour in place and occupation, and we shall have to take more care, rather than less, than at present to secure that arrangements which seem superficially desirable do not hinder that mobility.

It is inevitable, I suppose, that the question will be asked, whether cessation of the growth of population is to be regarded as a good or an

evil turn in human history. But the limitations of economics and perhaps of human nature prevent any straight answer being given. Nationalists in each nation want their own nation to increase in comparison with others ; if they think of the others' interest at all, they say and believe that it will be promoted by the predominance of their own nation. We can get no further that way, since the pretension of each is contradicted by the pretensions of the others. If we try to avoid this obstacle by saying that we will ignore national and racial differences, and assume either that somehow the generally fittest will grow at the expense of the others, or that each as well as the whole will have stationary numbers, we still have to face the fact that our conception of the distinction between economic welfare and welfare of other kinds is nebulous in the extreme, and that if it was clearer, we should not know—I think we never can know—how much of the one should be regarded as equal to a given quantity of the other.

Different persons will give different answers. Some agree with Paley that ten persons with sufficient subsistence must be in possession of more welfare than a single millionaire ; others with J. S. Mill that the world turned into a ' human anthill ' would be an undesirable place of residence. The same person will give different answers according to his mood at the moment. Personally, I spent my early boyhood in a town which throughout my life has been the most prosperous in England, and I have long lived in another which, having added motor manufacture to education in its old age, has lately been growing nearly as fast, and sometimes when I contemplate their growth I feel a little like G. R. Porter when he wrote the *Progress of the Nation*, during the period 1800 to 1831. At other times, and I think more often, I regret the open heath and the untouched pine wood which stretched in my early recollection to within a few hundred yards of the Bath Hotel at Bournemouth, and I hate the gasworks straddling the river and the bungalows shutting in the main roads out of Oxford ; then I agree with Mill that it is well that population should become stationary long before necessity compels it.

After all, the increase must stop some time, and watching the effect of the stoppage will be a very interesting experience which I should like to have been born late enough to enjoy.

APPENDIX.

Births and their Ratio to Marriages in England and Wales, 1851–1930.

Year	Births	Ratio	Year	Births	Ratio	Year	Births	Ratio
1851	616	4·36	1878	892	4·51	1905	929	3·61
1852	624	4·30	1879	880	4·49	1906	935	3·58
1853	612	4·14	1880	882	4·56	1907	918	3·51
1854	634	4·17	1881	884	4·58	1908	941	3·55
1855	635	4·12	1882	889	4·59	1909	914	3·49
1856	657	4·25	1883	891	4·55	1910	897	3·39
1857	663	4·25	1884	907	4·58	1911	881	3·31
1858	655	4·18	1885	894	4·48	1912	873	3·26
1859	690	4·39	1886	904	4·53	1913	882	3·25
1860	684	4·28	1887	886	4·45	1914	879	3·19
1861	696	4·30	1888	880	4·41	1915	815	2·89
1862	713	4·43	1889	886	4·41	1916	786	2·67
1863	727	4·45	1890	870	4·27	1917	668	2·27
1864	740	4·46	1891	914	4·39	1918	663	2·29
1865	748	4·42	1892	898	4·22	1919	692	2·38
1866	754	4·35	1893	915	4·23	1920	958	3·12
1867	768	4·35	1894	890	4·07	1921	849	2·64
1868	787	4·42	1895	922	4·19	1922	780	2·41
1869	773	4·34	1896	915	4·11	1923	758	2·37
1870	793	4·44	1897	922	4·05	1924	730	2·31
1871	797	4·45	1898	923	3·97	1925	711	2·28
1872	826	4·54	1899	929	3·89	1926	695	2·26
1873	830	4·53	1900	927	3·79	1927	654	2.17
1874	855	4·48	1901	930	3·74	1928	660	2.19
1875	851	4·39	1902	941	3·74	1929	644	2·13
1876	888	4·53	1903	948	3·72	1930	649	2·14
1877	888	4·52	1904	945	3·69			

The above table gives the births in thousands for each year and the ratio between this number and a figure for marriages made up of the sum of 2·5 per cent. of the marriages of that year, 20 per cent. of those of the preceding year, and 17·5, 15, 12·5, 10, 7·5, 5, 3·75, 2·5, 1·75, 1·25 and 0·75 for the years before that. In the table for 1851 to 1900, printed in the 1901 Report of the Association, and the *Fortnightly Review* for March 1902, the ratio is calculated for the legitimate births only, but the inclusion of the illegitimate makes very little difference and is defensible. Mr. L. R. Connor, in the course of a much more elaborate study than mine, gives figures for 1892 to 1923 (*Statistical Journal*, May 1926, pp. 562–3), which agree very closely with the above, though his weighting of marriages is rather different and includes thirty years before the date instead of the twelve at which disinclination for further labour caused me to stop.

From 1914 onwards the ratio as well as the number of births is disturbed by (*a*) the absence of men from their homes owing to the War till 1919, and (*b*) by the abnormal mortality of husbands owing to the War. The effect of the second influence in reducing the proportion of births to marriages must, of course, have been steadily diminishing, which makes the decrease in the proportion shown since 1923 the more significant.

SECTION G.—ENGINEERING.

POWER

ADDRESS [1] BY

SIR ALFRED EWING, K.C.B., LL.D., D.Sc., F.R.S.

PRESIDENT OF THE SECTION.

It is perhaps right to warn you at the outset that this is an attempt to kill two birds with one rather large stone. The address has to serve a double purpose. Besides being the usual offering to convention which is expected of the President of a Section, it has the responsibility of being a Thesis, delivered in fulfilment of a trust which was undertaken by the British Association many years ago. The thesis has a prescribed theme. So instead of being free, as presidents of sections generally are, to choose any text, or none, I find a text ready chosen for me. It is taken, as you will presently see, from one of the prophets. Not one of the minor prophets, for the prophecy about which I have to speak was uttered by Sir Frederick Bramwell. Nobody who is old enough to recall Bramwell's commanding presence, his generous proportions, his patriarchal air, his pleasant acceptance of acknowledged leadership, will ever think of him in terms such as the word ' minor ' would imply. Fifty years ago he was Pontifex Maximus in the world of engineering, not because he built bridges but because he spoke with almost papal authority. An opinion by Bramwell could do much to make or mar any enterprise. To-day we have to discuss a forecast which he offered at the jubilee meeting of this Association, a forecast to which he and his contemporaries attached particular importance. We must now assign to it a place in the long list of prophecies that have turned out to be over-statements ; nevertheless, it deserves attention not only as an item in the history of mechanical science, but because in the light of present-day experience we recognise how much there was in it of the vision of truth.

It deals with a subject that is appropriate for a presidential address to Section G. The president of this section cannot but be conscious that he is addressing an audience larger than a mere group of engineering experts. Beyond the professional circle is a fringe, and beyond the fringe a vague and mobile auditory of persons who take a lively interest in engineering questions, whose knowledge, so far as it goes, is real and practical. Among the triumphs of applied science is this that it has transformed the man in the street into a sort of engineer. Society has been mechanised : the noise and oil and unrest of the workshop are become a part of normal life. The language of the expert is no longer his own shibboleth ; it has been taken into the stock of common speech. A little knowledge used to be called a dangerous thing ; now we all have it and are content—or at least obliged—to live dangerously. I need not enlarge on a point of which

[1] See note on page 140.

everybody is well aware. But for myself it carries this lesson, that in speaking from the Chair of the Engineering Section I am now, so to say, broadcasting to a multitude of intelligent amateurs. I hope the experts will forgive me if in what I say they find nothing that is not already familiar. I shall try to speak plainly to the plain man. People who have a preference for the unintelligible will, no doubt, be able to gratify it in at least one other Section.

What has mechanised the world is, more than anything else, the production and the distribution of power. To-day we are concerned with the best ways of converting the heat of combustion of fuel into mechanical or electrical energy. One need draw no distinction in this regard between mechanical and electrical forms of energy, for each may be converted into the other almost without loss. But to get either from heat is another story. At the utmost you can convert no more than a modest fraction of the heat that comes from fuel: in specially favourable cases you may hope to convert something like a third; the rest is scattered beyond recall. How to effect this conversion, through the agency of a heat-engine, is a matter of perpetual interest to engineers. In assessing the merits of the engine they have to think not only of its *efficiency*—that is the fraction of the heat which it succeeds in converting—but also of other characteristics which are scarcely less important: of its fitness for particular types of fuel, its convenience and ease of driving, its reliability, its endurance, its bulk, its weight, its cost. The heat-engines with which we are practically concerned divide themselves into two great classes. First, there are those which use steam for working substance, whether of the reciprocating or turbine type; in them the heat of the burning fuel is generated outside and has to make its way to the working substance through a containing shell. Second, there is the internal-combustion class where the heat of the fuel is generated within the working substance itself. To this class belong the gas, oil, and petrol engines that have sprung into existence within the memory of many persons now living and have profoundly changed our habits and our outlook. Nothing perhaps has done more than the internal-combustion engine to make obsolete the Victorian attitude to life. Between the two classes of heat-engines there is a rivalry; one might almost say a sort of war. Each has fields of operation where its supremacy hardly admits of doubt, but there are disputed regions in which we find a lively contest. As sporting critics we must recognise the merits of both combatants. Here, surely, is a suitable subject for an address from one who is old enough to remember patting the rivals on the head when they were boys. He now draws up his easy chair, taking care not to put it too near the ropes, and proceeds to make such comments as he may about the game.

A quarter of a century ago I presided over Section G and now the honour is unexpectedly repeated: I am taken off the shelf, dusted and set to function. It would seem that the Council's policy in this happy centenary is to trot out some of the veterans, for the entertainment, let us hope, of less fallible youth. Before asking me to act as President of the Section, they had invited me to give the Bramwell Lecture, and I could accept the two duties only through their kindness in allowing one address to serve for both.

To explain the task of the Bramwell Lecturer we must recall the meeting of 1881, when the Association was celebrating its jubilee in the heyday of Victorian prosperity and confidence. It was a jubilant jubilee. Never, perhaps, was applied science more actively progressive. From day to day its achievements compelled attention. Electricity was knocking at the door, bringing a wallet big with gifts, wonderful gifts that established new contacts between the sciences of the laboratory and the arts of social life.

The world of invention was in a ferment; the brew was seething and bubbling. Some of the froth on the surface had to be blown away, but beneath that there were changes in substance which fifty years have strengthened and matured.

Think for a moment of what the late seventies and the early eighties gave to mankind. The telephone, the phonograph, the incandescent lamp, the dynamo in a practical form, the electric motor, the storage battery, the transformer, the internal-combustion engine using liquid fuel, cold-storage and refrigerated transport of food, the idea of public electric supply, the use of alternating currents, the first clear recognition of the potentialities of electricity as an agent for lighting, for traction, for the conveyance and distribution of power. There, indeed, was a dish to set before the potential rulers of a kingdom which was waiting to be explored, where every engineer in the bud might well fancy himself to be a coming king.

> 'Bliss was it in that dawn to be alive,
> But to be young was very heaven.'

Looking back now, it is curious to reflect how poor was the equipment of most of the pioneers. There were, indeed, a few great leaders—a Kelvin or a Hopkinson—who possessed the right kind of basic understanding, who could turn to theory for guidance and had the engineer's instinct to give it application. Here and there was a Ferranti, with vision and imagination to compensate for the lack of formal knowledge. But most of the zealous workers of those days were empirics, groping in what was at best a half light, full of enterprise and enthusiasm and not much more. They could get little help from textbooks. Some of them made what may now seem strange mistakes, and in that way they acquired a costly education.

Among those fertile years I would specially mention 1881, which was the date of Bramwell's prophecy as well as the Jubilee of the Association. Apart from that it marks an epoch. For the world then realised that a problem was at last solved with which it had been much concerned, the problem called the subdivision of the electric light. Before that the electric light had meant the electric arc—a dazzling unit, brilliant, overpowering, capricious, admired out of doors, but quite unfitted for the home. It was a tiger burning bright which declined to become a domestic pet.

Then came Edison and Swan who, working separately, taught us how to tame it by inventing the incandescent filament enclosed within a vacuum bulb. Near the end of 1881 Sir William Thomson (as he then was) lighted his house in Glasgow by means of Swan's lamps, using with

them a storage battery of Faure's cells, the advent of which had been hailed with enthusiasm and had raised unduly high hopes. For prime-mover he chose the new gas-engine of Dugald Clerk which completed its cycle in two strokes, unlike the already familiar Otto engine, which required four. Clerk's engine was itself a novelty the importance of which we have come to recognise. To this day all internal-combustion engines use either the Clerk or the Otto cycle, and for large powers the Clerk cycle has advantages which tend to give it the favoured place.

Fifty years ago the gas-engine was much in the public eye. It had already proved its value in many workrooms. There was still no supply of electricity from public stations, and for a private installation the gas-engine furnished a suitable and exceedingly convenient source of power. It was a day of small things; an engine which developed as much as 20 horse-power was described as a 'king of gas-engines.' Within this modest limit the Otto engine, which dates from 1876, had a well-deserved reputation as an efficient and trustworthy prime-mover that would run with little attention, using ordinary town gas as its fuel. At the Jubilee meeting of this Association Emerson Dowson drew attention to a less costly alternative: he had made what was called producer gas from coke or anthracite, and had run engines with it. He had even demonstrated that with Dowson gas you could get a horse-power from 1 lb. of coal per hour, instead of burning 2 or 3 lbs. at the very least, as you had to do in a steam-engine. From this it was clear that the gas-engine offered immensely attractive possibilities as a generator of power.

Even before 1881 engineers were busy with efforts to improve its efficiency. Fleeming Jenkin, my teacher and professional chief, was struggling to apply to it the regenerative methods of economising heat which Stirling originated in his air-engine a century ago—methods which in the meantime had revolutionised other industries, notably the manufacture of steel. No engineer who gave the matter serious thought could fail to see the advantage of having heat developed within the working substance itself instead of being conveyed to it by conduction through a containing shell; it was, in fact, the failure of the containing shell that ruined Stirling's engine. Another obvious merit of internal combustion was one that Carnot had recognised in the immortal little treatise where he laid the foundations of thermodynamics—the advantage, namely which you secure by supplying heat to the working substance at a much higher level of temperature than can be reached with steam. Finally, there was this broad difference: the gas-engine had the indefinite promise of youth; the steam-engine was an old servant whose limitations were well known. Nobody expected that steam would change its ways. Small wonder then, that the engineers of those days looked to the future of the gas-engine with exaggerated hope.

It was in that spirit that Bramwell made the prophecy we have now, after fifty years, to review. The occasion—as I have said—was the Jubilee meeting of this Association, held at York in 1881. The President of Section G was Sir William (afterwards Lord) Armstrong. His address was mainly on other subjects, but incidentally it contains an exceedingly apt criticism of the steam-engine as they knew it then. He said: 'In expanding the steam we quickly arrive at a point at which the reduced

pressure on the piston is so little in excess of the friction of the machine as to render the steam not worth retaining, and at this point we reject it. In figurative language, we take the cream off the bowl, and throw away the milk.' I shall show later that in the modern use of steam we no longer have to cry over the spilling of milk to which Lord Armstrong referred.

After the president had spoken, Bramwell also gave an address 'on some of the developments of mechanical engineering during the last half-century,' which is printed *in extenso* in the Report. It reviews a great field with the lucidity of which he was a master, dealing specially with applications of the steam-engine, and it includes a section relating to the transmission of power. Electrical transmission is barely touched on: it had, in fact, scarcely begun; but he speaks of transmission of power by means of gas, and in that connection he remarks:—

> 'I think there is a very large future indeed for gas-engines. I do not know whether this may be the place wherein to state it, but I believe the way in which we shall utilise our fuel hereafter will, in all probability not be by way of the steam-engine. Sir William Armstrong alluded to this probability in his address, and I entirely agree—if he will allow me to say so—that such a change in the production of power from fuel appears to be impending, if not in the immediate future, at all events in a time not very far remote: and however much the mechanical section of the British Association may to-day contemplate with regret even the more distant prospect of the steam-engine becoming a thing of the past, I very much doubt whether those who meet here fifty years hence will then speak of that motor except in the character of a curiosity to be found in a museum.'

The view expressed by Bramwell in this remarkable forecast found support in more than one quarter. In the following year Sir William Siemens was President of this Association. After acknowledging the great service which the Association had done to engineering by settling a consistent and practical system of electrical units, and himself suggesting that the unit of electrical power in that system should be called the Watt, he went on to compare the theoretical efficiencies of steam-engine and gas-engine on the basis of the theory of Carnot, and then remarked:—

> 'Before many years we shall find in our factories and on board our ships, engines with a fuel consumption not exceeding 1 lb. of coal per effective horse-power per hour, in which the gas producer takes the place of the somewhat complex and dangerous steam boiler.'

Again, the late Lord Rayleigh, speaking from the Presidential Chair at the Montreal meeting of 1884, says:—

> 'The efficiency of the steam-engine is found to be so high that there is no great margin remaining for improvement. The higher initial temperature possible in the gas-engine opens out much wider possibilities, and many good judges look forward to a time when the steam-engine will have to give way to its younger rival."

Let me quote one more authority. Fleeming Jenkin, lecturing on Gas and Caloric Engines at the Institution of Civil Engineers, in February, 1884, refers to the fact that Dowson gas even then allowed the gas-engine to compete favourably with the steam-engine, and concludes:—

> 'Since this is the case now, and since theory shows that it is

possible to increase the efficiency of the actual gas-engine two and even three-fold, the conclusion seems irresistible that gas-engines will ultimately supplant the steam-engine. The steam-engine has been improved nearly as far as possible, but the internal-combustion gas-engine can undoubtedly be greatly improved, and must command a brilliant future.'

Bramwell himself returned to the question when President of the Association in 1888. In his address from the Chair he repeated the forecast of 1881 with a qualifying phrase or two :—

'At the York meeting of our Association I ventured to predict that unless some substantive improvement were made in the steam-engine (of which improvement, as yet, we have no notion) I believed its days, for small powers, were numbered, and that those who attended the Centenary of the British Association in 1931 would see the present steam-engines in museums, treated as things to be respected, and of antiquarian interest to the engineers of those days, such as are the open-topped steam cylinders of Newcomen and Smeaton to ourselves. I must say I see no reason after the seven years which have elapsed since the York meeting, to regret having made that prophecy, or to desire to withdraw it.'

It is evident that Bramwell took his 'prophecy' very seriously. He was the acknowledged sage and spokesman of the engineering profession, occupying, in that regard, a unique position, such as no one could possibly hold in the more complex conditions of to-day. He was a humourist, and doubtless there was a conscious touch of humorous exaggeration in what he said. But for all that it was an engineering judgment delivered *ex cathedra*, and his judgments were accustomed to command respect.

Finally, in July, 1903, when within a few months of his death at the age of 85, he wrote as follows to the President of this Association. After quoting the words he had used at York twenty-two years before, he proceeded thus :—

'In saying this, no doubt, I then thought I was speaking somewhat hyperbolically, but from the close attention I have paid to the subject of internal-combustion engines, and from the way in which that attention has revealed a continuous and, year by year, a largely increasing development of such engines, I feel assured that although there may still be steam-engines remaining in work in 1931, the output of steam-engines in that year will be but small as compared with the output of internal-combustion engines.'

He added that he wished to keep alive the interest of the Association on this subject, and for that purpose offered a sum, which was to be invested in Consols, and in 1931 was to be paid as honorarium

'to a gentleman to be selected by the Council to prepare a paper having my utterances in 1881 as a sort of text, and dealing with the whole question of the prime-movers of 1931, and especially with the then relation between steam-engines and internal-combustion engines.'

That is the task I am now attempting to discharge. You do not need to be told that the prediction has, in great measure, failed to come true.

Steam is neither dead nor dying. On the contrary, its use has immensely developed both on land and sea. To-day, it is a much more efficient medium than it was for the conversion of heat into work, and you find it actuating engines of vastly greater individual and aggregate power than any that were even imagined when Bramwell spoke. But alongside of that we have wonderful achievements on the part of the internal-combustion engine which go far to justify the enthusiasm that stirred him fifty years ago.

Looking back now, one is amazed at the boldness of his prophetic outlook. It was more than bold; it was almost foolhardy. Remember that he had nothing to go by except the performance of the gas-engine, and that only in very small powers. Gas, whether the ordinary illuminating gas distilled from coal, or the cheaper product of the Dowson process, was the only fuel then in practical use for internal combustion. The oil engine in its various forms, the petrol engine, the Diesel engine—these were still to come.

The gas-engine itself did not develop so fast as those who had faith in it might have hoped. Sir Dugald Clerk, whose invention of the two-stroke cycle was followed up by many other important services to internal combustion, and who has become its historian, tells us that even in 1898 the largest gas-engines then built indicated 220 horse-power. By that time, however, B. H. Thwaite had demonstrated that the so-called waste gases of the blast furnace were a suitable fuel, and this led makers, especially on the Continent, to take up the design of large gas-engines in forms which for some years had a conspicuous vogue. There were examples in which as much as 2,000 horse-power was developed in a single cylinder working on the Clerk cycle, and a four-cylinder engine was built to indicate 8,000 horse-power. Such engines were notable for their great bulk and weight. They were also, for the most part, costly failures. The big cylinders, cylinder-heads and pistons were apt to crack. The difficulty of controlling the temperature of the metal and escaping effects of unequal expansion stopped the construction of gas-engines with large cylinders. Moreover, apart from that check it soon became clear that the chief advance of internal combustion was to take place on different lines, namely by having oil serve as the internal fuel. Gas still plays a useful part, but quite definitely a minor part. The builders of gas-engines have wisely sought security by restricting the dimensions of their cylinders, and confidence in their wares is now restored. Their products have a well-established and considerable market. But nowadays when we would treat of what internal combustion has accomplished, and of its future, we turn not to gas-engines but to engines which use liquid fuel. We think instinctively in terms not of gas but of oil, using that word to include not only the 'heavy' petroleum of the Diesel motor, but the more volatile constituents with a lower flash-point, which in this country go by the name of petrol.

The success of the Otto gas-engine led makers to design engines operating in much the same way but using for fuel a spray of oil instead of gas. Such engines found a place where gas was not available, as in the driving of agricultural machinery. For the most part their fuel was the safe and familiar oil of the paraffin lamp. Like the gas-engine, they were

heavy and they ran at very moderate speeds, such as 200 revolutions per minute. About 1883 Daimler set himself to produce an engine with much lighter working parts which should run at a far higher speed, five times as fast, or more, and should use for fuel an oil so volatile that a carburettor would serve to charge the incoming air with combustible vapour. After successful trials with a bicycle, he applied his motor, in 1887, to drive a car on the road. That was the beginning of a new era in locomotion. The world discovered in Daimler's petrol engine an appliance such as it had not possessed before—a light, convenient, inexpensive prime-mover, yielding amounts of power which were ample for road vehicles, easy to start and stop and regulate, demanding little attention and no particular skill. Before long it gave city streets an altered character and country roads an unsuspected value. Man acquired a new mobility which changed his notions of distance and of time. In due course the petrol engine also achieved the conquest of the air. At the end of 1903, only a few days after Bramwell's death, the brothers Wright took their first flight in a motor-driven aeroplane. It is the petrol engine that must bear the responsibility—the grave responsibility—of having made it possible for man to fly.

The era of the road-motor began with Daimler's experiment of 1887, but a good many years were to pass before it took the dominant position it holds to-day. The horse was already in possession, and did not yield without a struggle. That sensitive animal had a frank dislike of the horseless car. To meet his objections our legislators—not much wiser, it would seem, then than now—ordained for mechanically-driven vehicles a pace not exceeding 4 miles an hour, and required each of them to be in charge of three persons, one of whom should carry a red flag in front. Not till 1896 was the Red Flag Act repealed. The sinister emblem has gone, and the horse has nearly gone, too. But engineers will not let his memory perish. Thanks to the initiative of James Watt, they treasure his name in one of their most necessary words. The horse may become little more than an instrument of sport or an excuse for betting, but it is safe to say the horse-power will never die.

I have yet to mention another milestone in the history of internal combustion. It was soon recognised that the efficiency of the action depended on the extent to which the combustible mixture in the cylinder was compressed before it was fired; the more compression the greater was the subsequent expansion in the working stroke, and consequently the higher was the efficiency. But a practical limit was set by the danger that the mixture would automatically ignite before the proper time if too much compression were attempted. Users of petrol engines know this danger well; often they try to diminish it by introducing what are called dopes. It was not in petrol engines, however, but in heavy oil engines that Rudolph Diesel initiated an epoch-making change about 1895. Instead of compressing a combustible mixture, he compressed the air alone, bringing it to a very high pressure, and thereby making it so hot that when the charge of oil was forcibly injected at the dead-point there was instant ignition. This escaped all risk of pre-ignition and greatly augmented the efficiency of the action, as a thermodynamic consequence of the very high temperature at which the fuel gave up its heat. To

force the fuel in, he at first employed an auxiliary supply of still more highly-compressed air, but this plan is now less common than the simpler one of using a high-pressure pump, which delivers the oil in a spray of exceedingly fine drops. The essential feature of the engine is that the fuel does not enter the cylinder until the air there is highly compressed and the working stroke is about to begin. It is this feature which has made the Diesel engine the most efficient of all known means of obtaining mechanical work from the combustion of fuel. When I say the most efficient, I am using the word in its thermodynamic sense ; other factors obviously enter when you come to consider questions of mechanical simplicity, of suitability for a particular purpose, or of cost.

As a small-power prime-mover in situations where electric supply is not available, such as country houses, farms, or isolated workshops, the convenience of the internal-combustion engine has, in fact, led to its almost universal use in preference to steam.

We have still to speak of how, for larger uses, the steam-engine has held its own during this half-century of change. Before doing that, however, it may help us to realise the other side of the matter if we imagine our prophet of 1881 brought back to earth so that he may see for himself in what measure his expectations have been fulfilled.

He will come, of course, by aeroplane, and on the way the pilot will tell him of the part which the internal-combustion engine played in the War ; of submarines, and road-motor transport, and tanks and aircraft. He will be told of Zeppelins and air-raids, of the horrible superiority of attack over defence that characterises modern war. He will learn how prodigiously man has increased his power to kill his fellows and destroy their works. The old gentleman will be saddened to think that the world owes this to engineers, and especially to the internal-combustion engine. It will grieve him to reflect that the island safety of England has departed, never to return. On the other hand, he will be told of air-mails to India and Australia and the Cape, and it will interest him much to learn that the engine which is bringing him so swiftly and comfortably to earth weighs no more than a couple of pounds per horse-power, and that engines of much the same type, but lightened and tuned to the uttermost for racing, can develop more than a horse-power for every pound of weight. He will hear, perhaps with less enthusiasm, of speed records by air and sea and land, amazing records which are set up only to be broken. 'Brief life is here our portion' might be said of the records, and also, alas, of many of the record makers and record breakers. As he approaches London our aerial voyager will note the highways thick with motor-cars, coaches and lorries, and will wonder for a moment what has happened to the railway shares he left behind, doubtless selected as a secure investment of the terrestrial fruits of his industry and thrift. For in Bramwell's time there were still people who practised these now exploded virtues, and there were even Chancellors of the Exchequer who encouraged them.

We may imagine that instead of landing at Croydon the pilot brings him over the river and the docks, where he may see big motor-ships like the Nelson liners arriving with their frozen or chilled cargoes. One of his pet bits of engineering was mechanical refrigeration, and he will

take particular satisfaction in noticing ships that are not only driven but cooled by internal-combustion engines. And from the docks they will proceed over the City, where at every crossing he will observe the congestion of motor-cars and taxis, and the multitudinous motor-bus—but never a growler, which was the vehicle he used to favour. I well remember his taking me to visit a cold store on the south side of the river; we were on our way in a growler when the bottom fell out and we were left sitting in the road. He was, as I have hinted, no light weight; my part in the comedy was only that of the last straw. The cab stopped without injury to life or limb, Bramwell forming an effective automatic brake. His genial dignity suffered no eclipse. His spirits were undamped—and his person too, for luckily the street was dry.

Finally, let us think of the pilot bringing him over Waterloo Place to revive his memories of the beloved club where he used to spend many placid hours. Below him will be the Athenæum, more than ever a haven of rest for the mature, but now on the outer edge of a vortex which is fed by torrents of one-way traffic from the Haymarket and Trafalgar Square—a veritable inferno of internal combustion—an inferno that would be intolerable were it not tempered from time to time by authoritative outstretchings of the arm of the law. As he watches the maelstrom, and perhaps sees a bishop trying to reach the club, he will thank the fate which has removed him from the present-day terrors of the pedestrian, from compulsions to unseemly agility and temptations to unseemly profanity. Such temptations are, of course, only for laymen, but life in Waterloo Place, even for bishops, must sometimes be furious as well as fast.

When all these things have been seen, you must not imagine Bramwell posing as the satisfied prophet who complacently remarks 'I told you so.' He had too judicial a temper for that. He would want to know about other users of power, and would ask many questions. What about our navy, and other navies, and what about the biggest liners and the ocean tramps, and what about the railways and the great factories and the coal pits with their plant for winding and ventilating, and so forth, and what about the distribution of light and power from central stations—on what kind of prime-movers do these rely? And the answer would be steam, and steam, and yet again steam. He would soon learn that steam still does a great part of the work of the world, and that one need not go to the Science Museum at South Kensington to find specimens of its remains. But if he did go to the Science Museum (and let me say it is a pilgrimage no visitor to London should miss) he would see among the admirably displayed exhibits some remarkable engines. Side by side with the mementos of Newcomen and Watt, those fascinating heralds of the dawn, he would see engines of a far more recent type, enshrined there in the honour they so well deserve, not as relics of an obsolete past but as precursors of a modern era, the era which was opened to the world by the genius of Charles Parsons. For among the treasures of our national museum of science is Parsons' first steam turbine, which dates from 1884, also the first, or nearly first, turbine to which he fitted a condenser. which dates from 1891, and also a part of his famous little craft, the *Turbinia*, by which in 1897 he demonstrated the applicability of the steam turbine to the propulsion of ships.

These dates, as you will notice, are all subsequent to Bramwell's prophecy of 1881. Many factors have contributed to prevent that prophecy from being fulfilled, but none have been so potent as Parsons' invention and development of the compound steam turbine. That invention was no isolated event—no mere throwing out of a happy thought. It was the life work of a man who, to an extraordinary degree, combined creative imagination with energy and persistence and practical skill. The recent death of Parsons has deprived this Association of a famous past-president and a generous friend. Section G in particular mourns the loss of the most illustrious of modern engineers. It is fitting that we should dwell for a moment on the greatness of Charles Parsons.

The turbine as we know it now is the product of sustained effort and unquenchable faith. The genius of Parsons was indeed of the kind which includes an infinite capacity for taking pains. He was a dreamer of wonderful dreams, but he was tireless in compelling his dreams to come true. He never admitted defeat; a difficulty was a thing to be overcome, an obstacle was merely an incentive. He loved to attempt tasks which most men would pronounce impossible. And he was the severest critic of his own success; he was always striving to better what seemed already very good. These qualities made Parsons perhaps the most successful innovator the engineering world has ever known.

It was my privilege to know him from an early stage in his astonishing career. Forty years ago I was commissioned to test and report on his first condensing steam turbine—sent by people who were disposed to be sceptical of the merits of a thing so queer and so untried. The tests were entirely convincing. Like Balaam when he was commissioned to report upon the Children of Israel, I came back blessing where I had been expected to condemn. That was the beginning of a friendship which was broken only by Parsons' death.

At Parsons' own request, I carried out further tests from time to time on occasions when the steam turbine had reached some notable stage in the course of its development. Among these were trials of the *Turbinia*, before she made her dramatic appearance at the Diamond Jubilee Review in 1897. Her performance established what was then a record of speed for any ship. It prepared the way for a wide adoption of turbine driving in the navy and the mercantile marine. Every opportunity I had, then and later, of seeing Parsons at close quarters, of observing how he would bend his mind to the problem of the moment, increased my admiration of the inventor and my regard for the man. To the last, there was an endearing quality in his self-effacement, in the modesty with which he wore his world-wide fame, which gave him a peculiar charm once the veil of shyness was drawn aside. Now that he is gone, his friends feel that they are sharers in a personal no less than a national loss.

But such a man lives on in his achievements. To Parsons it was granted as to few men to see the fruit of his ideas and his labours. Long before he died the world recognised that he had revolutionised steam engineering. He had taught us how to generate power on a scale and with a concentration never before approached. Nothing, in a sense, could be simpler than his steam windmill with its successive rings of vanes, each in turn taking up a small fraction of the whole energy of the

blast. To conceive such a device was one thing, to give it being and action was quite another. That meant many subsidiary inventions and years of toil; it meant the removal of mountains of prejudice and difficulty. But the triumph is complete. Engineers, all the world over, are wholeheartedly converted. They build their steam windmills on a colossal scale, crowding 50,000 or 100,000, sometimes even 200,000 kilowatts into a single unit, confident in the knowledge that no more trustworthy and economical prime-mover is available for the gigantic stations which play so important a part in modern civilization as centres for the production and distribution of light and of power.

All these stations have come into existence in the fifty years which have passed since Bramwell made his prophecy and in them it has most conspicuously failed of fulfilment. Review the great power stations of the world, and you find their method of manufacturing electric energy from heat is almost wholly through the medium of steam. To illustrate how small a place is taken in them by the internal-combustion engine let me quote some figures for British power stations. A return published in 1930 by the Electricity Commissioners gives the aggregate capacity of the generative plant of various types as follows:—

	Kilowatts
Steam Turbines	5,531,952
Reciprocating Steam Engines	138,806
Oil Engines	71,331
Gas Engines	17,473
Water-power Plant	42,208

You will see that oil engines and gas engines together make up only $1\frac{1}{2}$ per cent. of the whole. And it is the case that abroad, as well as at home, the steam turbine is dominant. Its dominance is the more appropriate because the turbine was invented in the first instance for the express purpose of driving a dynamo. Parsons realized, in the early eighties, that the generating of electricity gave steam a new job to do, a job that needed high-speed rotation, a job for which reciprocating movement was out of place. So he invented the turbine, in which high-speed rotation was a fundamental feature, and he invented also a high-speed dynamo suitable for it to drive, and he patented them both on the same day in April, 1884. The dynamo, he used to say, gave him as much trouble as the turbine. In all the early turbine-driven dynamos it was the armature that was caused to revolve in a gap between the poles of a stationary field-magnet. But later, when a demand came for much greater power and much higher electric potential the parts were inverted: the field-magnet was caused to revolve, and thereby to generate alternating currents of high potential—often many thousand volts—in a surrounding stator which was made up of highly-insulated coils.

The commanding position of the steam turbine is, of course, mainly due to its high thermodynamic efficiency: in large sizes it gets more work out of coal than can be got in any other way. But apart from that, its avoidance of reciprocation gives it an advantage which only those who remember early power stations with their piston engines can fully realise. Again, it can readily be built and run in big units, and another merit

which appeals strongly to the central-station engineer is its wide range of economical working both above and below its normal load ; this specially fits it for the peaks and variations of demand with which power stations have to cope.

It is in the great power stations equipped with large turbines and coal-fired boilers, using steam of high pressure and high superheat, that we find, beyond any question, the most economical production of power. The very bigness of the units tends towards efficiency, but that is not all. The turbine, as a thermodynamic machine, has permitted a far closer approach to the ideal cycle of Carnot than was possible in the reciprocating steam-engine, which, as Lord Armstrong said, skimmed the cream and threw away the milk. In the turbine the steam expands right down to the lowest vacuum that the condensing water will produce, doing effective work all the way, and thereby saving a most valuable and previously wasted portion of the whole heat-drop. Moreover, with the turbine there is a complete escape from the alternate heating and cooling of metallic surfaces which was a source of much loss in engines of the older type. In still another notable respect the turbine cycle approaches the cycle of Carnot : it allows a method of regenerative feed-heating to be adopted in which some steam is ' bled ' at successive stages of the expansion to restore heat to the condensed water on its way back to the boiler. Finally, the steam turbine has immensely widened the range of the thermodynamic cycle by raising the upper limit of temperature through the use of higher pressure and higher superheat. Pressures of 600 or 700 lb. per square inch are now commonplace ; 1,200 lb. is becoming familiar ; 2,000 lb., or even more, is not unknown. Superheating is often carried to 750° or 800° Fahr.—sometimes to 850°, and in rare cases to 900°—and is limited only by the ability of the metallurgist to supply metal that will not ' creep ' too seriously under the combined influence of high pressure and high temperature. The more superheating the better, in this respect, that it tends to reduce the wetness of the steam in the late stages of the expansion and so avoid not only a loss of useful energy but also a tendency on the part of the turbine blades near the exhaust end to be pitted as a result of their impact against water particles. Another cure for wetness is to reheat the steam at one or more stages in its expansion ; with very high initial pressure this becomes necessary, but opinions differ somewhat as to the conditions that make it worth while to carry out so troublesome an operation. We have no time to discuss moot points or to dwell on details, but enough has perhaps been said to show why the steam turbine in fact achieves an efficiency far greater than was known to the steam engineers of Bramwell's day. A modern turbine can generate one electrical unit with a consumption of barely 1 lb. of cheap coal, which means that it converts into electrical energy fully 30 per cent. of the potential energy of the fuel. It is not surprising that the internal-combustion engine finds little favour in power stations, save as an occasional stand-by to assist in meeting the peak load.[1]

[1] Among other merits of steam turbine plant is its comparatively low capital cost. Published figures show that in recently-equipped British power stations the cost, including land, buildings, boilers, turbines, and all electrical machinery is from £14 to £16 per kilowatt of plant installed. For Diesel plant the corresponding capital cost is stated to be from two to four times as great.

Turning now to another field, we find that in railway traction the supremacy of steam is maintained. Higher pressures and the use of superheating have helped to hold it, and the most progressive locomotive engineers have experiments in progress which may carry practice further along these lines. Much attention has been paid to the Diesel engine as a possible alternative, but so far the number of Diesel locomotives that have found employment in main-line working is a negligible proportion of the whole. If the steam locomotive is to disappear, there is no indication that its place will be taken by an internal-combustion rival. What is much more likely is that it will in time be driven out—wholly or in part—by electric traction, as Lord Weir's Committee has recently suggested for the British railways. But electrification will mean that the prime-mover is still steam, though acting at a central station—except, of course, in countries which have available reserves of hydraulic power.

Such a country is Switzerland, and there the transformation from the steam locomotive to electric traction is already almost complete. The playground of Europe has lost little or nothing of its charm through becoming dotted with hydraulic power houses. Already its exports to less favoured neighbours include many million units of electric energy which it delivers through the graceful catenaries that girdle its mountains and span its valleys. The shrewd inhabitants doubtless demand a remunerative price for exported electricity, just as they quite properly do for the other amenities of their delightful land. A time may come when subterranean stores of coal and oil run low, but so long as the sun shines and the rain falls mankind will be able to continue its struggle for existence, though it may suffer a change in the centre of gravity of its industrial life. Industry will learn, like the Psalmist, to look to the hills from whence cometh its help, and Geneva will be more than ever the natural rallying point of a community of nations, physically linked by a comprehensive ' grid ' on which they depend for whatever modicum of light and power they are still permitted to enjoy.

For road motors, the internal-combustion engine is, of course, supreme; it has created as well as supplied a vast demand. Mr. Ricardo, writing in 1928 [2] said that the output of high-speed internal-combustion engines exceeded by more than ten times the total horse-power of all power stations, ships and railways. A statement at the World Power Conference, held in Berlin in 1930, gave the number of motor cars on the world's roads as thirty millions, with an output of at least 600,000,000 horse-power. I have not attempted to check these estimates; I do not suspect them of exaggeration; I am only thankful that many of the engines referred to spend a great part of their time in garages and parking places and are not in a state of continuous activity. They have come into existence to meet a new need; they do not, for the most part, enter into competition with the older uses of steam, except in small ships or by diverting traffic from the railways to the roads. In their own special field—the roads and the air—they have an unchallenged monopoly. And, indeed, they well deserve it. We ought, I think, to pay tribute to the constructive talent that has made these engines the convenient and

[2] H. R. Ricardo on 'Light High-Speed Internal-Combustion Engines,' Institution of Civil Engineers, Engineering Conference, 1928.

reliable prime-movers they have in fact become. Let me quote in this connection some very just remarks by Mr. Ricardo, who has himself done not a little towards securing the admirable results he here describes :

'With the advent of this class of engine there has been a marked change of attitude as regards both the manufacture and the handling of prime-movers. In the past, a prime-mover was regarded as a delicate piece of mechanism, luxuriously housed, and served by skilled engineers trained to anticipate all its needs and to minister to its ailments. To-day, the high-speed internal-combustion engine receives no such special care ; more than half the engines produced are tended by people who have no idea of how they work, and who consider that their obligations are fulfilled so long as they keep one compartment reasonably full of fuel and another of lubricating oil ; it is for such usage that the engine must be designed. A typical example of the modern attitude towards the high-speed engine is to be found in the case of the motor-bus. The modern bus engine is capable of developing 70 to 80 horse-power and of running at piston-speeds exceeding 2,000 feet per minute ; it is placed in the hands of a driver who knows nothing about engineering generally, or of his engine in particular. Such an engine runs 16 hours daily under very arduous conditions and, in the ordinary course of events, it continues so to do for six months before it receives any skilled attention. It is obvious that to withstand such usage an engine must be reliable.'

One may note in passing the remarkably successful efforts which have lately been made towards reducing the weight of high-speed engines which will burn heavy oil instead of petrol, with the consequent advantage (over petrol) of greater efficiency, less weight of fuel, greater safety, and smaller running cost. Light engines of this type open up new possibilities in the air,[3] as well as on the road and on the sea.

This brings me to the last field which must be surveyed in our brief review—the field of ocean navigation. And here we find a situation which is puzzling, unsettled, and difficult to analyse. For in the selection of prime-movers for ocean-going ships, there are sharp differences of opinion and of practice ; there is no sense of finality ; there is even—so it seems to me—a good deal of fashion and caprice, and of the probability of change which one associates with such moods of the mind. I do not suggest that superintending engineers are ever capricious or unreasoning ; indeed, if the matter were really left to them I believe it would soon settle itself ; but even a layman in marine matters knows that a shipping company's policy in questions of propulsion is sometimes governed by other factors than the considered judgment of the superintending engineer.

In our own navy and foreign navies there is a practical monopoly on the part of steam except, of course, in submarines. The advent of the steam turbine, of oil fuel, of gearing between turbine and propeller shafts, of water-tube boilers, of higher pressure, and of superheating—all these progressive improvements have only consolidated the position. Foreign navies have followed the British lead, and, for surface vessels, the only departure from that rule is to be found in a new German cruiser,

[3] See a lecture by Mr. D. R. Pye on 'Heavy-Oil Aero Engines,' *Journ. Roy. Aeronautical Society*, April, 1931.

which is still something of a mystery ship. Except for this rare and as yet unfledged bird, naval usage sticks to steam.

But the mercantile marine is in a state of flux. Before the War there were almost no motor-driven ships. Vigorous efforts had been made to promote the use of gas-engines with producer-gas made on board as it was required, but these achieved no permanent success. The *Selandia*, which dates from 1912, was the first conspicuous example of a large ship driven by Diesel engines. Her economy of fuel at once commanded attention. She was naturally hailed with delight by the powerful oil interests whose position, already strong in the mercantile marine through the extended use of oil under boilers, would become impregnable if the Diesel engine were generally adopted. In some important quarters the Diesel engine became the vogue. During the post-War years of marine reconstruction the number of oil-driven motor-ships rapidly increased, and it is still increasing. In 1930, according to Lloyd's Register, the gross tonnage of the world's shipping was in round figures 69½ million tons, of which 1½ million were survivors from the ancient régime of sails. Of the 68 million tons that were mechanically propelled 60 million tons were steam-ships and 8 million tons were motor-ships. The motor tonnage had increased nearly four-fold in the preceding three years. Of merchant vessels launched during the year 1930, considerably more than half the tonnage was motor-driven ; and even in Great Britain the motor tonnage launched in that year was nearly 52 per cent. of the whole tonnage launched. For the moment the motor-ship is in the ascendant. At this rate, a superficial observer might fancy that steam was in process of being driven off the high seas. But if that were his conclusion I think he would be quite wrong.

If you look at the list of shipping in detail you will notice several things. One is that none of the greatest and fastest ships are motor-driven—neither the *Leviathan*, which at present heads the list, nor any of the other leviathans of the deep, with their tonnages of 40,000 or 50,000 tons or more, and their speeds ranging from 20 to 28 knots. And this is true not only of the older ships but also of the newest, such as the *Europa* and the *Bremen* and the *Empress of Britain*, and the giant Cunarder which is now on the stocks and is confidently expected to eclipse them all. For such vessels, motors do not give the concentration of power that is needed whereas turbines do give it, and give it easily. From Lloyd's list of marine engines under construction at the end of March, 1931, I find that the average shaft-horse-power of the (51) turbines is nearly 20,000, whereas that of the (328) motors is not much over 2,000. The present fashion, if one may call it so, is to put motors into ships of moderate size and power. The same list gives 189 as the number of reciprocating steam-engines under construction; these are mainly for comparatively small and slow ships, for their average horse-power is of the order of 1,000. The ocean tramp is perhaps regrettably conservative in the manner in which she uses steam, but for ships of larger power the advantage of the turbine is too conspicuous to be ignored.

Of motor-driven ships many are tankers, a type that has been called into existence by the world's demand for transport of oil in bulk, both for internal-combustion engines and for burning under boilers. Of

tonnage launched during 1930 the tankers constitute more than 30 per cent. of the whole output. About seven-eighths of these are motor-driven; they carry oil and, naturally enough, consume it. Motor-driven tankers account for half the world-aggregate of motor tonnage launched in 1930.

It is when we turn to vessels of intermediate types, to cargo liners and passenger liners which, though not of the largest class, are still notable ships, often catering for the luxurious traveller, that we find the liveliest contest between the steam turbine and the Diesel motor. And here one notes the curiously potent influence of nationality and of what may be called the accident of ownership. Some nations, such as Denmark, Norway and Sweden, conspicuously favour motors. Others, such as America, no less conspicuously favour steam. One feels that both cannot be right. Nor can British practice, which is much divided, be right either. The choice would sometimes seem to depend more upon the taste and fancy of some dominating personality than upon a careful weighing of arguments such as appeal to engineers. One finds some shipowning companies going strongly for Diesel engines and other companies going no less strongly for steam. A notable example in the steam group is the Canadian Pacific Company, whose superintending engineer, Mr. J. Johnson, has communicated to the Naval Architects a very full statement of the grounds which have governed that company's engine policy.[4] His paper deserves careful study; I have not been able to find any equally detailed and convincing statement on the other side.

A point which will be plain to any reader of Mr. Johnson's paper is this, that to make a fair comparison of performance you must take both types at their certainly-attainable best. You must not compare modern Diesel engines with steam-engines of an antiquated type, but with turbines working under conditions that have been demonstrated to be practicable at sea, where high pressure and high temperature, with water-tube boilers, pure distilled water, no oil in the steam, and sound condenser tubes maintain an efficiency comparable with that which can be reached in the internal-combustion engine. It is right to recognise that the competition of the turbine with the Diesel engine has helped to develop on board ship an improvement in the efficiency of steam for which the way was already prepared by the experience of central stations ashore. Largely through the preaching and example of Parsons, we have learnt that the future of steam in marine propulsion depends on high pressure and high superheat. The experience of Mr. Johnson rubs this in.

When we attempt to appraise the merits of the rivals and to estimate their chances in the more distant future, we see that from the thermodynamic point of view the Diesel engine still has a small advantage. On the other hand, its oil is more costly than fuel oil for boilers, it must have lubricating oil, too, and the first cost of the engine is substantially greater than that of steam plant. In respect of weight and of space occupied there is not much to choose; when account is taken of all accessories there is, perhaps, a slight advantage on the side of steam. As to durability, I cannot speak; so far as I know, there is still a dearth

[4] 'The Propulsion of Ships by Modern Steam Machinery.' *Trans. Inst. Nav. Arch.*, 1929. Vol. 71, p. 39.

of published facts about the cost of upkeep with Diesel engines. *Prima facie*, the great number of reciprocating parts is a serious drawback. There must be a great number because the safe limit of cylinder size is soon reached, and it is only by having many cylinders that any large aggregate of power is developed. In a recent Diesel-engined liner of the luxurious type, a ship of some 17,000 or 18,000 tons, 12 Diesel cylinders operate on each of four shafts, making 48 in all, to produce a speed of 18 to 20 knots. Besides these 48 main cylinders, there are 24 more which serve purposes that are auxiliary but essential to the working of the main engines. Consider the number of working joints, of valves, of valve-rods and tappets, besides pistons and connecting-rods, which this involves. Does such an accumulation of reciprocating pieces with their hammer-blow accelerations mark a real engineering advance as compared with the cosy hum of a turbine engine room, and has it come to stay? Frankly, I think not.

As a last technical point I would say a few words about fuel for marine engines. Can anything be done to re-establish the ancient connection of the merchant service with the British coalfields? Remember that here and in most other places, the cost of coal is substantially less than that of oil, for the same quantity of heat. Where oil scores is in its greater convenience of handling. Much has been said and written about restoring prosperity to the miners by converting coal into oil. As a chemical operation, it is quite possible to make oil from coal; as a commercial proposition, it is impracticable, so long as nature continues to supply oil directly from the bountiful stores on which man now draws with careless and prodigal ease. Ships that burn oil must have it come to them from sources outside Great Britain. Well, then, can we expect ships to return to the use of coal as fuel? For some classes of ships I think we may, though not all classes. Neither in the navy nor in what one may call the upper division of the mercantile marine—the luxurious express liners which carry fastidious passengers and must keep to a time-table that means quick fuelling—can one expect a reversion to coal so long as oil fuel can be got at anything like its present price. But with cargo-liners and big cargo-boats, the case is different. They do offer a possible field for the use of coal, a field where I believe its use would be economically sound as well as of great national advantage. In the running of such ships, the incidental conveniences of oil fuel count for less. The cost of fuel is a relatively big factor and there is a clear advantage in being able to burn either coal or oil at option, according to the local cheapness of supply. There the geared turbine with coal fuel can more than hold its own provided the steam plant embodies the conditions that make for high efficiency, conditions which are now known to be attainable in marine practice. I think those engineers are right who contend that for such ships a highly economic mode of working would be to use pulverized coal for steam-raising in a small number of large boilers of the water tube type, with a pressure of say 500 lb. and a temperature of 750° F., each boiler having its own pulverizing mill and being fitted also for burning oil as an alternative fuel. In such a scheme, there would be no untried elements, but the combination of the elements would be experimental, and a conclusive demonstration of its advantages can be obtained only by testing it out

on a large scale in sea-going ships, trading on more than one route. An experiment of this kind is well worth the making. It is a matter of national moment to help a threatened industry by finding an increased use for coal; that aspect of it should not be overlooked by those who are willing to subsidise industrial research. Such expenditure would be a casting of bread upon the waters with a good prospect of its ultimate return.

And now, ladies and gentlemen, we take leave of our prophet of 1881. We may fancy him borne aloft in a chariot whose fires are unseen only because they burn within the cylinders. If we were to catch from him the mantle of prophecy, we should wear it ruefully; we should all be Cassandras or Jeremiahs, obsessed with the cheerlessness of the industrial outlook, and finding no escape from the conviction that the easy supremacy of Britain, as Bramwell knew it, can never be recalled. But my last word must not be an unqualified Ichabod. The engineers of to-day have as much courage and enterprise as their fathers, and they have an infinitely better understanding of the scientific principles on which, as on a smooth highway, the advance of engineering must steadily proceed. Moreover, to recognise evils, and the causes of evils, may be the first step towards their cure. The world has learnt, through a sharp lesson, that the gifts of the engineer are good gifts only if they are wisely used; that the new powers he has evoked have brought new dangers against which mankind must resolutely guard if it is to save its soul alive. The malignity of individuals and the madness of nations now command forces of destruction such as more primitive communities never knew, and were happier not to know. And apart from clamant and appalling abuses of gifts which ought to be beneficent, we have become aware of a more subtle and perhaps graver social menace. We see the mechanised arts of production over-reaching themselves, supplying commodities in a volume which cannot be absorbed, and with a facility that tends to deprive man of his richest blessing to body and spirit—the necessity of toil.

But these thoughts take us too far afield. They point to problems now conspicuously urgent, which, for the salvation of society, the engineer, the economist and the moralist must jointly set themselves to solve.

NOTE.

In 1903 Sir Frederick Bramwell established a trust in the Association under which at the Centenary Meeting ' a paper . . . dealing with the whole question of the prime movers of 1931, and especially with the then relation between steam engines and internal combustion engines,' should be prepared by ' a gentleman to be selected by the Council.'

The preceding Address discharges this trust.

SECTION H.—ANTHROPOLOGY.

THE PRESENT POSITION OF ANTHROPOLOGICAL STUDIES.

ADDRESS BY

PROF. A. R. RADCLIFFE-BROWN,

PRESIDENT OF THE SECTION.

In this address which I have the honour to make to you as the president of this section, I shall lay before you certain considerations as to the present position of anthropological studies. It might perhaps be regarded as my duty to make a survey of the history of these studies and what has been accomplished in them during the hundred years over which we are now, as an Association, looking back. But this address had to be written during a journey from one side of the world to the other, so that it was not possible for me to have access to the necessary books. Moreover, as between looking back over the past and looking forward to the future, I have a temperamental preference for the latter.

Anthropology, as that term is currently used, as for example in defining a university curriculum, is not one subject, but includes several somewhat related subjects while excluding others not less related. If we define anthropology as the science of man and of human life in all its aspects, then it is obvious that psychology, as the study of the human mind or human behaviour, must be included in anthropology between human biology, which deals with man's physical organism, and social or cultural anthropology, which deals with his social life. Yet actually not only is psychology not commonly included in what is called anthropology, but there is very little systematic co-ordination between psychological and other anthropological studies. The reason for this lies in the history of psychology, which was first developed in close relation with, or indeed as part of, philosophy. It is only gradually that psychology has been differentiated from philosophical studies, and by adopting precise methods similar to the experimental methods of the natural sciences has established itself as an independent scientific discipline. It seems to me that the time is now ripe for psychology to sever its connection with the philosophical subjects of logic and metaphysic and bring itself into closer relation with anthropology. This is not merely a question of a logical arrangement of the sciences. Both psychology and the other anthropological sciences will benefit greatly by a more systematic co-ordination.

Leaving aside psychology, then, we now find the general field of what is called anthropology divided into three separate portions. One of these

may best be named Human Biology, for the term Physical Anthropology is commonly applied in a somewhat narrower sense to cover only part of that field. In one part of this field, in Human Palæontology, we have witnessed in the last fifty years many important discoveries, of which the latest, Dr. Davidson Black's determination of *Sinanthropus pekinensis*, it certainly one of the most significant. In another part of Human Biology, the study of comparative racial anatomy, which is what is usually understood by the term Physical Anthropology, a great amount of work has been done in the way of measurements on the living subject and in the study of skeletal material. I cannot help feeling myself that the results obtained have not been by any means proportionate to the time and energy expended. I believe that one of the reasons has been the preoccupation with attempts to reconstruct the racial history of mankind, when we have as yet no precise knowledge of how varieties of the human species actually come into existence. I think we ought to look forward in the field of Human Biology to a closer co-operation of comparative racial anatomy with Human Genetics, and also to a further development of comparative racial physiology, in which so far much less work has been done than in anatomy.

The natural and most useful association for Human Biology is with the other biological sciences, with general biology, the results of which it has to apply to, or verify in, the human species, with comparative morphology and physiology, and with palæontology. There is much less benefit to this subject in a close association with prehistoric archæology or with social anthropology.

Human Biology (or Physical Anthropology) and Social Anthropology meet together in connection with two sets of problems. One of these is the effect of social institutions on the physical characters of a population. This study seems to me to fall within the sphere of Human Biology rather than in that of Social Anthropology, for it requires to be handled by one who is by training a biologist. The other problem is the reverse of this, namely, the discovery of what differences, if any, in culture are the result of racial differences, *i.e.* of inherited physical differences of different peoples. Now this problem, or this set of problems, can only be approached by means of a study of comparative racial psychology, or the comparative psychology of peoples. For it is obvious that any inherited physical differences between races will act chiefly through psychical differences in any effect they may have upon culture. Thus, the recent researches of Prof. Shellshear bid fair to enable us to define certain morphological differences of the brain as differentiating the Australian aborigines from the Chinese, and the latter in turn from Europeans. The determination of what mental differences are correlated with these differences of cerebral structure is a task for the psychologist or psycho-physiologist.

Comparative racial psychology, which is thus closely connected with Human Biology, is a subject of great difficulty in which little progress has been made as yet. The first task is that of providing a technique for determining with as much precision as possible the average psychological differences between different populations. Many of these differences are very obviously the result of differences of culture, and the ultimate task of such a study, of proving that certain observable psychological differences

are correlated with differences in the physical organism, and are therefore strictly racial differences, is one that we cannot yet hope to approach as a scientific problem.

Another field that lies within the general field of Anthropology as now organised is that of Prehistoric Archæology. I need not remind you how greatly this subject has developed and prospered in recent years. It has won far more popular interest and support than any other branch of Anthropology. At the same time it has become more definitely a specialised study. It has thus attained an independence that it did not possess when anthropological studies were first organised in associations and universities.

Besides these two subjects, Physical Anthropology, or, as I think it might be better called, Human Biology, and Prehistoric Archæology, Anthropology as now organised includes as a third field the study of the languages and cultures of non-European peoples, and particularly of those peoples who have no written history. This separation of the peoples of the world into two groups, one of which is studied by the anthropologist, while the other is left to historians, philologists and others, is obviously not justifiable by any logical co-ordination of studies, and is no longer so fully justified by practical considerations as it was when it first arose. Changes that are taking place in this field will soon require, I think, a different organisation of our studies in relation to others.

It is to this branch of anthropology, the study of the cultures of non-European peoples, that I wish to devote my attention in this address. Of the changes that have recently been taking place in it, which are important and significant for its future development, there is one which I will here only mention and will return to it later. In its earlier development the study was a purely academic one, having no immediate bearing on any particular aspect of practical life. This has now changed, and there is a growing recognition that the study of the life and customs of a tribe of Africa or New Guinea by an ethnographer or social anthropologist can be of practical assistance to those engaged in governing or educating that tribe. Anthropology, or this branch of it, is now being brought into close relation with colonial administration, and we may anticipate many important results from this association.

This new position of anthropology will, I believe, help to hasten forward the development of a change of point of view in the study, a change of orientation, which has been slowly making itself felt during the last few decades, and with which I propose to deal at some length. I will attempt to state in a few words what this change of orientation is. Using the word science to mean the accumulation of exact knowledge, we may distinguish two kinds of scientific study, or two kinds of method. One of these is the historical. The other method or type of study I should like to call the inductive, but there is a chance that the word might be misunderstood. I will therefore call it the method of generalisation. This distinction between the historical and the generalising sciences was emphasised long ago by Cournot. It is one of great importance in any question of scientific methodology.

Now when the study of non-European peoples was first undertaken, it

was very natural, and indeed inevitable, that it should be treated by the method of the historical sciences so far as those methods were applicable. But during the past hundred years there has been a steadily growing movement towards the creation of a generalising science of culture or society. The moment has come when the existence and independence of this science should be recognised.

I have said that in the early stages of the study of non-European peoples the approach made was that of the historical point of view. One of the tasks of history is to give us accurate descriptions of a society or people at a given time. The ethnographer's work of describing to us a non-European people was taken up precisely in this way. But history also gives us chronological accounts of the changes in a people's life. For the European peoples we have written documents that enable the historian to do this. For many non-European peoples we have no such records. The ethnologist, true to the assumption that history was what he wanted, engaged in the attempt to supply a conjectural or hypothetical history.

The procedure began in the eighteenth century, when attempts were made to identify native peoples in different parts of the world as the descendants of the ten lost tribes of Israel, or similarities of custom with ancient Egypt were interpreted as the result of Egyptian influence. The identification of the lost ten tribes of Israel seems to be no longer the concern of anthropologists, but the ingenious tracing of the most diverse customs all over the world to a hypothetical origin in Egypt still survives, and, as it seems to possess a strong emotional appeal for certain minds, will probably persist.

Towards the end of the eighteenth century, with Adam Smith and others in England and France, the hypothetical reconstruction of the past took another form. It was supposed that in some sense the less developed peoples represented early stages in the development of our own culture. The aid of knowledge about them was therefore called in to help in creating a conjectural history which dealt with such general matters as the origins of language or of civil government, and so on.

Thus from early times the attempts to utilise information about non-European peoples took two distinct forms. It will be convenient to have different names by which to distinguish the two studies, and I shall use the word ethnology to refer to one and shall speak of the other as belonging to social anthropology. This conforms fairly well to the ordinary usage of these two terms.

Ethnology, in the sense in which I am here using the word, is concerned with the relations of peoples. If we study the existing peoples of the world, and those of the past about which we have information, we are able to define certain similarities and differences in racial characters, in culture and in language. The ethnologist may confine himself to determining as precisely as possible these similarities and differences and so providing a classification of peoples on the basis of race, language and culture. If he seeks to go further and explain them he does so by hypothetical historical processes. It is evident that throughout the period of human life on the planet there have been movements and intermingling of races; there has been spread of languages, and the subsequent differentiation of one language into several distinct languages; and there have been movements

of whole cultures with the migration of peoples from one region to another, or spread of particular elements of culture through the interaction of neighbouring peoples. The present situation of the peoples of the world, or the situation at any moment of history, is the result of the total series of changes that have taken place over some hundreds of thousands of years. The aim of the ethnologist is to make hypothetical reconstructions of some of these processes.

Ethnology, as thus defined, is a historical and not a generalising science. It is true that in making their historical reconstructions the ethnologists often assume certain generalisations, but as a rule little or no attempt is made to base them on any wide inductive study. The generalisations are the postulates with which the subject starts, not the conclusions which it aims to attain as the result of the investigations undertaken.

Social anthropology, in the sense I am giving to that term, has concerned itself with a different type of problem. It has interested itself in the development of institutions in human society. From its earliest beginnings it attempted a sort of compromise between the two different scientific methods, the historical and the generalising. Undoubtedly one of the aims of Social Anthropology has been to understand the nature of human institutions and, if I may use the phrase, how they work. But instead of adopting outright the methods of the generalising sciences, social anthropology was dominated by the conception of history, of historical explanation and the historical method. And since historical records were insufficient it endeavoured to make a hypothetical history of institutions and of the development of human society. It discussed such matters as the origin of language and of religion, the development of marriage and of property, the origins of totemism and exogamy, or the origin and development of sacrifice or of animistic beliefs.

Social anthropology frequently sought the origins of social institutions in purely psychological factors, *i.e.* it sought to conjecture the motives in individual minds that would lead them to invent or accept particular customs and beliefs. Its explanations were frequently, or even usually, historical in one sense, but psychological in another, almost never sociological. This point will be returned to later.

Throughout almost the whole of the last century this historical-psychological method so dominated anthropological study that it was hardly possible for any one to escape from it. Thus, when Robertson Smith laid the foundations of the scientific study of religions and took up the problem of the nature of sacrifice, (for that, as we should now see it, was really the problem,) he was not content to isolate and classify the different varieties of sacrifice, and show their relation as different forms of a widespread type of religious rite—that would be the method of the modern sociologist, as represented in the essay of Hubert and Mauss—but the strong tradition of his time made him attempt to fit the different varieties of sacrifice into a scheme of historical development whereby one variety was supposed to have had its origin in another.

The compromise that social anthropology made between the historical and the generalising methods was one impossible to maintain. As a result there have been in the last few decades two movements, one towards ethnology and the other towards sociology, and the traditional social

anthropology has been subjected to criticism of different kinds from these two quarters.

Throughout almost the whole of the nineteenth century there was little distinction between ethnology and social anthropology. Tylor, for example, combined the two studies. It is true that some writers followed by preference one study to the exclusion of the other. Thus, Sir James Frazer has rarely concerned himself with ethnological problems. It is also true that the two methods occasionally came into conflict over particular problems, but this conflict did not become one between the two methods and the two points of view.

Towards the end of the last century and in the earlier part of this century there developed, in America, in Germany and in England, schools of ethnologists which, while disagreeing amongst themselves on particular questions of historical reconstruction, and even on the methods of ethnological analysis, yet all joined in attacking the methods of social anthropology from the point of view of historical method. These criticisms of what the ethnologists call 'evolutionary anthropology' are familiar to all of you.

The shift over from social anthropology to ethnology is illustrated in the development of the ideas of the late Dr. Rivers. I think I can speak with some knowledge of Rivers, for I was for three years his pupil in psychology, and was his first pupil in social anthropology in the year 1904. Rivers was from first to last primarily a psychologist, and was an inspiring teacher in psychology. He had no training in ethnology or in archæology, and only gradually made a partial acquaintance with those subjects. In his first period of interest in anthropology, from the time of the Cambridge Expedition to Torres Straits to the year 1909, his conception of the aims and methods to be followed in the study of non-European peoples was that of what I have been describing as social anthropology. Even if he could not regard Morgan's theories, for example, as being satisfactory, he yet assumed that the making of theories of that type was the task of the anthropologist, and I believe that even up to the end of his life he still accepted in general outline the animistic theory of Tylor and Frazer. Ultimately, during his work in Melanesia, his growing dissatisfaction with that method came to a head, and in 1911, in his presidential address to this section, he declared his allegiance to the ethnological method. In other words, from one type of historical study he transferred his attention to another. In the years 1913 and 1914 I had much discussion with Dr. Rivers on the subject of anthropological method by correspondence and in personal interviews, partly because at that time he did me the kindness to read and criticise, in manuscript and in proof, a book that I was writing. His view at the time our discussions ceased was that, while he was fully prepared to grant the validity and the necessity of the method of comparative sociology, he regarded the method of ethnology as equally valid and necessary and at the same time independent, and that he preferred to devote his own attention to the latter rather than the former. At the very end of his life there were indications that his attitude was changing once more, that he was growing somewhat dissatisfied with the ethnological method which he so stoutly defended in 1911, and that he

was directing his attention to the method which I am here speaking of as that of Comparative Sociology.

In the change of point of view that he made in 1911 Rivers was therefore representative of a general tendency. There had been a growing dissatisfaction with the theories of social anthropology. From the point of view of a desire for historical explanation that dissatisfaction is, I think, justified. A historical study ' explains ' by revealing particular relations between particular phenomena or events. History does not generalise or cannot legitimately do so. It shows us that at a given moment a particular event occurred, and as a result of this something else happened. Thus, a cause in historical explanation is something which happened once and was followed by certain results. It is not similar to what is called a cause in natural science, which is an event that recurs or may recur repeatedly and always produces the same effect. Historical explanation is always concerned with particulars, normally with showing a chronological relation between two or more particulars. The value of historical explanation is therefore directly proportional to the amount of certain and detailed knowledge that we have of the events with which we are concerned.

It may be said in one sense that the ethnologist *explains* the existing similarities and differences between peoples by means of his historical hypotheses. Actually, however, he is not interested, at any rate primarily, in explanation. Where he attempts a reconstruction of history it is because he wishes to discover something about a past of which we have no records in written documents. He is interested in a knowledge about the past, as far as it is attainable, for its own sake. Or if the ethnologist believes himself to be following some other aim, then he is pursuing the wrong method. All that his hypothesis can give him will be a certain number of more or less probable statements about the past. And his results will only be valuable or valid if he avoids basing them on assumptions as to general principles of historical change which have not been demonstrated by sociology, for it is the specific task of sociology to discover such principles.

The methodological difficulty in ethnology is, and always will be, the demonstration of its hypotheses. I do not suppose that anyone has ever accepted, or ever will accept, Rivers's elaborate reconstruction of the history of Melanesia. The theories of culture cycles, that are held so firmly by some ethnologists that they speak of them as though they were demonstrated beyond any possibility of doubt, are totally rejected by other competent and open-minded students. The Egyptian theory of the origin of culture has its special devotees, but so has the Atlantis theory.

It is certain that the ethnological method carefully used may give us a very limited number of highly probable, if not quite certain, conclusions. Thus there is no doubt that the language of Madagascar and a good deal of its culture are derived either from Indonesia or from some region from which the Indonesian languages and culture were also derived. In such an instance we are dealing with a great number of resemblances between the two regions which cannot be otherwise explained, and the matter of the languages is conclusive. Similarly it might be possible to demonstrate some sort of general relationship between Australia and South India, or between Indonesia and Melanesia. But it seems to me highly doubtful

if we can ever obtain from ethnology any considerable mass of proven detailed knowledge of the historical relations of peoples and regions.

I believe that this feeling is shared by many anthropologists whose interest still attaches to history. In the last thirty years or so we have watched the development of several diverse schools of ethnology or culture-history. Some of these have offered us elaborate schemes of reconstruction of the whole of human history; others have dealt with particular local problems. But it is impossible to reconcile the different theories with one another or even to discover principles of method about which there is general agreement. To say nothing of theories of the derivation of culture from a lost Atlantis or a lost Pacific continent, we are offered a choice between the Egyptian theory championed in its latest form by Prof. Elliot Smith, or the theory of culture-cycles of Graebner, or the somewhat different theory of Father Schmidt, or that of Frobenius, and I know not how many more. Each school goes its own way building up its own hypothetical structure, not attempting to seek out points on which agreement can be reached with others. The procedure is often that of disciples of a cult rather than that of students of a science. The result is that many would-be ethnologists, seeing how much hypothesis and how little certainty there is in these reconstructions of history, have been turning to archæology, in which at least some certainty and general agreement can be reached. This movement I think is a thoroughly sound one. Where written documents are absent it is first of all to archæology that we must look to give us some knowledge of the history of peoples and cultures.

If then we set out to study human life by the methods of historical science, we aim at discovering everything that we can of interest about the past. When written records are available we make use of them, and such study is called history in the narrow sense. We may supplement the written records by investigations in archæology. This study has reached a stage when it can give us precise and certain information within a limited field. It can only tell us about those things in the life of a people that can be directly inferred from their material remains. Ethnology can to a limited extent supplement history and archæology.

The historical interest in human life is one of the chief motives for the study of non-European peoples. But the same study offers scope for another interest, the desire to reach a scientific understanding of the nature of culture and of social life. In the past those two interests have been often confused. The progress of our studies requires that they be separated, and this separation has been taking place during the last few decades. Out of social anthropology there has grown a study which I am going to speak of as Comparative Sociology.

By this term I wish to denote a science that applies the generalising method of the natural sciences to the phenomena of the social life of man and to everything that we include under the term culture or civilisation.

The method may be defined as being one by which we demonstrate that a particular phenomenon or event is an example of a general law. In the study of any group of phenomena we aim at discovering laws which are universal within that group. When those laws are discovered they

'explain' the phenomena to which they refer. A science of this kind, as I conceive it, still remains descriptive, but in place of descriptions of particulars and their particular relations, such as the historical sciences give us, it provides general descriptions.

The older social anthropology did not follow this method, at any rate consistently. We have seen that it devoted most of its attention to formulating hypotheses about the origins of social institutions. Nevertheless social anthropology, by its comparative study of institutions, made possible the development of comparative sociology. I could, if I had time, show you how the new anthropology, *i.e.* comparative sociology, grew gradually out of the older study; how the first tentative movements towards this science began in the eighteenth century; how the work of such men as Steinmetz, Westermarck and others, and particularly that of Emile Durkheim and his followers, led step by step to the present position in which we can claim that there is now in existence a comparative sociology which demands recognition as something radically different in important respects from the social anthropology out of which it has grown.

The essential difference between the older social anthropology and the new lies in the kind of theories that one and the other seek to establish by the study of the facts. As I see it, comparative sociology rejects, and must reject, all attempts at conjecturing the origin of an institution when we have no information based on reliable historical records about that origin.

I can only hope to make my meaning in this matter clear to you if you will permit me to refer to a particular example. We may take as our example totemism, which has received a good deal of attention in social anthropology. Totemism is a name which we apply to a large number of different kinds of institutions in different cultures, all having in common the one feature that they involve some special relation between social groups and natural species, usually species of animals or plants. It is to be noted, first of all, that totemism is not a simple concrete thing; it is an abstraction, a name applied to a number of distinct and diverse things which have something in common. What is or is not included under the term depends on the definition we adopt, and different writers choose different definitions.

The older social anthropology concerned itself with the question of the origin of totemism. Even supposing that we have settled what we are and what we are not to include under the term, our question is still not specific. If we try to make it specific we must recognise that there are three possibilities. One is that all the things we call totemism in Asia, Africa, America and Oceania are historically derived from some one particular institution which had its origin in a particular region at a particular time. A second is that some one particular form of totemism may have arisen independently in two or more regions at different times as the result of similar historical processes, and that existing varieties of totemism are all derived from this. The third is that different forms of totemism may have had their origin independently in different regions at different times by different historical processes. If I had to decide which of these three possibilities seemed to me the most likely, I should select

the third. And this would mean, of course, that totemism has not had *an* origin.

In many of the theories of totemism it is difficult to tell whether the author is thinking of the first or the second of the two possibilities mentioned above. Prof. Elliot Smith, however, definitely adopts the first. If I understand him he would regard everything all over the world that he calls totemism (and I am not sure what he would include in or exclude from that term) as being derived in comparatively recent times from Egypt, and from a particular system of beliefs and practices which were the product of the special historical developments of Egyptian civilisation.

Sir James Frazer's final theory of totemism is well known to you. It assumes that all existing forms of totemism are derived from one simple original form. In making an assumption of this kind Prof. Elliot Smith and Sir James Frazer agree, but their agreement goes no further. The particular form selected by Sir James Frazer is what he calls conceptional totemism, the belief that the fœtus in a mother's womb is derived from some food (animal or vegetable) that the mother has eaten. The belief is known to exist in parts of Australia and Melanesia, and I should think that, if it were sought for, it might quite well be found in other regions from which it has not been recorded. This, then, on Sir James Frazer's theory, gives us the historical origin of totemism. It is not clear whether he conceives this form of totemism to have come into existence only once at a particular time in a particular spot, or whether he conceives it as having come into existence in different regions at different times. In completion of this theory he offers us a psychological explanation of the belief which, for him, is the germ out of which all diverse forms of totemism developed. Man, not being aware of the physiological causes of impregnation, but being desirous of finding some explanation, was led to the theory that food eaten by a woman and followed by sickness (the sickness of pregnancy) was the cause of the pregnancy, with which it was thus associated.

I do not intend to offer you criticisms of these two theories of totemism. If criticism is to consist, as I think it always should in science, of a re-examination of the evidence adduced in favour of a hypothesis, I cannot see that any evidence has yet been offered for the historical reality of either of these hypothetical processes. Indeed, I find it impossible to imagine what real evidence of that kind could be discovered.

For comparative sociology, totemism presents a different problem or series of problems. These may be described as being concerned with the nature and function of totemism. To elucidate the nature of totemism we have to show that it is a special form of a phenomenon much more widespread, and we must aim at demonstrating that it is a special instance of a phenomenon or at any rate of a tendency which is universal in human society. For this purpose we have to compare totemism with all other related institutions in all cultures.

From the outset of our inquiry, therefore, we cannot isolate totemism and deal with it as a separate thing. First of all totemism in any given culture is part of a more extensive system of beliefs and customs, and may occupy a preponderant position in that system, as in many Australian

tribes, or may occupy a small and almost insignificant position. In different cultures totemism is not the same thing.

When we examine totemism by the sociological method, the first thing we discover is that it is merely a special example, or rather a collection of special examples, of a larger class, namely of ritual relations established by the society between human beings and objects of nature such as animals or plants, and such things as rain. We find that there are important systems of beliefs and customs establishing such ritual relations which are not included under the term totemism. We find them among people such as the Eskimo or the Andaman Islanders, who have no totemism. The problem of totemism thus becomes a part or aspect of a much wider problem, that of the nature and function of the ritual relations between man and animals and plants in general. Thus, many years ago I wrote what was intended to be a direct contribution to the sociological theory of totemism in the form of a study of the relations between man and natural species in a non-totemic people, the Andaman Islanders.

This problem, however, which is wider than the problem of totemism, is itself merely a small part of a still wider problem, that of the nature and function of ritual and mythology in general. If we wish to know why certain peoples treat wild animals and plants as sacred things, we must discover the general principles on the basis of which things of all kinds are treated as sacred. Thus the problem of totemism, as soon as it is fully stated, leads straight to one of the fundamental problems of Sociology, that of the nature and function of ritual and myth. This is characteristic of the sociological method, that any problem, however small, is part of a general fundamental problem of the nature of culture and of human society.

Nevertheless we must, and we can, partially isolate particular problems for special study. The provisional conclusions we reach will be subject to revision when the particular problem we are dealing with is considered in relation to the general problem of which it is part.

Without attempting the impossible task of trying to fit in to a brief statement the theory of the nature of ritual in general, I think we can formulate one important principle which is relevant to the problem of totemism. This is that in societies in which the whole population, or the major portion of it, is engaged in immediate subsistence activities, those things which are of vital importance in relation to subsistence become important objects of ritual. Perhaps we may be more cautious and say that there is a strongly marked tendency for this to happen. For there are possible exceptions, such as the lack of any record of a cattle cult amongst the Hottentots.

Special examples of this law or tendency are the cattle cults of pastoral peoples, the corn cults of tillage people, and the weather and season cults of peoples of all kinds. The treatment of wild animals and plants as objects of ritual by hunting and collecting people is partly or very largely to be regarded as simply another special example of this general tendency. Other factors come in, with which I have not time to deal, but once we recognise their possibility they need not affect our argument.

We have thus reached one provisional generalisation covering those customs and beliefs of which totemism is a part. But the special character

of what is commonly regarded as the normal form of totemism is that the whole society is divided into segments (moieties or clans), and there is a special ritual relation between each segment and some one or more species. This can also, I think, be shown to be a special example of a general law or tendency whereby in any segmentary structure, which has a religious basis or function, the solidarity of each segment, the differentiation or opposition between the segments, and the wider solidarity which unites the segments into a larger whole in spite of that opposition, are expressed and maintained by establishing a ritual relation between the whole society and certain *sacra* and by establishing a special relation between each segment and some one or more of these *sacra*. Totemism of clans or moieties is only one example of what is a much more widespread general phenomenon in the general relation of ritual to social structure.

There would, of course, be very much more than this in a general sociological theory of totemism. There are a great many different kinds of totemism, and their relations to one another and to the theory would all have to be considered. But the general method would be the same, seeking, in relation to each particular phenomenon we examine, to see it as a particular example of a widespread class.

By pursuing this process of analysis and generalisation we can come to see totemism as a particular form taken by what seems to be a universal element in culture. Every culture that we know has some system of beliefs and customs by which the world of external nature is brought into a relation with society in which the two form a single conceptual structure, and relations are established between man and nature of a kind similar in certain respects to the relations established within the society between the human beings themselves. I am inclined to regard it as one of the essential functions of religion to provide this structure. Our own relations to a personal God who has created or who is regarded as maintaining the natural order, is an example of what I mean. The fully developed or elaborated totemism of a people like the Australian aborigines is an example of the same general or universal process. It establishes a whole system of special social solidarities between men and animals, plants, and other phenomena of nature.

When we have in some such way as this arrived at a satisfactory conception of the nature of totemism we can proceed to a study of its functions. By the function of an institution I mean the part it plays in the total system of social integration of which it is a part. By using that phrase, social integration, I am assuming that the function of culture as a whole is to unite individual human beings into more or less stable social structures, *i.e.* stable systems of groups determining and regulating the relation of those individuals to one another, and providing such external adaptation to the physical environment, and such internal adaptation between the component individuals or groups, as to make possible an ordered social life. That assumption I believe to be a sort of primary postulate of any objective and scientific study of culture or of human society.

When we take up the functional study of totemism, then we must examine in each particular case of a sufficient number, what part the special variety of totemism of a given region plays in the total system of integration which the whole culture provides. We might study in this

way the functions of a number of different varieties of totemism in Australia, and then draw certain general conclusions as to the function of totemism in the general integrative system of Australian tribes. We should not thereby be entitled without examination to draw conclusions as to the functions of totemism in America, or India, or Melanesia, or Africa.

Just as the question of the nature of totemism is part of a very much wider sociological problem, so the study of the functions of totemism is part of the general sociological problem of the function of religion.

The foregoing brief and inadequate statement of how I conceive that comparative sociology will take up the problems of totemism will, I hope, have served the purpose for which it was introduced, namely, to illustrate the difference of method that distinguishes the newer social anthropology from the old. I have chosen the subject of totemism because some of the most important steps of the passage from the old to the new methods are to be seen in Durkheim's treatment of this subject in his 'Elementary Forms of the Religious Life.' Unfortunately, Durkheim retained some of the ideas and some of the terminology of the older social anthropology. He speaks of his study as aiming to determine the 'origin' of totemism, and although he seeks to give a new meaning to the word 'origin,' yet his use of it misleads most of his readers, and I think it really misled Durkheim himself and caused him to cast what is really a theory of the nature and function of totemism into a form which renders it open to criticism, and which has caused it to be misunderstood by many of his readers.

I think we should use the term origin, in speaking of any institution, as meaning the historical process by which it came into existence. Thus we can speak of and actually study the origin of parliamentary government in England. In comparative sociology, if we are to make it the science it should be, we must reject absolutely all attempts to conjecture the origin of any institution or element of culture. Wherever we have good and sufficient documentary evidence as to the origin of anything this can of course be utilised by sociology, but that is an entirely different matter.

I have pointed out that the theories of the older social anthropology often took a psychological form. The procedure was one of conjecturing processes of thought in the minds of individuals which would lead them to adopt a certain belief or custom. I have not time in this address to discuss the subject of the relation of sociology to psychology. There is still a great deal of confusion as to that relation. The position maintained by the sociologist is (1) that in social institutions and in the phenomena of culture generally the sociologist has a field of study which is entirely distinct from that of the psychologist, and that generalisations made in this field must be sociological and not psychological generalisations; (2) that therefore any explanation of a particular sociological phenomenon in terms of psychology, *i.e.* of processes of individual mental activity, is invalid; (3) that ultimately the nature of human social life is determined by the nature of the psycho-physical organism of man, and that therefore when we have discovered universal sociological laws it will be the duty of the psycho-physiologist to discover their basis in psycho-physical processes; (4) that, on the other hand, the behaviour or the psychology of

an individual human being is largely determined by the culture which has been imposed upon him by the society in which he lives.

The sociologist therefore claims that it is possible and necessary to distinguish psychology and sociology as two distinct subjects, just as distinct as physics and chemistry. It is only when the two subjects are so distinguished that it will be possible to obtain real co-operation and co-ordination between them.

The newer social anthropology then, as I see it, differs from the older in several vital respects. It rejects as being no part of its task the hypothetical reconstruction of the unknown past. It therefore avoids all discussion of hypotheses as to historical origins. It rejects all attempts to provide psychological explanations of particular social or cultural phenomena in favour of an ultimate psychological explanation of general sociological laws when these have been demonstrated by purely sociological inquiries. It endeavours to give precise descriptions of social and cultural phenomena in sociological terms, and to this end seeks to establish a suitable exact terminology, and seeks at the same time to attain to a systematic classification of those phenomena. It looks at any culture as an integrated system and studies the functions of social institutions, customs and beliefs of all kinds as parts of such a system. It applies to human life in society the generalising method of the natural sciences, seeking to formulate the general laws that underlie it, and to explain any given phenomenon in any culture as a special example of a general or universal principle. The newer anthropology is therefore functional, generalising and sociological.

Although the newer anthropology rejects much of the methods of the older, and rejects all the theories of origins with the elaboration of which the latter was so much concerned, yet the new anthropology has grown out of the old, would not be possible without it, and starts with valuable knowledge of social phenomena and some insight into their nature which were incidentally provided by the earlier anthropologists in their search for origins. The work of such men as Tylor, Robertson Smith, Frazer, Westermarck, to mention only some of the greatest and of this country only, paved the way for the advance that we are now making. In rejecting the conclusions they reached by what we regard as an unsound method, we do not forget all that we owe to them in the first systematic exploration of the regions we now seek to survey more exactly and with new instruments.

Comparative sociology, as I am here calling the newer form of anthropology, requires a new conception of the aims and methods of field investigations amongst non-European peoples. It is not so very long ago since for most of our information about the life and customs of such peoples we had to rely on the writings of persons who had no training for the work of observation and description, travellers and missionaries principally. It is now recognised that we can no more rely on such information than we could rely on the observations of an untrained person in such a science as geology. The first point, therefore, in relation to field research is that to have its full value for scientific purposes the description

of the culture of a non-European people must be based on the careful work of a thoroughly trained observer.

During the last forty years there has been a considerable quantity of work carried out in this way, particularly in America. Under the influence of Dr. Haddon in England and Prof. Boas in America, a good deal has been done in developing a technique of ethnographical field-work.

It is true that we still meet with persons who regard themselves as competent to carry out such work of observation without the preliminary training. One also still finds writers who quote from accounts of missionaries and travellers, as if their records were as reliable as those of trained specialists.

As ethnographical field-work has become in recent years more systematic, observation has tended to become more extended and more penetrating. Earlier ethnographical descriptions were mostly confined to the more accessible aspects of a culture, its formalised elements. The result was normally a very incomplete picture of the life of a people. Recent work, such as that of Prof. Malinowski or Dr. Margaret Mead, gives us, as the result of more extended and methodical observation, valuable information about what may be called the unformalised aspects of the life of a people such as the Samoans, the Trobriand Islanders, and the Admiralty Islanders. Without information of this kind we can never hope to make full comparative use of any description of a culture.

Comparative sociology involves another and perhaps even more important change in the conception of the nature of field research. On the older view the task of the field-worker was simply to observe the facts and record them as precisely as possible with the help of such concrete material as photographs, texts in the native language, and so on. It was not his business, at any rate as a field-worker, to attempt any interpretation of the data he collected. This he could leave to others who would make it their business.

The conception of the newer anthropology is the opposite of this, and is that only the field-worker, the one actually in contact with the people, can discover the meaning of the various elements of the culture, and that it is necessary for him to do this if he is to provide material to be fully utilised for the purposes of science.

When I speak of the 'meaning' of an element of culture, I use the word very much as we do when we speak of the meanings of words. If we consider an individual, the meaning of a word that he hears or uses is the set of associations that it has with other things in his mind, and therefore the place it occupies in his total thinking, his mental life as a whole. If we take a community at a given time the meaning of a word in the language they use is constituted by the associations normally clustering around the word within that community. Therefore the maker of dictionaries collects examples of the usage of a word and tries to classify and, as far as possible define, the different varieties of usage.

Now the meaning of an element of culture is to be found in its interrelation with other elements and in the place it occupies in the whole life of the people, *i.e.* not merely in their visible activities, but also in their thought and feeling. The discovery of this with any certainty is obviously only possible for one who is living in actual contact with the people whose

culture is being studied, and as the result of systematic directed investigation. It is true that when we have a somewhat full knowledge of a people and of all aspects of their culture, we can form ideas as to the meaning of their customs and beliefs. Thus I think that it is possible in the case of the Eskimo to be fairly certain that the essential meaning of the Sedna myth lies in its relation to the division of the year into two parts, summer and winter, and the effects this division has on the social life. But even so, the full elaboration of this hypothesis, and still more the actual verification of it, the demonstration that this really is the meaning, could hardly be carried out except by further investigation amongst the natives themselves.

It must not be supposed that the meaning of an element of culture can be discovered by asking the people themselves what it means. People do not think about the meanings of things in their own culture, they take them for granted. Unless we are anthropologists we do not think about the meaning of even such familiar customs amongst ourselves as shaking hands or raising the hat. If by chance the ethnographer comes upon an individual who has thought about the meaning of his people's customs, he is likely to give what is his own individual interpretation which, significant and interesting though it may be, cannot be taken as a valid statement of what the custom really means to the community in general. The meaning of any element of culture can only be defined when the culture is seen as a whole of interrelated parts, and this can only be accomplished by one who is able to take an objective view of it, the ethnographer or descriptive sociologist, in fact.

The field worker, therefore, has to follow a special technique for discovering the meanings of the facts of culture that he observes, a technique analogous in some ways to, but on the whole more difficult than, that used by the lexicographer in recording a spoken language for the first time. This technique is now being slowly developed, but its full development will only be possible as progress is made in sociological theory.

From the point of view of the comparative sociologist much of the work done in the recording of the cultures of non-European peoples in the past is unsatisfactory and cannot be properly utilised. The cases of our ethnographical museums are filled with objects the full meanings of which we do not know and probably can never discover. Our libraries are full of collections of myths obtained from native peoples, and books containing detailed and illustrated accounts of ceremonies, without anything to reveal to us the meanings of those myths or ceremonies. Such material can, of course, be put to some use by the sociologist, but it is of decidedly less use than it is to be hoped that field-work of the modern type will be.

I think that the first movement towards this new kind of field-work was made many years ago by Dr. Haddon when he organised the Cambridge Expedition to Torres Straits. In those days, however, it was thought that the proper person to undertake the systematic interpretation of a culture would be a psychologist. Dr. Haddon took with him three of the foremost psychologists of our times. The experiment had valuable results, but that general interpretation of the Torres Straits culture, that was to have been included in the volume of the Reports dealing with Psychology, will never be written. The psychologist as such is not

qualified to undertake the task of interpreting culture. It is a task that belongs not to psychology but to sociology. Dr. Haddon's attempt came too soon in the history of anthropology.

As France led the way in the development of the theoretical study of comparative sociology, we might have expected that it would be in France that the new methods of field-work would be elaborated. The work of Doutté in Morocco was an early step in that direction, and the later work of Réné Maunier is a good example of the new methods. Marcel Granet's important work on China is based rather on the study of Chinese documents than on observation of the living culture. But the French apparently are not drawn very strongly towards ethnographical research.

At the present time it is only in the work of a small but increasing number of investigators that the new methods are illustrated. I can indicate the work of Prof. Malinowski and of Dr. Margaret Mead. But during the next few years we may expect to see the publication of a good deal of work carried out on these lines.

An objection that is and can be raised against this kind of work is that there is a great deal of room for the personal equation of the investigator to influence the results. That is true and must be recognised, but its importance can easily be exaggerated. A remedy, not perhaps perfect but very valuable, will lie in the development of a technique or methodology of interpretation, whereby the validity of a particular interpretation can perhaps be demonstrated by crucial facts or at any rate tested in such a way as to reduce, if not eliminate, the effects of the personal equation. The elaboration of this technique is one of the problems that face us at the present time, one of the urgent needs of our science. The multiplication of studies of this kind, by bringing a larger number of observers into the field, and by providing us in some instances with observations in one region by two independent workers, and also the occasional co-operation of two or more persons in one investigation, will all help towards the elimination of the effects of the personal equation. But the most important thing of all in this direction will be the development of sociological theory which will afford a guide to the field-worker in his studies and assist him to obtain both objectivity and completeness in his observations.

An adequate sociological understanding or interpretation of any culture can only be attained by relating the characteristics of that culture to known sociological laws. These laws can of course only be discovered by the comparative method, *i.e.* by the study and comparison of many diverse types of culture. The procedure in our science must therefore depend on the building up of a body of theories or hypotheses relating to all aspects of culture or social life and the testing of these hypotheses by intensive field research. The field-worker of the future, or indeed of the present, must be thoroughly cognisant of all the sociological hypotheses that are partly verified, and if possible of those in course of elaboration, and must direct his research to the testing ot these hypotheses, either his own or those of other workers in the science, by their application to a particular culture. Only in this way can the hypotheses be tested and either verified, rejected, or modified; and the normal result will probably be modification rather than complete verification or complete rejection.

Only so can the proper method of the generalising sciences be carried out, namely, the process of making a preliminary study of the known facts, the formulation of hypothetical generalisations, the testing of these hypotheses by a further examination of a specific series of data, the modification of the original hypotheses in the light of the new data, the further testing of the hypotheses in their new and possibly more complex or more definite form, and so on. Only in some such way as this, in default of the possibility of actual experiment, can we build up a science of human society.

I have said that the meaning of any element of a culture is to be found by discovering its relation to other elements and to the culture as a whole. It follows from this that the field-worker must normally, or whenever possible, undertake an integral study of the whole culture. It is not possible, for example, to understand the economic life of a native people without reference to such things as the system of magic and religion, and of course the converse is equally true. The necessity for such unitary intensive studies of selected areas was long ago insisted on by Dr. Haddon and later by Dr. Rivers, and may be said to be part of the tradition of the Cambridge school. The development of the sociological point of view has made the necessity even more evident than before.

It may be noted here that this view of the unitary nature of culture is one of the most important features of the new anthropology, and a point in which it differs markedly from some of the former and present-day anthropology and ethnology. Certain writers on culture adopt what might perhaps be called an atomic view of culture. For them any culture consists of a number of separate discrete elements or 'traits' that have no functional relationship with one another, but have been brought together as a mere collection by a series of historical accidents. A new element of culture has its origin somewhere and then spreads by a process of 'diffusion,' which is frequently conceived in an almost mechanical way. This point of view has arisen largely from the museum study of culture.

The new anthropology regards any persisting culture as an integrated unity or system, in which each element has a definite function in relation to the whole. Occasionally the unity of a culture may be seriously disturbed by the impact of some very different culture, and so may perhaps even be destroyed and replaced. Such disorganised cultures are very common at the present day all over the world, from America or the South Seas to China and India. But the more usual process of interaction of cultures is one whereby a people accepts from its neighbours certain elements of culture while refusing others, the acceptance or refusal being determined by the nature of the culture itself as a system. The elements adopted or 'borrowed' from neighbours are normally worked over and modified in the process of fitting them into the existing culture system.

The scope of field-work amongst non-European peoples is being widened in another direction, partly as a result of the new conception of the theoretical aims of the study, and partly as a result of the relations now being established between anthropology and colonial administration. In former days if a field-worker went to a people who had been subjected to European influence, as was usually the case, his task was to discover

as far as he could; and in detail, what the original culture was, before that influence took effect. It was not considered a part of the ethnographer's work to study in detail the changes produced in the native culture by the contact with Europeans. But a precise knowledge of these changes and how they occur is often of great value for theoretical sociology, and even more for the provision of a scientific basis of exact knowledge for colonial administration. The ethnographer's first task remains the same, that of learning all that it is possible to discover about the culture as it was originally. Only after that has been done with some measure of completeness, is it possible to understand the changes that European influence brings about. But if anthropology is to be of real assistance to colonial administration the field-worker must now undertake to study and interpret the changes which he finds taking place in the culture he is investigating.

Such studies are, however, of little or no value either for sociological theory or for practical purposes when the culture in question is in process of complete disintegration or destruction, as, for instance, amongst the Australian aborigines or some of the tribes of North American Indians.

In the new anthropology, therefore, the work of field research has become much more difficult and of much wider scope. The selection and training of persons for that work is also more difficult. The field-worker should be equipped with a thorough knowledge of all the latest developments of theoretical sociology. At the present time this cannot be obtained from books, but only by personal contact with those who are working in the subject. Then he should have learnt the technique of field-work, both as to observation and interpretation. Further he must have a knowledge of all that has been so far learnt about the culture of the culture region in which he is to work, and if possible some knowledge of the languages also. Finally the success of a field-worker in ethnography often depends on certain qualities of temperament and character. Not everyone can win the confidence of a native people.

It is obvious that the ideal field-worker is not easy to find, and needs some years of training. Yet the rewards of the career are much less even than those of other sciences. One of the great difficulties in this science is that of finding workers and providing the means for them to carry out their work. Research in social anthropology is generally expensive. It cannot be carried out, as so much scientific work can, within the precincts of a university. A most urgent need is the provision for such research by means of research fellowships which would enable the anthropologist who has been trained for field-work to carry out such work over a span of years without having to abandon it in favour of a teaching or other appointment, such as at present is the only way of attaining an assured and continuous income.

Yet the future of the comparative sociology of non-European peoples lies entirely with the field-worker. The day has gone by when we could accept the scientific authority, in this study, of any one who has never himself made an intensive study of at least one culture. In the past we have owed a good deal to those who have been called 'armchair anthropologists.' But in the present situation of the science no insight, however genial, can fully compensate for the absence of direct personal contact with the kind of material that the anthropologist has to study and explain.

This, then, is still another important feature of the new anthropology, the insistence that research and theory must not be separated but most be as closely united as they are in other sciences. The observations of the data, the formulation of hypotheses and the testing of these hypotheses by further direct observation are all parts of one single process which should be carried out as far as possible by the same individual.

Meanwhile there is one fact that seems to me at times to make the position of our science almost tragic. Now that by the gradual development of theory and the improvement of methods of investigation we are in a position to make the most important contributions to the science of man by the intensive and exact study of the less developed cultures of the world, those cultures are being destroyed with appalling rapidity. This process of destruction, through the combined action of European trade or economic exploitation, government by European officials, and missionary activity, is taking place with accelerated pace. During the twenty-five years since I first took up this work myself I have seen great changes. Tribes in Australia and Melanesia and in North America from which we could have obtained most valuable information a quarter of a century ago will now afford us little, or in many instances nothing. In another quarter of a century the position will be ever so much worse. Work that is still possible in all parts of the globe will then be forever impossible. Is there any other science, or has there ever been another science, faced with such a situation, that, just at the time it is reaching maturity, but while through lack of general interest and support it has few workers and very scanty funds, a great mass of most important material is vanishing year by year without the possibility of making any study of more than a minute fraction ?

It will be through field researches that anthropology makes progress towards becoming a real and important science. But intensive studies of single cultures or societies are not sufficient in themselves. Such intensive studies must themselves be inspired and guided by theory, and theoretical sociology must rest on the comparison of different cultures one with another, for comparison in this science has very largely to take the place of experiment in other sciences.

The newer anthropology is developing a different conception of the comparative method from one that has been current in the past. In the older anthropology we were offered books or monographs in which similar, often only superficially similar, customs or beliefs were collected from all sorts of cultures all over the world and thrown together. It was this that was in fact often thought of as constituting the comparative method. Such a procedure may be useful in giving a first survey of some particular problem or group of problems, and has been useful in that way in the past. But it can never do more than indicate problems, it cannot solve them. For that, a more precise and more laborious procedure is necessary.

To understand what precisely the comparative method should be we must bear in mind the kind of problems to the solution of which it is directed. These are of two kinds, which we can distinguish as synchronic

and diachronic respectively. In a synchronic study we are concerned only with a culture as it is at any given moment of its history. The ultimate aim may be said to be to define as precisely as possible the conditions to which any culture must conform if it is to exist at all. We are concerned with the nature of culture and of social life, with the discovery of what is universal beneath the multitudinous differences that our data present. Hence we need to compare as many and as diverse types of culture as we possibly can. In the diachronic study of culture, on the other hand, we are concerned with the ways in which cultures change, and seek to discover the general laws of such processes of change.

It seems to me evident that we cannot successfully embark on the study of how culture changes until we have made at least some progress in determining what culture really is and how it works. Thus the study of synchronic problems must necessarily to some extent precede the study of diachronic problems. The changes that take place in the institutions of a people are not properly comprehensible until we know the functions of those institutions. On the other hand, it is also true that if we can study changes taking place in some aspect of culture it will help us greatly in our functional investigations.

As the problems of comparative sociology are of two kinds, so the comparative method will be used in two ways. In relation to the synchronic study of culture we shall compare one with another different cultures as each exists at a given moment of its history, and without reference to changes in the culture itself.

The loose comparative method, as it was often used, and indeed is still used by some writers, is scientifically unsound in that it makes immediate comparisons between isolated customs or beliefs from different regions and from cultures of very different types. Further, it concentrates attention on similarities of custom, and often on what are only apparent and not real similarities. But for the sociologist the differences are certainly not less important than the resemblances in culture, and the new comparative method concentrates its attention on these differences.

I have already indicated how comparative sociology regards a culture as normally a systematic or integrated unity in which every element has a distinct function. It therefore aims, and must aim, at comparing whole culture systems one with another, rather than comparing isolated elements of culture from diverse regions. The procedure, therefore, has to be analogous to that of the comparative morphologist and physiologist in the comparison of animal species. They carry on their studies by comparing varieties within the same species, or species within the same genus, and then proceeding to the comparison of genera, of families and of orders.

In comparative sociology, as Steinmetz pointed out many years ago, scientific procedure must be based on a systematic classification of cultures or of social types. Our first step, therefore, is to define as well as we can certain culture areas or types of culture. The procedure, of course, is as old as Bastian, but has acquired a new importance and use.

Thus we find that Australia as a whole is a single sufficiently homogeneous area, having the same type of culture throughout. We can therefore immediately proceed to a comparison of the various Australian tribes one with another. Each tribe, or each small group of tribes, can thus be

regarded as offering us in its culture system a special variety of a general type. By studying these variations as minutely as possible we can carry out a process of generalisation which enables us to give a general definition or description of the type itself. By this process we are often able to discover correlations between one element of culture and another. Further, this procedure is almost essential in any attempt to discover the meaning and the function of any element. For by it we are able to determine, in any institution or custom or belief, what remains constant and what varies as between one part of a culture area and another.

This study of culture types and varieties in comparative sociology is quite different from the study of culture-areas in ethnology. The latter aims, above all, at providing material for the hypothetical reconstruction of movements of culture diffusion. The former is essentially a process of generalisation, a means of discovering general features or principles which remain constant throughout the type while taking different forms in different parts of the area.

In this study of variations of a single culture type we should aim at comparing the whole culture of one tribe with that of another. But that is often impossible ; in fact, in the present state of our knowledge, almost always. We may proceed, therefore, by making a comparative study of variations in some particular aspect of the culture. But we must be careful how we isolate one part of the culture from another for the purposes of study. Thus, a good deal of misunderstanding has resulted from dealing with some particular aspect of the social organisation of Australian tribes, instead of dealing with that organisation as a whole.

There is perhaps no other region which is quite the same as Australia in the opportunities it offers for the study of many variations of a single culture type. In other regions, therefore, our procedure must be somewhat different. Thus, if we wish to deal with the Bantu cultures of Africa we must begin by dividing the whole region into suitable units. One such unit would be composed of the Basuto-Bechuana tribes, while the Zulu-Kaffir tribes would provide us with another. Our first step will consist of a careful study of the variations within the unit region. We then compare the one region with the other, and may proceed in this way to explore the whole Bantu area in such a way as to be able to give a sound description of the general characters of Bantu culture as a whole. Only when we have carried studies of this kind a certain distance does it become really profitable to make comparisons between Bantu culture and Polynesian or North American.

Thus, for the new anthropology the comparative method is a method of obtaining generalisations. Amongst the variations of institution and custom in one region we seek to discover what is general to the whole region or type. By comparing a sufficient number of diverse types we discover uniformities that are still more general, and thus may reach to the discovery of principles or laws that are universal in human society.

A word, the constant use of which has been a great obstacle to scientific thinking in anthropology, is the word ‘ primitive.’ It conveys the suggestion that any society to which we apply it represents for us something of the very beginnings of social life. Yet if culture had, as we may well assume, a single origin some hundreds of thousands of years ago, then any

existing culture has just as long a history as any other. And although the rate of change may vary, every culture, just as every language, is constantly undergoing change. But, quite apart from this implication of the word as meaning in some sense ‘ early,’ harm is done by the current application of it to the most diverse types of culture. The difference of culture between the Maori of New Zealand and the aborigines of Australia is at least as great as that between ourselves and the Maori. Yet we group these two cultures together as ‘ primitive,’ and contrast them with our own as ‘ not primitive.’ I am well aware how difficult it is to avoid completely the use of the term, or some equally unsuitable one, such as ‘ savage.’ Perhaps if we keep sufficiently in mind the great cultural differences between the various peoples whom we thus lump together we shall avoid the chief disadvantage attaching to its use. We shall then be able to avoid the fault of the loose comparative method, of regarding as immediately comparable with one another all those very different types of society that are labelled primitive.

This abstract discussion of method, I fear, will hardly convey to you any very definite conception. Will you permit me, therefore, to select a particular example of a synchronic problem and indicate briefly the lines along which I would attempt to solve it ? We may take for our example one of the fundamental problems of sociology, that of the nature and function of the moral obligations which a society imposes on its members. For the purposes of scientific investigation this general problem must be subdivided into a large number of subsidiary problems. Thus we can isolate, as one such, the problem of the nature and function of the rules prohibiting marriage between persons who stand in certain social relationships ; in other words, the nature and function of the prohibition of incest. These prohibitions were, of course, dealt with by the older social anthropology, and we have had a number of theories of the ‘ origin ’ of the prohibition of incest. Even Durkheim faced this problem in the old way. Now, quite apart from the fact that any hypotheses as to how prohibitions of this kind first came into existence many hundreds of thousands of years ago are entirely incapable of verification, it is also evident that even a plausible hypothesis of origin can give us no explanation of the great diversity that we find in the prohibitions current in different existing social types. Yet it is the explanation of these differences that is really the crux of the problem. In this, as in so many other sociological inquiries, we have to seek an explanation *per genus et differentiam.* We wish to know why every society has rules of this kind and why the particular rules vary as they do from one social type to another. As soon as we state the problem in this way, we have a comparative problem of the kind I have been referring to. In dealing with such a problem I would first select a culture in which the rules prohibiting marriage are definite and highly elaborated. The culture of the Australian tribes is obviously in this respect a very suitable one. Further, we must have a culture in which there are sufficient variations between one tribe and another, while the general type remains the same. Here again Australia is a very suitable region. I would therefore begin the investigation by a comparative study of Australian tribes. Note that this is not at all because Australian

culture is 'primitive' in the sense that it represents the early beginnings of human society. On the contrary, Australian culture is a highly specialised one, in which there has been an extreme elaboration of the kinship organisation, and it is exactly for this reason that I would select it for the study of any problems relating to kinship. Australia represents not the beginning but the end of a long line of development of kinship structure. Thus, my reasons for selecting Australia are the exact opposite of those put forward by earlier writers who have made the same selection.

Having selected a first field for comparative study I would compare the social organisation, as a whole, of all the Australian tribes about which we have adequate information, in order to define what is the nature of the correlation between the rules prohibiting marriage and the social structure. In other words I should be seeking to define as precisely as possible the function of such rules as part of the total system of social integration. The investigation has to rest on the detailed examination of variations. As the result of such a study of Australia we can reach a number of significant generalisations. We shall, for example, reach certain provisional conclusions as to the nature (not the origin) of exogamy. These conclusions must now be tested by a similar study of other types of culture. It would be impossible for one student even in a lifetime to make a thorough investigation of all known cultures in this way. That is why the co-operation of a number of students in the study of any single problem is so essential in sociology. But a close study of one other type of culture sufficiently different from the Australian would permit of a very valuable verification of the provisional results obtained.

When a theory as to the nature and function of the prohibition of incest has thus been reached, the next step will be to seek for the *experimentum crucis* by which it can be more critically tested. Such a crucial instance will often be one which appears to conflict directly with the theory. Thus, on my own theory we ought to find marriage everywhere prohibited between parent and child and between brother and sister. The various societies in which the marriage of brother and sister is permitted, therefore, offer us an opportunity of testing the theory, for we must be able to explain these exceptions on the basis of the theory itself. The exception must prove the rule. Other similar crucial instances can be sought by which to test the validity of the general theory.

As a result of such an investigation we should, if we are at all successful, reach certain conclusions as to the relation between moral obligations and social structure. In other words, we should have learnt something about the place of such obligations in social integration. Sociology would then have to undertake similar investigations on other problems within the general problem. We might study in the same way the obligations relating to the taking of human life, or those relating to the rights of property. As the final result of such a series of related studies we could arrive at a theory of the nature and function of morality in general. Incidentally, of course, any single investigation of this kind must be linked with and throw light on a great number of other sociological problems. Thus, the study of the prohibition of incest necessarily involves a close study of kinship from other aspects also.

I hope that the example I have given will have made it clear that the comparative method as used for the synchronic study of culture is something different in important respects from the older comparative method used as a means of arriving at theories of the origin of institutions.

When we turn to the diachronic problems with which comparative sociology has to deal, *i.e.* with the problems of how cultures change, the comparison of cultures as each of them is at a given moment of history, while it may give us a certain amount of help, is not sufficient by itself. Thus, the study of the variations that have been produced in a single culture, as, for example, in Australia, although we have no observations as to how or when they occurred, can nevertheless give us our preliminary orientation in the study of how variations do occur. In other words, the comparative study of cultures without history is a method of enabling us to formulate with some precision the problems with which we shall have to concern ourselves in a diachronic study of culture.

Ultimately, however, if we are to discover the laws of social change we must study the actual processes of change. This we can do to some extent by means of historical records, wherever we have records that are sufficiently reliable and complete. But it is desirable that as soon as possible the sociologists themselves should undertake to study the changes that take place in a culture over a period of years. The comparative method in this instance will consist in the careful comparison of accurately observed processes of change.

In the present organisation of anthropology the social anthropologist is supposed to confine himself to the study of the peoples without history, the so-called primitive or savage peoples who still survive outside Europe. If he considers Europe at all he is supposed to concern himself only with prehistoric times and with what is called folk-lore, *i.e.* certain aspects of culture which have been regarded as survivals from earlier, more primitive, cultures. This division of the peoples of the world into two groups for the purpose of study was apparently satisfactory enough, as long as anthropology was dominated by the historical method. The historian could give us the real history of European languages and cultures throughout historic times. It was left to the anthropologist, as ethnologist or archæologist, to concern himself with the reconstruction of the past in those regions and periods that lay outside the field of history proper.

But for comparative sociology as the generalising science of culture. this division of the historic and the non-historic cultures is entirely unsuitable, and indeed detrimental. The sociologist must study all cultures and by the same methods. In dealing with historical cultures he is not competing or conflicting with the historian, for the two follow quite different aims and methods. The historian does not or should not seek generalisations. He is concerned with particulars and their particular and generally chronological relations.

I am sorry that I have not time in this address to deal properly with the relation of the study I have described as comparative sociology, and the studies pursued sometimes under the name of sociology or social science. I can do no more than offer a few brief remarks. First let me

say that what is called sociology in France, or at any rate at the University of Paris, is the same study precisely as that which I have been describing as comparative sociology, and it is largely owing to the work of the French sociologists Durkheim, Hubert, Mauss, Simiand, Halbwachs, Hertz, Granet and Maunier, to mention only some of them, that the subject is as far advanced as it is.

In Germany a great deal of what is called sociology is really better described, I think, as social philosophy or philosophy of history. One writer who represents the comparative sociology that I have described is Richard Thurnwald.

In England we have very little of anything that is called sociology. Hobhouse, who stood for sociology in this country, was a philosopher rather than a scientist.

In the United States there are a great number of departments of sociology scattered through the universities. It is difficult to summarise the various kinds of study that are included under the term. A considerable part of the work in many departments of sociology consists of what would be called civics in this country and in studies connected with social welfare work. There is still a little of what should properly be called social philosophy, though much less than there was a quarter of a century ago. The most marked activity of these departments at the present time is what can be described as factual social studies, *i.e.* the collection of precise information, in statistical form wherever possible, about certain aspects of social life, principally in the United States itself, but also to some extent in other countries.

I think I have made it clear that my own view is that any attempt to discover the general laws of human society must be based on the thorough detailed study and comparison of widely different types of culture. It was, indeed, the very firm conviction that this was so that led me to enter the field of anthropology a quarter of a century ago. I am, if anything, more convinced than ever of this, and see no hope for the development of any really scientific sociology except on this comparative basis.

Unfortunately, what has happened has been that anthropology has largely neglected the sociological study of non-European peoples in favour of conjectural history, and at the same time most of those engaged in one form or another of sociological study have had little thorough knowledge of non-European societies. What I have described as comparative sociology has, except in France, been left by the anthropologists to sociology, and by the sociologists to anthropology. I believe that the unsatisfactory results of this division of studies, whereby comparative sociology has failed to find any proper place, is now coming to be recognised in America, partly as the result of the work of the Social Science Research Council in attempting to co-ordinate the various social studies, and I live in hope that before another quarter of a century is out the science of comparative sociology will have obtained a recognised and very important place in any well-organised school of social sciences.

English universities, or I may say British universities in general, have been very chary of admitting sociology in any form as a subject of study in strong contrast with the popularity of the subject in the United States. To some extent that caution has been a wise one. The subject is still in

its formative stages. But, on the other hand, its absence from the list of recognised university studies has stood very much in the way of its development.

You will see that in this address I have been chiefly concerned with trying to indicate a new alignment of the studies which are grouped together under the name Anthropology. This new alignment is itself a natural growth, but should be recognised, and must ultimately be made the basis of any satisfactory co-ordination of studies in universities and elsewhere.

First, there are the three studies that have been traditionally associated under the name anthropology—Physical Anthropology, Prehistoric Archæology and Ethnology.

Physical Anthropology seems due to be absorbed in a wider study of Human Biology, which requires to be carried on in close association with the biological sciences. The present procedure by which Physical Anthropology is taught as part of Anatomy is not always quite satisfactory. It is liable to neglect the physiological study of man as a living organism. and to deal very perfunctorily with the important problems of human genetics. I should like to see Human Biology given recognition as an independent and very important subject. We have, of course, the Galton Laboratories as one centre for such studies in England. The widespread interest—not always, I fear, entirely scientific—in Eugenics and in race problems could be utilised to obtain sufficient support. On the other hand, there seems no particular advantage to Human Biology in being linked to Archæology and Ethnology.

Prehistoric Archæology is now an independent subject with its own special technique and carried on by specialists. The archæologist, of course, requires to have a knowledge of Human Palæontology, but equally he needs a knowledge of general palæontology and geology. The natural affinity of Archæology, however, is with History.

Ethnology, in so far as it attempts not merely to classify races, languages and cultures, but to reconstruct their history, must necessarily maintain a very close connection with archæology. It may, indeed, very well be treated as in a sense a branch or further development of archæology, as that is of history. Thus, Prehistoric Archæology (or Palæ-ethnology as it is occasionally called) and Ethnology may well be regarded as one subject pursuing the aims and methods of historical science.

Over against the historical sciences there stand the three generalising sciences of Human Biology, Psychology and Comparative Sociology.

The closest and most important relation for Comparative Sociology is with Psychology. There is no particular advantage to the comparative sociologist in acquiring more than an elementary knowledge of Prehistoric Archæology. A study of history, so far as it deals with culture rather than with the doings of kings, statesmen and soldiers, is of much greater value to him. Particularly at the present time it is desirable that the comparative sociologist should avoid becoming entangled in the conjectural reconstructions of history which I have described above as belonging to Ethnology.

As I see it, therefore, the subject of anthropology is dividing itself

into three subjects, distinguished either by differences of method or of subject-matter; Human Biology, which is, or should be, allied with the biological sciences; Prehistoric Archæology and Ethnology, which belong with historical studies; and Comparative Sociology, the relations of which are with psychology on the one side and on the other with history and with the social sciences, economics, jurisprudence, &c.

I have said nothing yet on the study of languages. We have witnessed in recent decades the development of a general science of Linguistics which has been winning for itself an independent place. It is, I think, highly desirable that a close connection should be maintained between Linguistics and Comparative Sociology. I have no time on this occasion to discuss in detail the relations of the two subjects.

In concluding this address I wish to return to a matter that was briefly mentioned at the beginning, namely, the very important recent development of what we may call Applied Anthropology or Administrative Anthropology. During more than a decade my own work has been very largely concerned with this study in Africa and in Oceania. If I seem to you to speak dogmatically in what I have to say, I would ask you to remember that in the time at my disposal I can only put before you certain of my conclusions without explaining the considerations on which they are based.

For a very long time the anthropologists have been declaring the necessity of utilising their science in the practical work of governing and educating dependent peoples. So far as the British Empire is concerned this has at last led to certain practical steps being taken. There have been appointments of Government anthropologists in two of the African colonies, and in Papua and the Mandated Territory of New Guinea. Cadets and officers of the services of the African colonies are now given brief courses of instruction in anthropology at Oxford and Cambridge. In South Africa the School of African Life and Languages of the University of Cape Town started some years ago a vacation course on anthropology and native administration and education for government officers and missionaries, and I believe that these courses have been continued. In Sydney a more extensive experiment has been carried on since 1927. Cadets who are selected for the administration of the Mandated Territory are sent to the territory for one or two years to make acquaintance with the kind of life and work they will have, to test their suitability for it and to enable them to judge if they do finally wish to take up the career. They then attend the University of Sydney for one academic year of nine months and devote their whole time there to a special course of training. This includes two short courses in Topographical Surveying and in Tropical Hygiene, but the greater part of their time is devoted to the study of Comparative Sociology and Colonial Administration. The result of this arrangement will be that in a certain number of years all the administrative officers of the territory will have a sound knowledge of the principles and methods of Comparative Sociology, and by its means will have acquired a considerable knowledge of New Guinea institutions and customs and their meaning, and will have made a systematic study of administrative problems and methods. The cadet system has not been accepted by the

territory of Papua, but a number of the senior officers of the administration have devoted their vacations to attending special courses at Sydney.

Thus, some progress has already been made in turning anthropological studies to practical use. There is still a great deal more that might be done and that ought to be done. Some of the British colonies, such as the Western Pacific and British Malaya, have neither government anthropologists nor any regular training in anthropology for their officers. Moreover, it seems to me that the courses now taken by officers in the African services are inadequate. A few weeks given to anthropology may be better than nothing, but certainly cannot be called sufficient. There is no doubt that one of the most efficient native administrations is that of the Dutch East Indies, and the qualification for this requires five years of special studies, including native languages and native law and custom.

A question of some importance is, what kind of anthropological teaching should be given to native administrators to fit them better for carrying on their work. There is, I think, no value to them in a study of physical anthropology or the classification of races that falls under physical anthropology or ethnology. There is equally no value for them in any study of prehistoric archæology. Further, those attempts to reconstruct the history of cultures and peoples that I have been calling ethnology are of absolutely no practical value in the work of native administration or education.

There is obvious practical value in training which will help the colonial officer to speak the language or languages of the peoples he is dealing with. This is already provided for in some of our colonies.

What the administrator and educator amongst dependent peoples need above all is a detailed knowledge of the social organisation, the customs and beliefs of the natives and an understanding of their meanings and their functions. This can be attained only by means of a general study of comparative sociology, followed by an intensive study of the particular people in question.

I have on many occasions met with persons who were engaged in the government or education of native peoples who have expressed the view that, whatever academic interest anthropology might have, it has no practical value in work such as they are engaged in. I have found that what was thought of as anthropology by these persons was the series of academic studies that includes physical anthropology, the classification of races, the ethnological reconstruction of history, prehistoric archæology and the social anthropology that elaborates theories of the origins of institutions. One magistrate complained to me that, though he had read the whole of the *Golden Bough*, he did not find that it gave him any practical help in dealing in his court with the customs of a native tribe. Another, who had interested himself in the writings of Elliot Smith and Perry, was firmly convinced that a study of anthropology could be of no practical use to him in spite of its interest. An officer of one of the African colonies who was specially sent to give advice on methods of colonial administration to one of the British Dominions, was asked if it would be a good thing to give a training in anthropology to those who would ultimately become district officers. He replied that it would be useless or even harmful; that a magistrate so trained would be thinking

about the shape of a witness's head instead of attending to the evidence he was giving in court. These are typical examples of the sort of thing I have met with over and over again. For the man in the street anthropology is the study of skulls or stone implements or of the ethnological specimens that we collect in our museums, or else theories about the travels of ancient Egyptians round the world in search of pearls. And indeed, if he judges by the subject as treated in universities, or by the contents of anthropological periodicals, or the proceedings of anthropological congresses, these things do constitute the major part of what is known under the name.

I do not wish for a moment to suggest that these studies are not of academic and scientific value. I am only saying that they are of no value in the practical business of governing and educating dependent peoples. On the other hand, I have been experimenting for ten years with a course of study which consists of a general course covering the whole field of comparative sociology, followed by a functional sociological study of the culture with which the students were to be concerned (Bantu Africa in one instance, New Guinea and Melanesia in another), supplemented by a comparative study of methods and policies of colonial administration and native education considered in the light of the results of comparative sociology. I have found good evidence that such a course of study pursued over not less than one year is really adapted to the needs of the students, and does do what it is claimed anthropology should do, namely, provide a scientific basis for the control and education of native peoples.

In this Empire of ours, in which we have assumed control over so many diverse native peoples in Africa, Asia, Oceania and America, it seems to me that two things are urgently needed if we are to carry out as we should the duties we have thus taken upon ourselves. We have exterminated some of these native peoples and have done, and are doing, irretrievable damage to others. Our injustices, which are many, have been largely the effect of ignorance. One thing, therefore, that is urgently needed is some provision for the systematic study of the native peoples of the Empire. I have pointed out how rapidly material that is of inestimable value for the scientific study of mankind is disappearing through the destruction or modification of backward cultures. From the practical point of view of colonial administration a thorough systematic knowledge of native cultures is required before administration and education can be placed on a sound basis. Research of this kind has been all too long neglected. It can, of course, only be carried out effectively by trained experts. But even if we can find enthusiastic students to take up the difficult and unremunerative work, there is no such provision for research as there is in other sciences. A little, really a very little, considering the magnitude of the work, has been done from our universities, but I am afraid that most of our British universities will not be likely to take any real active interest in the subject until it will be too late to do the work that is now waiting to be done. The International Institute of African Languages and Cultures is preparing to undertake a five-years' program of research in Africa, which I hope will be continued and extended. But for such work we still have to rely on occasional contribu-

tions of funds, most of which come from the United States. I feel sometimes ashamed that the great British Empire has to go begging to America for the few hundreds of pounds with which to carry out a little of that work which it is the primary duty of the Empire to undertake if it is ever to rule its dependent peoples with justice based on knowledge and understanding.

I find it difficult to understand how it is that the study of native peoples of simpler culture receives so little support. There seems to be little difficulty in raising very considerable sums of money every year for archæological investigations. Yet there is no such urgency about these as there is for the immediate study of the living cultures that are being destroyed by the encroachment of the white man. However interesting these dead cultures may be, we study only their dead remains. We can learn very little about their thoughts and feelings, their laws, customs, religion, or mythology, such as we still can learn about the natives of Africa or New Guinea. At a time, not so long ago, when it would have been possible to observe a people such as the Australian aborigines or the Bushmen making and using stone implements of palæolithic type, prehistorians were spending their time speculating as to how the very similar Mousterian and Aurignacian implements might have been used.

A second urgent need at the present time seems to me to be the making of further provision for the application of anthropological knowledge to the problems of the government and education of native peoples. I do not think that anyone would maintain that the provision at present made is anything like adequate.

There has been lately some talk of an Institute of Colonial Studies which would be at the same time a centre for research and for making the results of that research available for those engaged in administrative work. I can only express the hope that before many years it will be possible to bring some plan of that kind to completion.

Meanwhile, in spite of repeated setbacks and disappointments, anthropology has at last succeeded in winning for itself some place in the world of practical affairs, some measure of recognition as a study that can make most valuable contributions to problems that are going to be amongst the most important with which this century is faced, those that have arisen from the mingling of diverse peoples and cultures all over the world. The task of the twentieth and succeeding centuries is that of uniting all the peoples of the world in some sort of ordered community. Attention has quite naturally been concentrated on the relations of the great nations. But the problems of finding the proper place in a world community for the tribes of Africa, Asia and Oceania are possibly not less vital to the successful completion of the task.

SECTION I.—PHYSIOLOGY.

ADDRESS TO THE PHYSIOLOGIGAL SECTION,

Introducing a Discussion on the Biological Nature of the Viruses.

BY

H. H. DALE, C.B.E., M.D., D.Sc., F.R.C.P., Sec.R.S.,

PRESIDENT OF THE SECTION.

I have counted it a very high honour to be called to preside over this Section at the Centenary meeting of the British Association. In the earlier history of the Association's meetings the Section, and its subject, seem to have had a precarious independence. At the original meeting in 1831, sub-committees were formed to deal with particular branches of knowledge, but physiology was not among them. In 1832 the committee dealing with zoology and botany took physiology and anatomy under its charge. Anatomy and physiology attained the dignity of a separate section in 1834, and this took the title and scope of a section of medical science from 1836 to 1844. It kept its separate existence, as a section of Physiology, for three years more, was then re-absorbed for no less than forty-six years into the section of Biology, emerging again into independence with its present title in 1893, since which date it has changed in form only by budding off a daughter-section of Psychology.

Addressing a section with a history of such varying affiliations and fluctuating boundaries, I need hardly apologise for taking a wide view of its scope and its interests. When the British Association first recognised its existence, in the year before Johannes Müller began his tenure of the Chair of Physiology in Berlin, Physiology had become an almost stationary science. It had been dependent for its progress for some centuries before that date on the occasional and rare emergence of a William Harvey, a John Mayow, a Stephen Hales, or a Lavoisier. Since that date, Physiology, the science of the process of life, has been reborn, as a body of knowledge progressing continuously by experiment. In such a progress it was inevitable that new fields of investigation should be opened, each producing its own methods and its own organisation of special investigators. In 1831, and for many years afterwards, nobody could have foreseen the sudden rise of a new science of Bacteriology, or the later development of Biochemistry as a separate discipline. Either of these could probably to-day lay claim to a more numerous body of investigators, and a greater output of new observation, than some departments of scientific activity which have obtained separate sectional representation. Yet I think it is a matter for congratulation, rather than regret, that, in the meetings of the British Association, the section of Physiology should still remain

the centre for report and discussion of all investigations bearing on the nature of the life process in the individual, in health or in reaction to disease. The present tendency, indeed, is for the creation of new links rather than new cleavages. We may observe with satisfaction that among our colleagues in the sections of botany and zoology the detailed study of the vital processes of plants and animals has gradually been displacing that of form, habit and distribution from the predominance which it held in earlier years. Not only the phenomena of the normal life, but those also of the diseases of plants, have claimed the attention of our botanical colleagues. Functional Biology presents problems sufficient in number and interest to occupy the attention of several sections, and it is all to the good that different aspects of the same group of phenomena should engage the attention of investigators belonging to more than one. Particularly welcome is this combined approach to a subject such as that chosen for to-day's discussion, to which my own remarks can only serve as introduction. We are to deal with a group of agents, the existence of which would certainly be unknown to us, but for the changes produced by their presence in the bodies of higher animals and plants. They seem to have one property at least of living organisms, in being capable, under appropriate conditions, of indefinite reproduction. We know nothing of their intrinsic metabolism : it has even been asserted that they have none. Few of them have yet been rendered visible by the microscope ; it is, indeed, a question for our discussion whether any of them have yet been seen or photographed. It is a question again for discussion, whether any of them, or all of them, consist of organised living units, cells of a size near to or beyond the lowest limits of microscopic visibility ; or whether, as some hold, they are unorganised toxic or infective principles, which we can regard as living in a sense analogous to that in which we speak of a living enzyme, with the important addition that they can multiply themselves indefinitely. Some, however, would attribute this, not to actual self-multiplication, but to a coercion of the infected cells to reproduce the very agent of their own infection. Since nothing is known of their structure or their metabolism, these so-called viruses cannot yet be claimed as belonging either to the kingdom of animals or to that of plants. Our colleagues the botanists claim an interest in them, indeed ; but only because agents having the characters attributed to viruses cause a large number of diseases in the higher plants, acquiring thereby an added scientific interest and serious economic importance. So far as I am aware, there is no similar claim for the study of the viruses infecting animals and man to be regarded as belonging to Zoology.

In this section of Physiology, apart from our wide responsibility for bringing the British Association into contact with new and fundamental investigation in medical and veterinary science, the problems presented by the nature and behaviour of the viruses cannot fail to raise questions of the greatest interest to anyone concerned with general physiological conceptions. What is the minimum degree of organisation which we can reasonably attribute to a living organism ? And what is the smallest space within which we can properly suppose such a minimum of organisation to be contained ? Are organisation, differentiation, separation from

the surrounding medium by a boundary membrane of special properties, necessary for the endowment of matter with any form of life? Or is it possible to conceive of a material complex, retaining in endless propagation its physiological character, as revealed by the closely specific reaction to it of the cells which it infects, though it is not organised into units, but uniformly dispersed in a watery medium? Among those who study the viruses primarily as pathogenic agents, these questions provide matter for debate, the warmth of which may even penetrate our discussion of to-day. I suggest that they are questions with which the physiologist may properly be concerned. To-day, we may do little more than display the nature of the biological problem; but I am not without hope that its discussion, by those who study diseases in animals and plants, may enlist the interest of physiologists in its solution.

We cannot afford to-day much time for the history of the subject; but it is of interest to note that Edward Jenner was dealing, in small-pox and vaccinia, with what we now recognise as characteristic virus infections, long before there was any hint of the connection of visible bacteria with disease. Pasteur himself was dealing with another typical case of a virus infection in the case of rabies. The clear recognition, however, of the existence of agents of infection, imperceptible with the highest powers of ordinary microscopic vision, and passing through filters fine enough to retain all visible bacteria, begins with Ivanovski's work in 1892 on the mosaic disease of the tobacco plant, brought to general notice and greatly developed by Beijerinck's work on the same infection some seven years later; and with Loffler and Frosch's demonstration, in the same period, that the infection of foot-and-mouth disease is similarly due to something microscopically invisible, and passing easily through ordinary bacteria-proof filters. Since those pioneer observations the study of viruses has spread, until they are recognised as the causative agents of diseases in an imposing and still growing list, containing many of the more serious infections of man, animals, and plants.

If we are to discuss the biological nature of the viruses, it is obvious that we should begin by attempting some kind of definition. What do we mean by a virus, and what are the tests by which we decide that a particular agent of infection shall be admitted to, or excluded from, the group? But a few years ago, I think that we should have had no difficulty in accepting three cardinal properties as characterising a virus, namely, invisibility by ordinary microscopic methods, failure to be retained by a filter fine enough to prevent the passage of all visible bacteria, and failure to propagate itself except in the presence of, and perhaps in the interior of, the cells which it infects. It will be noted that all three are negative characters, and that two of them are probably quantitative rather than qualitative. Such a definition is not likely to effect a sharp or a stable demarcation. We shall see that its failure to do so is progressive. Nevertheless, it would still be difficult to refuse the name of virus to an agent which fulfils all three criteria; and we must, therefore, in consistency, apply it, on the one hand, to the filtrable agents transmitting certain tumours, and, on the other hand, to the agents of transmissible lysis affecting bacteria, and now widely known and studied as bacteriophages.

But the strict application of such a definition, based on negative characteristics, must obviously narrow its scope with the advance of technique. We may look a little more closely at the meaning of these different characters, concerning one or another of which those who follow me will, doubtless, have more to say in detail.

Microscopic visibility is obviously a loose term. Rayleigh's familiar formula, in which the lower limit of resolution is equal to one-half the wave-length of the light employed, divided by the numerical aperture of the objective, only gives us the smallest dimensions of an object, of which, with the method of transmitted illumination habitually used in former years, a critical image can be formed. There can be no doubt that the separate particles, of practically all the agents to which the term virus would be applied, fall below this limit of size. To put it in plain figures, their diameter is less than 0·2 micron. On the other hand, progress has recently been, and continues to be, rapid in the direction of bringing into the visible range, minute bodies associated with a growing number of viruses. This has been effected, on the one hand, by improvement in staining technique, which probably owes its success largely to increase of the natural size of the particles by a deposit of dye on their surface; and, on the other hand, by forming visible diffraction images of the unstained particles with wide-aperture dark-ground condensers, and by photographing the images formed of them with shorter, invisible rays. Mr. Barnard will present evidence for success in obtaining such sharp photographic images of the bodies associated with one virus, measurements of which give their natural size by simple calculation. The reaction of a cautious criticism to such a demonstration seems to have taken two different directions. There has been a tendency, on the one hand, to exclude an agent from the group of viruses as soon as the microscope could demonstrate it with some certainty. Many have for years thus excluded the agent transmitting the pleuropneumonia of cattle, though the status of this organism has been compromised even more by the success of its cultivation on artificial media. Visibility seems to have rendered doubtful the position of the Rickettsia group of infections, and, if the test is logically applied, the process of exclusion can hardly stop before the agents transmitting psittacosis, fowl-pox, infectious ectromelia, and even vaccinia and variola, have been removed from the group of viruses into that of visible organisms. In discussing the biological nature of viruses as a whole, however, we can hardly begin by accepting an artificial and shifting limitation of that kind. The real task before us, rather, is to discuss to what extent the evidence of these recent developments, which appear to show that some of the agents, known hitherto as viruses, consist of very minute organisms, can safely be applied to other viruses, which are still beyond the range of resolution. Do these also consist of organisms still more minute, or are any of them unorganised? Another line of criticism, sound in itself, while not excluding from the virus group these agents for which microscopic visibility has been claimed, demands more evidence that the minute bodies seen or photographed are really the infective agent, and not merely products of a perverted metabolism which its presence engenders. It is obvious that complete

evidence of identity cannot be obtained until a virus has been artificially cultivated in an optically homogeneous medium. Meanwhile it is a question of the strength of a presumption, on which opinions may legitimately differ. Let us recognise that the evidence is not perfect, but beware of a merely sterilising scepticism. I suspect that the attitude of some critics is coloured by past history of the search for viruses, and especially by that part of it concerned with the curious objects known as "inclusion bodies," which are readily demonstrated with relatively low powers of the microscope, in the cells of animals and plants infected with certain viruses. The nature of these bodies will probably form the subject of part of our discussion. From the earlier and admittedly hasty tendency to identify them as infective protozoa, opinion seems to have swung too quickly to the opposite extreme, of dismissing them as mere products of the infected cell. It is so comparatively simple, in some cases, to separate these bodies, that it is surprising that so few efforts have been made to test their infectivity. However, the power of such a body to convey at least one virus infection has been demonstrated ; and since they have further been shown, in several cases, to consist of a structureless matrix packed with bodies looking like minute organisms, the burden of proof in other cases seems to me, for the moment, to rest on those who suggest that they consist wholly of material precipitated by the altered metabolism due to the infection. We shall probably hear evidence for both points of view.

The physical evidence, obtained by filtration through porous fabrics and colloidal membranes, and by measuring rates of diffusion, is, of course, purely concerned with the size of the units of infective material, and must be taken in conjunction with the evidence provided by the microscope. The crude, qualitative distinction between the filterable and non-filterable agents of infection has long since ceased to have any real meaning. There is no natural limit of filterability. A filter can be made to stop or to pass particles of any required size. It is now realised that the only proper use of a filter in this connection is to give a quantitative measure of the maximum size of the particles which pass it. Evidence from failure to pass must always be subject to correction for the effects of electrostatic attraction and fixation by adsorption on the fabric of the filter. A large amount of filtration evidence has, further, been vitiated by reliance on determinations of the *average* pore size of the filter. In dealing with an infective agent, the test for the presence of which depends on its propagation under suitable conditions, it is obviously the maximal pore size which is chiefly significant. For these reasons a good deal of the evidence showing that certain viruses can be detected in the filtrates, obtained with filters which will not allow hæmoglobin to pass in perceptible quantities must be regarded at least with suspicion. Dr. Elford will tell us of his recent success in preparing filter-membranes of much greater uniformity, with a small range of pore-diameters. His measurements with these of the sizes of the particles of different viruses, show a range approaching the dimensions of the smallest recognised bacteria, on the one hand, and falling as low, in the case of the virus of foot-and-mouth disease, as about three or four times the size of the hæmoglobin molecule ; the latter being

given not only by filtration-data, but by other physico-chemical measurements, such as those obtained by Svedberg with the ultracentrifuge. It should be noted, as illustrating the difficulties of the problem and the uncertain meaning of some of the data, that Elford has regularly found a bacteriophage to be stopped by a membrane which allows the foot-and-mouth virus to pass; while, on the other hand, recent determinations of the rate of diffusion of bacteriophage, made by Bronfenbrenner, put the diameter of its particles at 0·6 of a millimicron, i.e. only about one-fifth of the accepted dimensions of the hæmoglobin molecule. If we accepted such an estimate, we should be obliged to conclude, I think, not merely that the bacteriophage is unorganised, but that its molecules are something much simpler than those of a high-molecular protein. It has even been suggested, though on very imperfect evidence, that it may be a moderately complex carbohydrate. Are we, then, to suppose that the foot-and-mouth virus is a similarly unorganised and relatively simple substance? It is difficult, in view of the series of other agents, all conforming in many aspects of their behaviour to the classical type of the foot-and-mouth virus, and yet showing a range of dimensions up to that at which their units are apparently becoming clearly visible by modern microscopical methods. It will be clear, indeed, that, if we accept the lowest estimates for the size of the units of some viruses, such as the bacteriophage and the agents transmitting some plant diseases, we cannot by analogy apply the conception of their nature, thus presented, to viruses consisting of organisms which are ceasing to be even ultramicroscopic; and we should be led to doubt the identity with the virus of the bodies which the microscope reveals. If, on the other hand, we regard the still invisible viruses, by analogy with those already seen, as consisting of even much smaller organisms, we can only do so by rejecting the conclusions drawn from some of the physical evidence. It is, of course possible that some of the agents called viruses are organisms and others relatively simple pathogenic principles in solution; but to assume at this stage such a fundamental difference, among members of a group having so many properties in common, would be to shirk the difficulty.

The third negative characteristic of a virus, viz., its failure to propagate itself, except in the presence of living cells which it infects, may obviously again provide an unstable boundary, shifting with the advance of our knowledge and skill. We may regard it as not only possible, but even likely, that methods will be found for cultivating artificially, on lifeless media, some of those viruses, at least, which have the appearance of minute organisms. Evidence in support of one claim to such success will probably be put before us. It would be playing with nomenclature to let inclusion in the virus group depend on continued failure in this direction. On the other hand, the dimensions assigned to the units of some viruses, representing them as equal in size to mere fractions of a protein molecule, might well make one hesitate to credit them with the power of active self-multiplication. Experience provides no analogy for the growth of such a substance by self-synthesis from the constituents of a lifeless medium; the energetics of such a process might present an awkward problem. To account for the multiplication of such a substance at all, even in cells

infected by it, we should be driven, I think, to the hypothesis which has been freely used to account for the propagation of bacteriophage, on the one hand, and of typical viruses like that of herpes, on the other; namely, that the presence of the virus in a cell in some way constrains the metabolism of the cell to produce more. Bordet has used the reproduction of thrombin by the clotting of the blood, as an analogy for the suggested reproduction of bacteriophage in this manner. Another, and perhaps closer, analogy might be found in recent evidence that a culture of pneumococcus, deprived of its type-specific carbohydrate complex, can be made to take up the carbohydrate characteristic of another type, and then to reproduce itself indefinitely with this new, artificially imposed specificity. The response of the cells of the animal body, to even a single contact with a foreign protein, by the altered metabolism producing immunity, and often persistent for the lifetime of the individual, may suggest another parallel; but here the protective type of the reaction is in direct contrast to the supposed regeneration by the cells of the poison which killed them. Boycott, again, has emphasised the difficulty of drawing a sharp line of distinction between the action of normal cell-constituents, which promote cell-proliferation for normal repair of an injury, and the virus transmitting a malignant tumour, or that causing foot-and-mouth disease. I do not myself find it easy, on general biological grounds, to accept this idea of a cell having its metabolism thus immediately diverted to producing the agent of its own destruction, or abnormal stimulation. It is almost the direct opposite of the immunity reaction, which is not absent, but peculiarly effective in the response of the body to many viruses. It is difficult, again, to imagine that a virus like rabies could be permanently excluded from a country if it had such an autogenous origin. The phenomena of immunity to a virus, and of closely specific immunity to different strains of the same virus, are peculiarly difficult to interpret on these lines. This conception, however, of the reproduction of a virus by the perverted metabolism of the infected cell, has been strongly supported by Doerr, in explanation of the phenomena of herpes. There are individuals in whom the epidermal cells have acquired a tendency to become affected by an herpetic eruption, in response to various kinds of systemic or local injury. From the lesions so developed, an agent having the typical properties of a virus can be obtained, capable of reproducing the disease by inoculation into individuals, even of other species, such as the rabbit, and exciting, when appropriately injected, the production of an antiserum, specifically antagonising the herpes infection. Such phenomena have a special interest for our discussion, in that they can be almost equally well explained by the two rival conceptions. One regards the herpes virus as a distinct ultramicroscopic organism, and the person liable to attack as a carrier, in whom the virus can be awakened to pathogenic activity and multiplication, by injuries weakening the normal resistance of his cells to invasion. The other regards it as a pathogenic principle produced by the cells in response to injury, and awakening other cells to further production when it is transmitted to them.

This forms a good example of the central difficulty which we should try to solve in to-day's discussion, on the group of agents at present classed together as viruses. They seem to form a series; but we do not know

whether the series is real and continuous, or whether it is formed merely by the accidental association, through a certain similarity in effects, and through common characteristics of a largely negative kind, of agents of at least two fundamentally different kinds. If we approach the series from one end, and watch the successive conquests of microscopical technique; or if we consider the phenomena of immunity over the whole series; we are tempted to assume that all the viruses will ultimately be revealed as independent organisms. If we approach from the other end, or consider analogies from other examples of a transmissible alteration of metabolism, we may be tempted to doubt the significance of the evidence provided by the microscope, and to conclude that all viruses are unorganised, autogenous, toxic principles. If we take the cautious attitude of supposing that both are right, and that viruses belonging to both these radically different types exist, where are we going to draw the line? Is the test to be one of unit dimension? If so, what is the lower limit of the size of an organism? Are we to suppose that inclusion bodies can only be produced by viruses which are independent organisms? And if so, does this conclusion also apply to the 'X' bodies associated with the infection of plant cells by certain viruses? If we try to form an estimate of the lower limit of size compatible with organisation, I think we should remember that particles which we measure by filters of known porosity, or by microphotographs, need not be assumed to represent the virus organisms in an actively vegetative condition. They may well be minute structures, adapted to preserve the virus during transmission to cells in which it can resume vegetative life. Attempts to demonstrate an oxidative metabolism in extracts containing such a virus, separated from the cells in which it can grow and multiply, and to base conclusions as to the non-living nature of the virus on failure to detect such activity, must surely be regarded as premature. Our evidence of the vitality of its particles is, as yet, entirely due to their behaviour after transmission. They may accordingly contain protein, lipoid and other molecules in a state of such dense aggregation, that comparisons of their size, with that of the heavily hydrated molecules of a protein in colloidal solution, may well give a misleading idea of their complexity. Workers in the cytology of genetics, accustomed to picturing a complex of potentialities as somehow packed into the compass of a gene, may find less difficulty, than does the bacteriologist, in attributing sufficient organisation, for true self-reproduction, even to particles still far beyond the range of detection by the microscope. If, in spite of such considerations, we find ourselves forced to the conclusion that some viruses consist of units so minute, that we cannot believe them to be living organisms, I hope we may avoid one common method of expressing the alternative conception, which refers to them as 'enzymes.' I know of no evidence that any enzyme has the properties of a virus, or that any virus has those of an enzyme. We may regard it as a transmissible toxin, when it causes the infected cell to disintegrate, or as a transmissible stimulant, when it induces an abnormal proliferation. In either case we must then postulate, as the special characteristic which makes it a virus, a power of imposing on the cell which it infects an altered metabolism, which leads to its own reproduction.

Apart from their known function as the agents transmitting many of the best known among the acute infections, it is impossible, to anyone having even a slight knowledge of the recent developments which began with the work of Rous and Murphy, to doubt that in the advance of knowledge, concerning the nature of the viruses in general, lies the brightest hope of finding a clue to the dark secret of the malignant tumours. In unravelling what is still such a tangle of contradictions, the animal biologist needs all the help that can be given by concurrent study of the analogous phenomena in plants.

One word, in conclusion, as to the choice of such a subject for a centenary meeting. It might seem appropriate, on such an occasion, to consider in retrospect the advance of physiology and medical science during the completed century. It seems to me not less appropriate to look forward into that which now begins. For the study of infection, the past century, and the latter half of it especially, has been the epoch of the visible bacteria. The new century seems likely to give us an epoch of not less important discovery concerning the viruses. The methods for their study are now taking shape, and what seemed to be immovable difficulties are beginning to yield to patience and ingenuity. Perhaps a new Pasteur will arise, to reconcile the still scattered and conflicting indications in an order yet unseen. Perhaps the advance may continue, as it seems at present, to be along a wide front, common to many workers in many countries. In any case, I think we may now feel some confidence that advance will continue, and that progress will be steady, towards the knowledge for which mankind is waiting, and without which we have no power to deal with many of the worst diseases which afflict us and the animals and plants on which we depend for our life.

SECTION J.—PSYCHOLOGY.

ON THE NATURE OF MIND.

ADDRESS BY

CHARLES S. MYERS, C.B.E., M.D., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

Si monumentum requiris, circumspice. The centenary meeting of a scientific association is one of those 'monumental' occasions when the attention of its members naturally turns to a survey of the modern development of the branch (or branches) of science in which they are variously interested—a survey comprising both the progress of the subject within the Association and the present position which it holds outside. For this reason, no doubt, in many sections the Presidential chair is filled this year by some senior member who through length of personal experience is in a favourable position to review the history of his subject and of the changes he has seen it undergo.

Psychology and the British Association.

Psychology was specifically recognised by this Association as a separate science in the year 1913, when for the first time it was constituted a Sub-section under Physiology, which had itself been established as an independent Section (distinct from Biology) in 1893. This Sub-section of Psychology continued to function as such at the successive meetings of the Association held in the years 1915, 1916, 1919, and 1920, when it was accorded the rank of a Section.

It is perhaps noteworthy that so long ago as 1906 Sir Edwin Ray Lankester, in his Presidential Address to this Association at York, included a short section devoted to psychology, in which he said: 'I have given a special heading to this subject because its emergence as a definite line of experimental research seems to me one of the most important features in the progress of science in the past quarter of a century. . . . Hereafter, the well-ascertained laws of experimental psychology will undoubtedly furnish the necessary scientific basis of the art of education, and psychology will hold the same relation to that art as physiology does to the arts of medicine and hygiene.'

Since then the applications of psychology have extended to medicine, industry, anthropology and other branches of knowledge. The importance of its relations to physiology and biology was clearly enunciated in the Presidential Address of Sir Charles Sherrington at the Hull Meeting of this Association in 1922. He asked—'And if we knew the whole how of the production of the body from egg to adult, and if we admit that

every item of its organic machinery runs on physical and chemical rules as completely as do inorganic systems, will the living animal present no other problematical aspect? The dog, our household friend—do we exhaust its aspects if in assessing its sum-total we omit its mind? A merely reflex pet would please little even the fondest of us. . . . But this Association has its Section of Psychology. . . . It is to the psychologist that we must turn to learn in full the contribution made to the integration of the animal individual by mind.'

So, ultimately, psychology was given the status of an independent Section, with the previous approval of the Sections of Physiology and Education, at the Cardiff Meeting in 1920. The new Section met for the first time in 1921 under the presidency of Professor Lloyd Morgan, F.R.S. Thus the present Centenary Meeting of the Association marks also the completion of ten years' existence of our Section.

I can vividly recall the doubts which were expressed, not so much in words, as in general attitude, by the Committee of Recommendations of this Association when in 1920 it was asked to consider the formation of a separate Section of Psychology. Such hesitation was probably based on several grounds, not wholly on any one of them. Psychology, it must have been realised, is not immediately concerned with *material* phenomena; unlike these, its 'subject matter,' the mind, cannot be weighed or measured; nor can mind be satisfactorily regarded merely as a blind mechanism. Moreover, as each scientist carries his mind about with him, be he mathematician, physicist, zoologist, physiologist, physician or educationalist, he has always himself felt competent to speak from every-day experience on psychological problems without previous systematic training in the subject, sometimes thus advancing, but probably as often retarding, its progress and its reputation, and always suggesting by such intrusion that psychology neither possesses nor needs any special discipline of its own.

More than thirty years' experience has convinced me that a thorough familiarity with the practice and theory of the psycho-physical methods is essential for reliable systematic psychological investigations of any kind. It is largely to the uncontrolled genius of psychologically untrained experts in other fields that we owe the exaggerated importance which has been variously attached of late to conditioned reflexes, sex, inferiority, behaviour, mental tests, correlations, etc., in psychology. Thus have often arisen the various 'schools' of modern psychology, characterised by the same narrow bigotry as is to be found among contending religious sects, each school almost worshipping its founder, each contributing something of truth and value, but each refusing to recognise truth and value in its rivals, and blind to other important conceptions than its own and to other important problems the investigation of which is essential for the progress of psychological science.

The Mathematician in Physics and Psychology.

But some of the grounds for hesitancy in recognising psychology as scientific or as a separate science have lost much of their force to-day, because of the pronounced change that has since taken place in the

attitudes and beliefs which were in vogue among physicists of that time. No physicists would then have dared, as now, to cast doubt on the sole sway of determinism in the physical world. None of them would then have suggested, as now, the impossibility of predicting what any *individual* atom (or still smaller *individual* entity) will do next. None would have questioned, as now, the universal truth of the second law of thermodynamics or of the principle of conservation of energy. None would have ventured, as now, to suppose that electrons change in the very act of becoming known to us, and that therefore the mental factor is ultimately inseparable from physical investigations. None would then have dared, as now, to conjecture that particles of matter correspond in their properties to certain *group* waves of the ether, the *constituent* waves of which, travelling at an enormous speed, 'guide' and 'direct' the group waves without any energy of their own.

In those days one of the most distinguished physicists refused to accept a theory unless he could make a mechanical model of it; whereas to-day we are asked to believe, e.g. in an inconceivable space or ether of ten dimensions in order that the theory of wave mechanics may describe in the simplest terms what happens when three electrons meet one another. In those days it was urged that 'nothing can be more fatal to progress than a too confident reliance on mathematical symbols; for the student is only too apt . . . . to consider the *formula* and not the *fact* as the physical reality.'[1] But to-day (whether rightly or wrongly) we have passed far beyond the study of mere 'physical realities.' At first called in as a servant, the mathematician has now come 'to assert himself as master.' 'He does not ask permission from Nature when he wishes to vary or generalise the original premises. . . . In geometry . . . he has forgotten that there ever was a physical subject of the same name, and even resents the application of the name to anything but his network of abstract mathematics.'[2] The mathematician is not primarily interested in the physical significance of the variables that he is discussing. His particular interests lie in mathematical operations and in numbers and figures *for their own sake.*

Psychology has been similarly bereft of 'reality' by the operations of mathematicians, present and past. The earliest example of this was the derivation by Fechner of the 'law' which now bears his name, that the intensity of a sensation is proportional to the logarithm of the magnitude of its stimulus. This statement was deduced by purely mathematical procedure from Weber's law that just appreciable differences between sensations depend on a constant ratio between the magnitudes of their respective stimuli. Weber's law, however, was based on direct observation and experiment; whereas Fechner's 'law' was the outcome of purely mathematical calculations which not only neglected a constant appeal to the 'reality' of experience, but ran actually counter to it—neglectful, for example, of the 'facts' (i) that Weber's law holds for only moderate magnitudes of stimuli; (ii) that from the standpoint of conscious experi-

[1] *Treatise on Natural Philosophy,* by W. Thomson and P. G. Tait. Oxford, 1867, vol. i, p. viii.

[2] *The Nature of the Physical World,* by A. S. Eddington. Cambridge, 1929, pp. 161, 162.

ence it implies a *single experience* of difference, not a *difference between two* separate experiences; and that (iii) from the same standpoint we are quite unwarranted in adding together or subtracting from one another two intensities of sensation or two sensation differences as such.

Elsewhere in his treatment of psychological data, as in his treatment of physical data, the mathematician has arrived at results that can directly be neither verified nor rejected by conscious experience. The establishment of 'general mental factors,' involved in and influencing the performance of various mental tests and other processes, affords us another example. By mathematical operations on experimental data we can, it is claimed, deduce the existence of such general factors. But from the strictly psychological standpoint the nature of these factors cannot be interpreted; for we are unable to appeal to direct experience to ascertain what these factors are. At best their significance in terms of actual experience can only be conjectured by abstraction and imagination; or it is expressible only in terms of general behaviour. At worst, as in the case of *g* (the so-called 'general intelligence') we are quite ignorant of their psychical nature. We cannot hope for direct psychological evidence as to the precise mental nature of such mathematically deduced 'factors.'

It may be argued that the same ignorance holds in physics, say for electricity or gravity, the relations of which to other physical phenomena we can experimentally and mathematically determine, yet of the *nature* of which we are entirely ignorant through direct experience. But it may be retorted that electricity or gravity is an independent *external non-mental activity*, which is only related to conscious experience for its interpretation and conception; whereas the results of psychological investigations must ultimately be expressible in terms of concrete *conscious experience*, not merely in terms of mathematical abstractions or of any physical activity which is fundamentally independent of such experience.

With perhaps greater force it may be argued that introspection, by which alone conscious experience can be directly studied, is unreliable and not amenable to scientific methods—valid only for the particular individual who introspects, communicable to others only by outward behaviour, and fallible owing to illusion, rationalisation and other causes of error; and that just as physical experiment deals with such terms as electrons and quanta which are beyond the sphere of immediate experience, just as mathematical calculations yield for physics conceptions the realisation of which may be inconceivable by conscious experience, so modern experimental and mathematical psychology has a perfect right to express mental processes in terms which are foreign or even unknowable to conscious experience.

Psychology and Behaviour.

The escape from such difficulty on the physical side—so as to avoid dethronement of the literally divine claims which some mathematicians have made for the fundamental truth of their own science—is to regard the Universe as constituting a vast nexus of ultra-physical and mathematical necessities and probabilities, only some of which can become physical through the further operation of the human mind. We might perhaps adopt a corresponding attitude towards some of the subjectively

unverifiable conclusions arising from the applications of mathematics to psychological data.

But in psychology an additional difficulty confronts us. We have to recognise that the data with which the 'mathematical psychologist' operates are not measurements of the fundamental subject-matter of psychology—conscious mental processes. (Nor can they probably be measurements of unconscious processes, so long as the latter are regarded as mental in character.) For mental processes are not directly measurable: we can *grade* a series of conscious experiences according to their degree or amount, say of hue, brightness, loudness, pitch, temperature, extent, duration, clearness, pleasantness, etc.: we can say that one member of such a series has more or less of any one such character or quality than another member, or that the difference between two members of a series in respect to any one of these characters or qualities is greater or less than, or equal to, the difference between two other members. We cannot deny that 'whatever exists exists in some amount.'[3] But the psychologist can only *measure* the amount of any conscious experience indirectly—either by reference to behaviour, i.e. to the organism's *physical* response or expression, or by reference to the *physical* character of the relevant stimuli, in terms of objective standards of number, space and time which are immediately independent of actual subjective experience.

Let us remember, then, that when we are attempting to measure any mental ability or character or quality by means of a test or series of tests, we are not directly measuring that mental ability or character or quality, but only the corresponding stimulus or the outward response or expression by which that mental ability or character or quality is manifested. We are, no doubt, justified in assuming a broad correlation between the speed, accuracy, amount, etc., of the response or expression of a mental ability or character or quality and the degree in which that mental ability or character or quality is present. Even this broad assumption, however, is sometimes unjustifiable, as in the case where too much of a given mental ability or character or quality may lead to a deterioration, and no longer to an improvement, in the corresponding performance. But we are certainly never justified in assuming that we can accurately measure any mental process by measuring its objective response—that, for instance, twice the amount of the response necessarily means twice the quantity of the mental process of which the response is the expression. All that we are measuring is *behaviour*—that is to say, something largely on the efferent side, something largely physical and indescribable in terms of pure immediate experience, involving a complex of factors many of which, indeed, may be remote from those which we commonly believe we are measuring; whereas what we ultimately aim to deal with in psychology is *experience*—the meeting point of the afferent and efferent sides.

In fact, then, the 'behaviourists' are quite right when they insist that scientific measurement is applicable only to the behaviour of the organism. Where they are quite wrong is in their assumption that conscious processes must necessarily be ousted from scientific psychology,

[3] 'Measurement in Education,' by Thorndike. *xvii Yearbook of Nat. Soc. Stud. Educ.* 1922, Pt. I, 1–9.

because measurement is excluded; the truth being that, even where measurement is excluded, the possibilities of systematic observation and experiment still remain. Natural science has surely a function wider than that of merely reducing its subject-matter to units of space and time. Highly valuable and deserving of the utmost encouragement as is the measurement of behaviouristic data, however helpful be the light they may ultimately throw on mental processes and their general characters, however wider be mental processes than the range of mere conscious experience, the scientific study of the mind by direct observation and experiment is never to be discountenanced or discarded.

Self-activity.

Just as experimental physics patiently pursues its researches into Nature, heedless of such mathematical conclusions as are not amenable to verification by experiment—so experimental psychology must realise that its progress is not primarily dependent on, however much assistance it may receive from, the work of those who fail to recognise that the fundamental subject-matter of psychology is conscious experience, not conduct. Now conscious experience can only be enjoyed by the active self, i.e. the 'individual' (i.e. undivided) mental activity of the entire living organism. It is the fundamental function of such self-activity—by recourse to past experiences, by receiving present experiences, by foreseeing future experiences and by creating new experiences—to select from alternative responses and from alternative environments those which are most advantageous to the ever evolving and developing organism. To secure the most suitable movements and environment and thus to help in the evolution of the organism are the prime objects of consciousness; and where, as in plants, mobility and plasticity are at a minimum, self-activity and consciousness are inappreciable. Self-activity is to be regarded as the highest, unitary integration of the directive mental (conscious and unconscious) activity of the organism.

The Derivation of Presentations (or Contents) from Feelings.

Self-activity and its inherent consciousness may presumably be traced back to a stage where self and not-self are but just distinguishable. At this remote stage in animal evolution there can hardly have been more than a differentiation of self-activity into 'acts' of the self and 'modifications' of the self. These modifications of the self became early differentiated into (*a*) those which are due to internal happenings within the organism, and (*b*) those which are due to more variable external happenings in its environment, and later into (*a*) those which we come to recognise as 'affects' *of* the self and 'feeling tones' and (*b*) those which come to be regarded as 'presentations' *to* the self or as 'contents' of the self's consciousness. Sensations, perceptions, memories and thoughts—all that we finally come to recognise as conscious *presentations* to the self—have been differentiated (onto- and phylo-genetically) from modifications of the self: instead of being *feelings* of the self, they have become *contents* of consciousness.

We end by 'projecting' certain of these original feelings. The external 'objects' of our perception have been separated from or carved out of originally vague external 'situations' of which we or our remote ancestors were first conscious merely as diffuse modifications or feelings of the self. So, too, any colour or sound comes to be regarded no longer as a self-feeling but as a something projected and existing outside us. The degree to which such projicience and presentation is carried out varies with different sensations: colours clearly have a projected, apparently independent, existence; sounds, smells, tastes, hardness and temperature are only imperfectly projected; the painful prick of a pin and our sensations of movement, though not projected, are nevertheless regarded as 'presentations' to the self; whereas our experiences of visceral sensations are hardly even presented: they seem almost as clearly modifications of the self as are our emotions and other affects.

The Origin of Acts and Contents.

This difference between the acts and the contents of consciousness—between the conscious *acts* of apprehending, recalling, deciding, inferring and *what* is consciously apprehended, recalled, decided, inferred—is a most important one. It is exemplified in the two kinds of memory which are distinguishable. On the one hand, we may recall the *separate acts* of the self, say, in the course of solving a problem or of acquiring some specific skill; these are individually unique and only individually revocable. On the other hand, we may recall the *generalised contents* of our consciousness, i.e. of presentations we have received by a repetition of such acts—e.g. in learning a prose passage or series of skilled movements.

I would suggest that the distinction between conscious acts and contents has come about with the gradual differentiation of higher and lower levels of mental activity—and in the following manner. There is no awareness of self-activity when we sense a colour or a temperature, or when we perceive a familiar object, or when an idea 'occurs' to us. Our sensations, our perceptions and many of our thoughts and ideas are, I suggest, the unconscious 'acts' of relatively lower mental levels. But when these lower-level 'acts' are accompanied and received by the self-activity of the highest levels, they become *ipso facto* 'presentations' to the self. A loud noise to which we are impelled to attend or an idea which 'occurs to the mind' is not a conscious presentation (or content of consciousness) until the self receives it.

I suggest that such differentiation of higher and lower levels has never occurred to the same extent in the case of our kinæsthetic and, especially, cœnæsthetic sensations and in the case of our feelings (which depend on a more primitive, thalamic, activity): we fail, therefore, to objectify them immediately as presentations, and they continue to be received in their primordial undifferentiated state.

By virtue of recall, however, even the acts of the self and its feelings can become more or less objectified as 'presentations' (although, of course, they are not 'projected' as independent objects). The acts of decision or apprehension and the emotions, attitudes, etc., of the self at one moment can through lower level transfer become at the next moment themes of contemplation by the self. Thus, we may account for the occur-

rence or absence of presentations in many varied circumstances. E.g. (*a*) the fully developed human self not only knows, but knows that it knows, and so on. (*b*) A really efficient actor must not so wholly lose ' himself ' in the part that he is playing as no longer to know that he is acting; he must have at least some measure of objectivation of and self-control over the self that he is portraying. (*c*) So, too, for its full æsthetic enjoyment of a play, an audience must not wholly lose its ' self ' in the scenes it is witnessing: an audience must enter some distance into the play, but for the highest appreciation of beauty a certain ' psychical distance,' as Bullough[4] has called it, neither too close nor too remote, must be preserved. (*d*) In certain cases of multiple personality (cf. Morton Prince's Sally[5]) the self may look down upon the acts of one or more other selves who behave as actors in command of the situation. (*e*) In the abnormal condition known as ' depersonalisation ' the self's experiences may temporarily seem strange and the very acts of the self may seem strange, so that it appears as if some other personality than the self were acting and experiencing, the highest self once again looking on, so that what in normal conditions would be regarded as the self's own experiences become projected as the experiences of another lower self. (*f*) Similar changes occur in certain phenomena of hypnotism and ecstasy.

The Redintegration of Multiple Personalities.

It is a matter of common experience that in our normal selves our personality is ever changing according to our environment. We are one person in the conduct of our business or profession, another during our play, yet another in the bosom of our family; and we act and feel accordingly. But whereas, normally, our single self is behind all the acts and other experiences of these different personalities, there also occur those well-known abnormal conditions of ' multiple personality ' in which these personalities exist as alternating selves, often in the apparently complete absence of such single higher activity. It is interesting, however, to observe how redintegration may occur in those cases of alternating personality where a highest self seems to be continuously present, however far banished to the background. This is well illustrated in the case of the Rev. Thomas Hanna, who thus describes the final phases of his recovery.

' The first mental struggle was during the very next primary state, which, by the doctors' earnest request and my own extraordinary effort, was already prolonged to three or four hours. . . . Suddenly there was a glimpse of the secondary life, only a glimpse, it is true, yet a revelation of infinite wonder as being the first real insight into one state from the other. Instantly the thought came " What is the use of enduring this severe struggle when invited into that attractive life, the secondary state ? " . . . But saying mentally again, " What is the use ? " there was a letting go, and the primary life was again lost. . . .

[4] Cf. ' " Psychical Distance " as a factor in art and an æsthetic principle,' *Brit. J. of Psychol.*, 1912, Vol. V, pp. 87–118.

[5] *The Dissociation of a Personality.* By Morton Prince. London: Longmans, 1906.

'I was still in the secondary state, but the other life dawned on me, and nothing but my will pertinaciously clung to the secondary state. . . . While both lives were presented to the mind, where was the possibility of combining them ? And had not I lived and felt each life ? Yet how could one person live and feel both lives ? Here was the critical point. . . But the lives were constantly becoming more and more personal, until at last, by a deliberate, voluntary act, the two were seized, and have both remained . . ., though for some time after the recovery it was difficult to dovetail together the detached portions of each life so as to present a continuous history.' [6]

An Analogy.

Let me return to stress once more the fact that the development of presentations *to* consciousness out of what were originally modifications or feelings *of* the conscious self is to be regarded as one of the most important features of mental evolution. It has occurred, as I believe, *pari passu* with the fuller development of the self and with the increasing complexity and differentiation of mental levels. The early self in more primitive organisms functioned in a manner so diffuse that it can barely be called unitary. Its later, more highly concentrated, unitary character developed with the differentiation of higher and lower levels and with the gradual distillation of supreme control into ever less diffused, loftier and more 'pontifical' spheres of influence. (Perhaps roughly corresponding, on the material side, to these primitive and later mental stages are the early plexus structure and the later synaptic plan of the nervous system.)

We might compare the earliest stage with that of a primitive monarchical government whose king was the weak, diffusely moving spirit in all its varied activities. At an intermediate stage we might envisage a stronger government whose cabinet consisted of a large number of members, each, however, busily acting in considerable independence of his colleagues and of his chief, the prime minister. At the highest stage we may conceive differentiation, co-ordination and integration as having so harmoniously co-operated as to produce a prime minister who is in perfect sympathy with, and hence functionally identical with, the king, and has such complete control that he regards the more important 'acts' of his hierarchy of lower-level colleagues (even those of his deputy and assistant prime ministers) as his own 'presentations.' By some such analogy as this, I suggest, we can dimly portray the evolution of the self, its increasing powers of control and devolution, its development of the function of presentation, and its ability, in certain conditions, to look down on what appears to be itself, or on one or more other selves, acting and experiencing feelings and presentations.

The Functions of Felt Impulse and Emotion.

The humblest servants of such a highly complex government would be entrusted with duties which they can perfectly well perform without ever

[6] *Multiple Personality*, by B. Sidis and S. P. Goodhart. London : D. Appleton. 1905, pp. 225, 226.

troubling, or indeed being able to have access to, the members of the cabinet. Such functions are comparable to our present reflex actions (e.g. the pupil reflex), which are inherited, unalterable, and are absolutely divorced from consciousness. There would, also under the government, be servants of a higher level, comparable to the instincts which are improvable by experience, the activities of which affect consciousness in the form of impulses to action. One manifest purpose of the consciousness or awareness of impulse is to enable the self to modify or to control the relevant instinct. Instincts may war with one another (so, too, may alternative motives to voluntary action). The self may *passively* allow the stronger instinct (or the stronger motive) to predominate. But it may also, by using its own *activity*—implying the whole, most highly integrated, mental system or personality of the individual—interfere with and repress an instinct (or a motive) which, left to itself, would otherwise predominate and yield involuntary action.

Whether or not we can regard all instinctive activities, e.g. walking, as accompanied by emotional consciousness, a close association between instinctive action and many emotions generally holds. Emotional activity is psychologically and neurologically older than higher intelligent activity. Where an emotion, closely related to some instinct, enters consciousness, its probable object is to prevent the self from intelligently inhibiting the related instinct, and to insure the carrying out of the instinctive act. Such emotions, indeed, are more strongly felt in proportion to the amount of conflict or other obstacles impeding expression of the relevant instinct.

Developments of Feeling and Presentation.

We have noted two paths of differentiation of the modifications of the self—those of feeling and of presentation. Feeling develops, on the one hand, into emotion and this into sentiment, and on the other, into feeling-tone (pleasure and displeasure). Feeling-tone, emotion and sentiment are recognised as being largely dependent on thalamic activity; whereas with the development of the cerebral cortex arise the increasing integration, discrimination and grading of presentations, the elaboration of their meaning and of their spatial and temporal relations, and the evolution of thought and speech, on all of which the rise of rational intelligence depends. Finally, valuation and volition achieve their highest plane with the harmonious co-operation of the highest products of these two evolutionary paths—sentiment and intelligence.

Unconscious Direction and Purpose.

The self is the highest controlling and directing power. The orders which it consciously gives and the efforts which it consciously makes may, once started, continue to be carried on unconsciously, i.e. without the conscious participation of the self. Thus we may consciously but vainly try to recall some past experience or to solve some difficult problem; and after giving up the effort, this directive activity may still persist unconsciously until suddenly the forgotten object, or the abandoned solution, suddenly flashes full-born and unbidden into the self's con-

sciousness. So, too, we may go to sleep determined to wake up at a given hour, or we may accept, in the hypnotic state, a decision to carry out some prescribed act on the lapse of a prescribed period of time after emerging from that state; and at the ordained moment the sleeper wakes, or an uncontrollable impulse is felt to perform the suggested act.

But not only is purposive activity not limited to the duration of conscious activity; it need not originate there. The inspirations of genius and the intuitive judgments and decisions which, crude though they may be before submission to the self's judgment, arise apparently from the 'depths' of the mind with impulsive force and compelling conviction afford striking examples of this fact. The well-known improvements in learning which continue after we have ceased to practice, so that it has been said of us that we learn to skate in summer and to swim in winter, are further examples of such activity—whether or not we choose to ascribe such improvement to the gradual disappearance of adverse initial inhibitions or to the direct strengthening ('consolidation') of acquired integrations (or associations). Further, the self is continually being played upon both by the impulsive and by the perseverating forces of lower mental systems. They struggle, not less than the self, for their own existence and for their own lower 'self-ish' ends. Where they are modified by inhibition (or repression), it is only to ensure general harmony and general compatibility. Inhibition is not to be viewed as a mere act of passive drainage of energy from one mental constellation to another, but as an active repressive force against which the inhibited constellation ever tends to rebel in its endeavour to gain somehow or another liberty of action, in some lower degree purposeful and directive.

The Psychical Indescribability of the Unconscious.

Despite certain unscientific methods and no little prejudice in interpretation and procedure on their part, we owe a debt to the psycho-analysts for their detailed study of the conflicts responsible for such repressions and of the ways in which repressive forces exert their influence and are not infrequently, as it were, outwitted. But let us not imitate the psycho-analysts in their failure to recognize that we can never describe the nature of unconscious mental processes in terms of consciousness. We are as powerless to do this as we are powerless to describe the nature of God in terms of the human body and mind. 'Psycho-morphism' in psychology is an error not less cardinal than anthropomorphism in religion. We are bound to adopt an agnostic position as to the nature of the unconscious. To describe in the language of consciousness an 'unconscious wish,' an 'unconscious motive,' an 'unconscious emotion' or an 'unconscious idea' is a contradiction in terms. At best we can but say that if a particular unconscious mental process were to become conscious, it would manifest itself as a certain wish, motive, emotion or idea. But the extremely uncertain nature of such statements must be kept ever before us.

The Cerebral Localisation of Consciousness.

While consciousness always implies self-activity, and while the self is to be regarded as the expression of the highest level of mental activity,

we must guard against the notion that such high-level activity implies a narrowly-limited zone of mental processes. On the contrary, it implies a wide sphere of activity rather than a punctate, pineal gland-like soul. It follows, therefore, that we cannot hope to localise any act or any content, of consciousness in one small region of nervous substance. Afferent-efferent localisations of function undoubtedly occur—regions where the incoming impulses become deflected to outgoing processes : our knowledge of the physiology and structure of the spinal cord clearly points to this. Sensori-motor localisations may, in a sense, be said to exist similarly in the brain. The occipital region of the cerebral cortex, for example, is concerned with vision. But because vision ceases when the *area striata* in this region of the cortex is injured, we are not justified in saying that this area is the *seat* or centre of our visual consciousness. All that we are warranted in concluding is that it is *essential* for our visual consciousness, that without it vision is impossible—a very different statement.

Once again, let me repeat, consciousness implies self-activity. There are no separate loci for different kinds or modes or qualities of consciousness. The nervous system and the system of self-activity works as a wide-spread unity. Different regions of the brain are more particularly concerned in *giving rise* to certain kinds of consciousness. The thalamus, for example, is especially concerned with the emotional consciousness ; but we are not justified in calling it the *seat* or centre of such consciousness.

It is impossible to localise consciousness. There are no specific 'mental' symptoms diagnostic of cerebral tumours in different regions of the brain. The work of recent experimenters[7] suggests that the *more complex* skills depend for their acquirement on the activities not of any particular cortical area but of wide areas of cerebral tissue. The larger the area of brain destroyed, the slower is the rate of subsequent acquirement of such skills, and in the rat, at least, this seems independent of the locus of the lesion. The same conditions appear to hold for the destructive effects of cortical lesions on complex skills which have been already acquired. Various *simpler* skills, on the other hand, appear to depend for their performance on the integrity of isolated mechanisms specific, in the normal animal, to definite areas of the cortex ; but after their destruction, their functions may nevertheless be taken on by other regions of the nervous system. Such skills, once acquired, are hence abolished by injuries to some particular cortical area, but to no other area. Nevertheless, in the rat the destruction of that particular area does not appear to affect the subsequent acquisition of the *simpler* skills, and there is no relation between (*a*) the magnitude (within certain limits) or the locus of wide cortical injury and (*b*) the ease of such acquisition. Doubtless the number of such localised mechanisms increases *pari passu* with mental evolution, and at the same time their diffuseness diminishes. But the number of possible complex skills, involving the activity of wide areas of the cerebral cortex, must simultaneously increase also.

[7] Cf. *Brain Mechanisms and Intelligence.* By K. S. Lashley. Chicago: University Press, 1929.

It would appear, then, that the *higher* processes of learning, of comparison, and indeed of all that underlies higher intelligent activity, have a *generalised* localisation—comparable, perhaps, to the *diffusion* of epicritic sensibility over the skin as contrasted with the more primitive, ungraded, *spot* systems of protopathic cutaneous sensibility, which in turn may perhaps be compared with the *narrow* so-called localisation of visual, auditory and other 'sensory' and low-grade areas in the cortex. But even where it exists, cortical localisation is only relatively definite. The boundaries of a given 'motor area' in the cerebral cortex fluctuate widely in different individuals; they vary also in the same individual according to the direction of exploration, previous exploration and other factors; and, as we have already noted, other areas may successfully assume the function of that area when it is destroyed. A certain degree of 'equipotentiality' exists throughout the brain, although some cerebral regions appear normally to have fairly definite, circumscribed, lower-level functions.

We are, in fact, neither warranted in supposing that there are definite seats or centres of sensation or emotion, nor justified in supposing that our manifold percepts, images or ideas each have their seat in different narrowly localised centres of the brain. And a similar truth holds for the association (or integration) of such experiences. We can *mentally* picture an integration of two 'patterns' of conscious activity occurring when two experiences *a* and *b* follow one another repeatedly, so that when *a* is later given, *b* (or rather the whole *a–b*) recurs. But *neurologically* we can form no simple corresponding picture of two collections of nerve cells being associated together. We have no evidence to support such cerebral localisation of association areas; indeed such experimental evidence as we have is against it.

Even if there were no evidence pointing in one direction or the other, how could such localisation of memories and habits possibly occur? Consider the babe that is learning to associate its mother with the satisfaction of its hourly wants. Its mother is never twice the same—now in one dress or facial expression, now in another; and the visual image of the mother received by the retina is never twice the same, e.g. sometimes the mother is very near, sometimes further off; sometimes the image falls on one part of the retina, sometimes on another. How can we imagine, then, any definite collection of retinal or cortical nerve cells responsible for developing the image of 'mother'? What develops is surely rather a 'meaning'—a generalisation of images, 'standing for' something, i.e. for the assuaging of certain needs, for the execution of a wide range of adjustments of the infant.

Relationships and meanings are therefore the all-important mental acquirements. The acquisition of such relationships is shown, for example, in the many experiments conducted on a large variety of species, high and low in the animal scale, where by long practice the organism is trained to enter B, the brighter of two alternative compartments, A and B, in order to reach its food. When later, in place of A and B, B and C are substituted, C being now brighter than B, does the animal go to B to which it had previously been trained to go? Generally, no. It enters the C compartment. That is to say, it has learnt to enter not a *particular*

compartment, but the *brighter* of any two compartments. It has not learnt to select a 'particular' object. It has learnt a 'relation.' Surely evidence of this kind is contrary to any atomistic localisation of individual mental functions in separate cerebral areas.

The Relation between Directive and Mechanical Activity.

The fundamental purpose of consciousness is to enable the self to preserve the organism by guidance and direction,—by the formation and satisfaction of ends and values. As in the evolution of living species something far more is involved than the mere blind running down-hill of a wound-up mechanism, so in the mental and bodily life of each organism the physical conceptions of 'entropy' and of mechanical energy are inadequate. On the physical side we can form no conception of the mode of working, throughout life and mind, of anything resembling Clerk Maxwell's directive 'demon.' Physiologically, that is to say physically, the brain-worker should need food with a far lower caloric value than he actually takes and requires for the successful maintenance of his purely mental activities. But in fact[8] mental work appears to make far greater demands on metabolism than it should according to purely physical considerations of the expenditure of mechanical energy.

At present we can form no conception of the nature of the undoubted connection between chemical metabolism and direction in the living organism—between senility of body and senility of mind, between the rise and decay of procreativeness and the rise and decay of the creativeness of genius. At present we can form no bridge between mechanical and creative, directive activity. We can only say that both activities are essential to a conception of the evolution and working of life and mind. Mental activity is but the quintessence of the non-mechanical, directive activity of life ; and consciousness is but that activity raised to its highest power. Even lower-level mental and neural systems, even the activities of the lowest living organisms are characterised by unconscious creation, direction, guidance and purpose, in varying degrees. But conscious creation and direction, the consciousness of *acts*, is limited to the highest-level psycho-neural activity—the self.

In this address I have suggested that, when the physiological activities of the lower-level systems meet with the highest-level activities, they may become manifest as conscious *presentations* ; these highest-level activities are, I believe, to be regarded as arising from the supreme organisation and distillation of the directive activities of the living organism. The acts at this highest mental level constitute the purposeful, directive, creative and contemplative self, and are the recipients of presentations from lower cortical, and also of feelings from lower, primordial, thalamic activity.

The psychologist's principle of the *conservation of self*, which corresponds to the biologist's inevitable principle of the *struggle for existence*, is the fundamental function of this conscious activity. It is as real and important

[8] Cf. 'A Study in Nutrition.' By E. P. Cathcart and A. M. T. Murray, Medical Research Council's Special Report, No. 151. H.M. Stationery Office, 1931.

as the physicist's principle of *conservation of energy.* We must leave to the future the task of bridging the present impassable gulf which yawns between these two principles. Meanwhile let us always remember that blind mechanism in the material world is a truth not more fundamental than the reign of guidance, creation and purpose in the world of life and mind, and, it may well be, throughout the Universe; indeed our very notions of these two principles governing perhaps both the living and lifeless world appear to be the outcome of, even if they do not wholly depend upon, the experiencing, reasoning and imagining self.

SECTION K.—BOTANY.

THE ADVANCEMENT OF BOTANY.

ADDRESS BY

PROF. T. G. HILL, D.Sc., A.R.C.S., F.L.S.,

PRESIDENT OF THE SECTION.

My first duty is an act of piety: the commemoration of our fellow botanists who have died since the last meeting of the British Association.

Martinus Willem Beyerinck; Thomas Ford Chipp; Heinrich Gustav Adolf Engler; Jakob Eriksson; Thore Christian Elias Fries; Emil Godlewski; Hans Kniep; Rudolf Marloth; Spencer le Marchant Moore; Charles Edward Moss; Sergius Navaschine; Carl Emil Hansen Ostenfeldt and Richard von Wettstein.

Give Honour to their Memory.

My predecessors in this Chair have, for the most part, concerned themselves in their presidential addresses with those aspects of botany in which they were investigators. On this occasion, however, I am taking the risk of breaking the tradition, and this for two reasons: my immediate interests are rather too specialised for a general audience; and secondly, this is a special occasion, a centenary meeting, and therefore calls for some review of the past, a wholesome thing now and then to do. But for an hour's discourse the volume of literature is overwhelming; I shall, therefore, only attempt to give in outline mine own impressions.

In considering the history of botany, there would appear to be four epochs: the first of sporadic and unco-ordinated investigations, often of first-rate importance; the second, the development of morphology and physiology; the third, the renaissance in Britain; and the fourth, the post-war epoch. Thus do I propose to order my Address; lightly touching the first phase by way of introduction, and continuing certain aspects of the succeeding epochs forward when the occasion demands.

Introduction.

It has been said that research had to await the printing press, since the circulation of observations, deductions and ideas is the greatest stimulant to enquiry; this is but part of the truth. Advance in knowledge of the physical sciences was only possible when reason and liberty grew stronger than faith and discipline, and for this cause scientific research did not come effectively into being until the seventeenth century. Systematic botany slowly but continuously advanced to our own times, had the fates been kinder to Commerson its momentum would have been greater; but morphology, in the broad sense of the term, and physiology

lagged far behind despite the brilliant lead given by Grew, Hales and Knight, our own countrymen, and that illustrious Italian, Marcello Malpighi.

The Second Epoch, 1831–1882.

Doubtless it is a coincidence that the year 1831 marks not only the founding of the British Association for the Advancement of Science but also, as far as a date can mark, the beginning of a three-fold epoch in botany: the search for a natural system of classification; the emergence of morphology as a study separate from taxonomy; and the development of physiology.

In the first Britain took her full share; Robert Brown, the Hookers, Lindley, Harvey, Daniel Oliver, and Bentham are worthy of ranking with Brongniart, Decaisne, de Candolle, Endlicher, Eichler, Engler, Gray and Warming.

In morphology and physiology, on the other hand, the tale of British activity is dismal indeed: against a battalion of continental workers we can barely range a section in which ranked Robert Brown, Griffith, Harvey, Berkeley, Witham, and Williamson among morphologists, and Draper, Graham, Gilbert, Lawes, Burdon-Sanderson, and Darwin representing plant physiology. It will be seen that during this period British work caused but a ripple on the botanical surface. But there is one exception: the brilliant generalisations of Charles Darwin.

Come December 27 next, H.M.S. *Beagle* sailed from Devonport a hundred years ago.

Morphology.—In 1831, morphology for the most part had relation to the phanerogams only and its concepts were based more on philosophical considerations than on inductive science; the structure and life history of no plant was completely known; the fields of the vascular cryptogams, the bryophytes and the thallophytes were almost untrodden; and the realms for research were thus well nigh unlimited. It, therefore, is not surprising that when ontogeny came into being, the trickle of investigations fast developed into a flood and for this the greatest honour is due to Schleiden, Nägeli, von Mohl and Hofmeister, for they showed the way and contributed the most. Amongst the many famous investigators of this period, names occur and recur. Thus, under Algæ is noted the work of Nägeli, Cohn, Reinke, Pringsheim, de Bary, Woronin, Alex. Braun, Thuret and Bornet. Bornet, in addition to his distinguished work on the Algæ, shares with Stahl the honour of establishing the nature of the lichens, not only by analysis but also by synthesis, and thus proving the truth of the earlier views of Schwendener. Amongst those who laid the foundations of our knowledge of the Fungi were Cohn, Tulasne, Woronin, van Tieghem, who was particularly interested in the mucors, Brefeld and de Bary. In the elucidation of the Muscineæ, Mirbel, Bischoff, Leitgeb and Hofmeister are most famed. The vascular cryptogams attracted many: Cramer, Nägeli, Leitgeb, Bruchmann, Mettenius whose best work was, perhaps, that on *Ophioglossum* and *Phylloglossum*, Millardet, Hanstein, Hegelmaier, Russow, Hofmeister, Cramer, von Mohl, Prantl, Kny, Goebel, de Bary, Strasburger and many others. This recital may be almost without meaning to some, it is not the present fashion to read

history, but to the few it may recall some happy days and some moments of inspiration.

Then the angiosperms: but here I stop, for the commemoration of those who laid the foundations of this knowledge might be wearisome, for their names, which include many already mentioned, unlike the names of the plants they studied, hardly sing themselves as they run. A welter of facts were discovered many of which, especially those relating to the Thallophyta, had to await the arrival of a new technique, that of cytology, before co-ordination was possible; but for the Muscineæ, the vascular cryptogams and the angiosperms the times produced the man whose patient and detailed work, directed by a keen intellect, gave cosmos to morphological enquiry. I refer to Hofmeister, one of the greatest botanists of all time. He first traced the development of the sexual organs and of the embryo in the angiosperm, and then turned to the vascular cryptogams and the Muscineæ and did likewise. He was the first correctly to interpret heterospory: he correlated the asexual spore with the gametophyte and the oospore with the sporophyte; he discovered alternation of generations. Nowadays alternation of generations is accepted as a matter of course, but when it is realised that Hofmeister was working it out, almost cell for cell, more than eighty years ago without the aid of the microtome and of cytological technique, as we know it, it will be appreciated how great was his work. Sachs' judgment, here at any rate, was just: 'Embryology was the thread which guided the observer through the labyrinth of comparative and genetic morphology; metamorphosis now received its true meaning, when every organ could be referred back to its parent form, the staminal and carpellary leaves of the phanerogams, for example, to the spore bearing leaves of the vascular cryptogams. That which Häckel, after the appearance of Darwin's book, called the phylogenetic method, Hofmeister had long before actually carried out, and with magnificent success. When Darwin's theory was given to the world eight years after Hofmeister's investigations, the relations of affinity between the great divisions of the vegetable kingdom were so well established and so patent, that the theory of descent had only to accept what genetic morphology had actually brought to view.'

Anatomy.—In considering the development of anatomical knowledge during this epoch in some small measure of particularity, it is realised that although the many contributed in varying degrees to knowledge, the great advances were associated with the names of relatively few men. Of these, Schleiden and Schwann, the zoologist, are the first to be commemorated since they laid the foundation of the scientific study of anatomy in their theory of the cell which, in a few words, was that the cell is the unit of structure and function in the organism. This, to-day, is a platitude, but in 1838 it was a thesis of the greatest importance since it enabled the co-ordination of, hitherto, jumbled facts into orderly conceptions, and enabled others, especially von Mohl and Nägeli, to build knowledge of the anatomy of plants on a sound basis. Von Mohl discriminated the tissue elements and traced the course of the vascular system, and thus paved the way to comparative work. He was the first accurately to trace the development of vessels even to the partial obliteration of the originally partitioning walls of the superposed elements, and

the deposition of encrusting substances on the parts which persisted. Von Mohl correctly described simple pits but curiously enough did not understand bordered pits which were first elucidated by Schacht. Von Mohl gave the first correct account of stomates; he observed their movements which he associated with variations in the turgidity of the guard cells which he correlated with the accumulation of soluble assimilatory products due to the activity of the chloroplasts. His observations on periderm and cambial activity are of first-rate importance. Von Mohl's technical ability and power of observation were so great that it is, perhaps, remarkable that he did not do more; thus some of his preparations, which I am told are still in existence, show the continuity of protoplasm; he did not describe it, did he observe it? He certainly never agreed with Hartig that the sieve plate was perforated. But Von Mohl was essentially a practical man, slow to draw a conclusion and suspicious of speculation; his work however, was complemented by a man of greater imagination: Nägeli. Nägeli, famous in all branches of botany, traced the differentiation of the desmogen strands into primary vascular tissues and correlated phyllotaxis with nodal and internodal organisation. He investigated the structure of sieve tubes and elucidated the anomalous secondary thickening in angiosperms. He systematised anatomical knowledge and this led to progress, for it is to be remembered that knowledge is advanced by its periodic ordering. His investigations on the cell wall and on the starch grain are classical: the recognition of the cell wall as a product of protoplasmic activity, the striation of the cell wall, growth by intussusception and the occurrence of granulose and amylose in the starch grain, need only be mentioned. The results he obtained by the use of polarised light were as important as those of Sponsler and others by the use of X-rays in our own times.

The investigation of von Mohl and Nägeli were expanded in various directions by Bureau, Crüger, Hanstein, Radlkofer, and more especially Sanio, Th. Hartig and van Tieghem.

Cytology.—Although Robert Brown discovered the nucleus a hundred years ago, fifty years elapsed before its structure and changes were critically studied. This is hardly surprising since the new learning led men to the pursuit of the more obvious; to phytotomy, to use an old word, and to life-histories. Further, cytology had to await the arrival of new methods of fixation and staining and some attainment of the knowledge of the colloidal state; also the microtome, as we know it, had to be evolved. In the earlier study of cytology, von Mohl and Nägeli were the pioneers. Von Mohl was the first to see cell division but, apparently, he did not appreciate the significance of his observations. Later, he studied the 'mucilage' described by Schleiden in 1838: he recognised it as a distinct component of the cell and gave it its present name, protoplasm. He described the primordial utricle and realised that the streaming movements of the cytoplasm were independent of the cell sap. Further, von Mohl perceived that the matrix of the chloroplast was protoplasmic. Nägeli more carefully observed the nucleus and appreciated the facts of cell division more truly; and in this he was the first to recognise that the nucleus is the starting-point. Later, in 1855, Unger suggested the identity of the protoplasm of the vegetable with the sarcode

of the animal cell, and his views were supported by the observations of de Bary on the mycetozoa.

With the passing of time it was recognised that the protoplasm of the plant and of the animal is identical and that it is the basis of life, a conclusion to which Payen, Cohn and Max Schulze contributed much.

Towards the end of this period, the improvement of microscopical technique led to great discoveries in the details of nuclear division. In this the work of Strasburger is outstanding, and to him is due the main credit of firmly establishing that aspect of botany now called cytology. The contributions of Fleming and of Schmitz must, however, not be overlooked, nor those of Guignard who, unfortunately, prejudiced his own work by his description of structures which had no existence.

Physiology.—Our knowledge of osmotic phenomena is founded on Dutrochet's (1827) studies on differential diffusion through a colloidal membrane. Graham, the father of colloid chemistry, showed (1854) that the rate of this diffusion depended, *inter alia*, on the nature of the membrane. This led to Traube's discovery of the co-called artificial cell produced by dropping a crystal of copper acetate into a solution of potassium ferrocyanide, whereby a precipitation membrane of copper ferrocyanide surrounding the copper acetate is produced ; the reacting salts thus become separated by a semi-permeable membrane and the 'cell' grows. This observation, which is periodically rediscovered, was used by Pfeffer (1877) who supported the membrane of copper ferrocyanide in the wall of a porous pot and with it made many highly important observations, more especially the facts that the osmotic pressure is proportional to the absolute temperature and to the concentration of the solution. Thus was born a branch of physical chemistry and one of the first results was van 't Hoff's theory of solutions. At this time de Vries also was busy on the problem and contributed much ; to students he is best known by his plasmolytic method by means of which he ascertained the isotonic co-efficients of many substances.

The mention of osmotic phenomena naturally leads to the ascent of sap and transpiration. Hales was the pioneer and his work is classical ; many years later (1837) he was followed by Dutrochet, who was the first to distinguish betweem root-pressure and the pull of transpiration. Later, the solution of the problem was essayed by Unger, Boehm, Sachs, Elfving, Hartig, von Höhnel and many others : the questions whether the water travelled as water of imbibition or through the lumina of the tracheæ and whether the rise was entirely a physical phenomenon or was dependent on the vitality of the parenchyma of the wood, were warmly discussed. Those interested in the history of the subject will find a good account in an unlikely place, in Marshall Ward's 'Timber and some of its Diseases' (London 1897).

Hales, Priestley, Senebier, Ingen Housz and de Saussure were the pioneers in the study of carbon assimilation, with the result that the more obvious facts were known before 1831. Of these men, de Saussure was the greatest : he was the first to strike a balance sheet which indicated the unity of the photosynthetic quotient, a fact confirmed by Boussingault in 1864, and he was the first to show that water and salts, as well as carbon dioxide, were essential for the nutrition of the green plant. His work,

however, did not convince the upholders of the humus theory; they, for the most part, were silenced by Liebig (1840) who, building on the foundation laid by de Saussure, argued that the only source of carbon in the green plant was that derived from the carbon dioxide of the air. The problem was settled beyond dispute—Liebig was deductive rather than inductive—by Boussingault who, in 1851, introduced the method of water culture and proved that the ordinary green plant could flourish in a soil bereft of organic matter and that its nitrogen came from the nitrates of the soil, not from the air.

Dutrochet (1837) correlated carbon assimilation with chlorophyll and was the first to use the escape of bubbles of gas from a cut shoot submerged in water as an indication of the process, a method adopted and improved by Sachs and used by him in quantitative experiments.

Draper (1844) was the pioneer in establishing the connection between carbon assimilation and the quality of light. He showed that the curve of assimilation was almost negligible in the lower red rays, quickly rose to a maximum in the yellow green and gradually declined towards the blue, which observations were extended by Pfeffer and by Engelmann. The significance of light intensity was not appreciated until recently.

Von Mohl (1851) considered that a carbohydrate was formed from the carbon dioxide, but it was Sachs who by experiment proved that the starch grains in the leaf, first described by von Mohl, were produced from carbon dioxide, and invented that experiment of our elementary classes which shows that the leaves of a green plant placed in the dark lose their starch and regain it on re-exposure to light. Sachs considered that starch was the first product of carbon assimilation; it was not until some years later (1885) that Meyer showed that there were sugar leaves as well as starch leaves and thus indicated that sugar preceded starch in the process.

Turning to the pigments associated with photosynthesis; Grew was the first to suggest that chlorophyll, using the word in its loose sense, was made up of more than one pigment, but to Stokes (1864) is due the credit of making the first real advance and thereby initiating a long series of investigations. By the fractional extraction of crude chlorophyll by organic solvents, Stokes demonstrated the presence of two green and two yellow pigments. These he studied optically and made certain observations on their chemical properties. His work was continued by others, notably by Fremy, Tmiriazeff, Kraus, Konrad, Sorby and particularly by Borodin, who in 1883 confirmed the discovery of the two carotinoids. The methods of separation of the mixed pigments were in time refined and culminated in the work of Tswett (1906) who, by his chromatographic method, separated two chlorophylls, thus confirming Stokes' observation, and five carotinoids, of which one was carotin and the others xanthophyll.

Many years were to elapse before the chemical constitution of chlorophyll was to be elucidated, but Hoppe-Seyler was the first to make the significant discovery, by the isolation of phylloporphyrin from chlorophyll in 1879, that there is a chemical relationship between the green pigment of the plant and the haemoglobin of blood.

Turning to the mechanism of carbon assimilation, the formaldehyde hypothesis was originated by Butlerow (1861) who, by the action of alkali on trioxymethylene—a condensation product of formaldehyde—obtained

a sugar-like substance. Baeyer (1870) seized upon this fact, and thus was born the well-known hypothesis which is still the theme of so many investigations on carbon assimilation.

Respiration.—Before 1831 certain fundamental facts of respiration were known: Ingen Housz had shown that the plant absorbed oxygen and evolved carbon dioxide and de Saussure had discovered that a continuous supply of oxygen was necessary for the life of the ordinary plant, that the more vigorous the organ the greater was the amount of oxygen consumed, and that this is associated with a rise in temperature. Dutrochet (1837) confirmed the work of de Saussure and showed that responses to stimulation are not made in the absence of oxygen. He understood, as far as was possible at that time, the diffusion of gas into and out from the leaf and he appreciated the intercellular space system as a physiological structure. Further, he distinguished between the evolution of oxygen in carbon assimilation and the evolution of carbon dioxide in respiration which facts were confused by many subsequent workers; and although Garreau (1851) continued and amplified Dutrochet's observations especially on the intensity of the respiration of germinating seeds and buds, there was much confusion of thought until Sachs cleared up the situation in 1865, especially the significance of oxygen and carbon dioxide in carbon assimilation and respiration. After this, progress for a time was slow, but interest was at last re-awakened; Detmer published his work on the physiology of germination in 1880 and in this year appeared the first edition of Pfeffer's Physiology of Plants. A few years later saw the first results of the work of Bonnier and Mangin on the respiratory quotient (1884) and of further investigations by Pfeffer.

The common hypothesis that in respiration there are associated a fermentative and an oxidative phase calls Pasteur to mind. He was the first to distinguish between ærobic and anærobic plants, and he contributed most to the scientific foundation of that ancient practice, alcoholic fermentation.

Irritability.—Although Knight published his observations on the reactions of shoots and roots to the stimuli of gravity and centrifugal force in 1806, progress in this branch of botany was sporadic and slow. Dutrochet (1837) considered that the intensity of light was all important in heliotropic movement whilst Payer and others maintained that the quality of light was the significant factor. In 1865 Darwin's 'Movements and Habits of Climbing Plants' appeared. Its subsequent issue in book form (1875) roughly coincided with the publication of several important contributions on the subject: de Vries on twining plants and tendrils, Pfeffer on autogenic movements and especially Sachs on the various tropistic movements. Sachs' invention of the klinostat was a notable event and rendered possible an analysis of geo- and heliotropism. Mention also may be made of Darwin's 'Power of Movement in Plants' (1880), which was condemned in no uncertain fashion by Sachs, in wh s developed the thesis that circumnutation is the basis from which ætiogenic movements have been derived.

The reasons for our deficiencies in this second epoch are patent. In the first place, the classical traditions of our old universities dominated

teaching, with the result that the approach to biology was, in the main, through medicine; botany was, indeed, of the faculty of medicine rather than of science—Strasburger's text-book still retains a vestige of the herbal in its coloured pictures of medicinal plants. Robert Brown, William Griffith, Williamson, Henfrey, Hutton Balfour, Dickson, Trimen, Sir Joseph Hooker, and these are not the complete tale, were all qualified medical men; Harvey also was a Doctor of Medicine, but his degree was *honoris causa* to enable him to qualify for the Chair of Botany in Dublin University, to which, incidentally, he was not appointed. Other reasons lie in the facts that the period was one of exploration, expansion and development of the Empire, which meant an inflow of great numbers of plants for identification, and the labourers were few.

Finally, the systematists did not entirely favour the new movement and, if not actively antagonistic, they looked upon it with amused tolerance. Even Sir Joseph Hooker, who thus wrote to Asa Gray in 1886, 'I . . . have thrown aside all idea of making headway with—any desire to keep up with even—heads of Chemico-botany, and Micro-phytology. I may content myself with a casual grin at young men calling themselves botanists, who know nothing of plants, but the 'innards' of a score or so. The pendulum will swing round, or rather back, one day.' The prophecy was partly right; the pendulum swung round, not back; taxonomy was eventually enlivened by the work of young men who knew the 'innards' of plants. But there is another side; Sir Joseph was consulted by Huxley regarding the teaching of biology, with the result that he modified his course at the Royal College of Science. Indeed, it would appear that with the passing of time, Sir Joseph Hooker appreciated the significance of the New Botany as is indicated in a letter to Francis Darwin:

> 'I am glad you are going to teach the Medicos a little practical Botany. It is lamentable to find that all this botanical teaching of the greatest Universities in England and Scotland does not turn out a single man who can turn his botanical knowledge to any use whatever to his fellow creatures. Where should we be if Medicine, Law, or any other pursuit were taught after that fashion.'

This was written in 1894!

Before leaving this aspect of our subject, I wish to correct any misapprehension which my words may have given origin: the pursuit of systematic botany is not only important, it is absolutely essential; the exact identification of a plant, sometimes to a variety or to a form, is the first step not only in morphological but also in some physiological investigations. I have, for instance, in mind two physiological papers containing the results of much skilful work: the first is valueless because the plant was wrongly determined, and the worth of the second is much impaired owing to the absence of a specific identification.

The Third Period: the Renaissance of Botany in Britain.

The botanical renaissance in this country began in about 1875 and it was here in this foundation, the Royal College of Science, that it happened. The credit for it is due to Thiselton-Dyer who, as Huxley's demonstrator, instituted and conducted the first laboratory classes in botany. Harry Marshall Ward was the first product.

University College, London, immediately followed, which is hardly surprising since the Professor, Daniel Oliver, as Keeper of the Herbarium in the Royal Gardens at Kew, was in close touch with Thiselton-Dyer who became Assistant Director to Sir Joseph Hooker. At Cambridge was Sydney Howard Vines.

University College, London, honours Frederick Orpen Bower and Dukinfield Henry Scott who there first taught and in turn migrated to the Royal College of Science and from thence Bower to Glasgow and Scott to the Jodrell Laboratory at Kew. Thiselton-Dyer sowed the seed and tended the seedling: Bower and Scott played the most prominent part in bringing the seedling to fruition: British morphology was raised to its peak; a most illustrious school of phyletic anatomy was founded. The advance also was accelerated by the translation of the great German textbooks, and the University of London, not then a teaching university, gave powerful aid by the high standard of its examinations and its insistence on practical tests.

This activity was marked by the appearance of a new journal, the 'Annals of Botany,' in 1887. Hitherto original work was published for the most part in the Proceedings and Transactions of the learned societies and in the 'Quarterly Journal of Microscopical Science,' but the output of botanical work so increased that the 'Annals' came into being and enfolded the shorter works of the new school.

The condition of botany at the beginning of this epoch will be realised by the reading of Sachs' text-books. In all branches of botany there were masses of well ascertained facts, the principles appeared to be settled and the problems formulated. Some aspects, such as anatomy, were in an advanced stage and were developing in various directions, such as taxonomic anatomy under the guidance of Radlkofer and physiological anatomy in which Haberlandt was the pioneer; other aspects, physiology, for example, were relatively backward. In the groups there were many gaps to be filled, more especially in the completion of our knowledge of life-histories and in the details of development. Much of this work was impossible of accomplishment without the appropriate technique and had, perforce, to await the arrival of the continuous ribbon microtome, which botanists were slow to adopt, and the application of precise chemical and physical methods.

Morphology.—In morphology great advances were made and the trend was twofold. For some years in this country the connecting thread of many investigations was phylogeny, but on the Continent, especially in Germany, causal morphology was more dominant in which development Goebel, impatient of phylogeny, played a pre-eminent part. His work and his teaching on the influence of the environment on the configuration of plants cannot be too highly appraised, he with Wiesner, Haberlandt, Stahl, Bonnier and other pioneers stripped morphology of its formalism and revealed the plasticity of the plant.

Of the lower groups I shall say but little since they have been the subject of recent authoritative addresses; one name, however, leaps to mind, that of Klebs, whose work on the conditions governing the reproduction of the green Algæ is classical; W. and G. S. West, father and son, also may be mentioned, for they did much to further our knowledge of the

green Algæ, a fact more fully recognised on the Continent than here in England. In mycology, Marshall Ward, in his investigations on *Hemileia mycoidea*, and on the digestive action of the hyphæ of parasitic fungi laid the foundation of the modern British school. Of a later date Blakeslee's discovery of heterothallism was of the greatest importance and led to far-reaching results. Passing on to the vascular cryptogams, Bower, after a few casts here and there, settled down to the study of development and produced a memorable series of memoirs on the spore-producing members, and on the phylogeny of the Filicales. Other notable contributors were Belajeff, Campbell, Kny, Le Clere du Sablon, Bertrand and Lang.

A great contribution to our knowledge during this period was given by the palæontologists. For some time it has been suspected that certain fossil plants were intermediate between the ferns and the gymnosperms. Stur and Williamson recognised the occurrence of fern and gymnosperm characters in certain fossil plants, and with the increase of evidence a separate phylum was recognised and named Cycadofilices by Potonié in 1899. Later, in 1906, Oliver and Scott made known their observations on the seed-like structures attached to the fronds of *Lyginodendron*. Further work revealed that *Lagenostoma*, *Trigonocarpum*, *Physostoma* and other fern-like plants were reproduced by seeds of a cycadean type. This discovery of the Pteridosperms is the brightest jewel of palæontological study. Passing on to the gymnosperms the work of Strasburger and others was continued and many gaps in our knowledge, especially of the gametophytes and embryology, were filled; and in this Arnoldi, Strasburger, Lawson, Thomson, Coulter, Chamberlain, Coker, Webber, and other botanists of the American School took a prominent part. But of all these investigations, the most sensational was the discovery in 1896 of the motile sperms in *Ginkgo* and *Cycas revoluta* by Hirasé and Ikeno, respectively. Mention also must be made of Wieland's important work on *Cycadeoidea* and other fossil cycads, the stimulus of which was immediately felt.

Like investigations were pursued in the angiosperms, in which many of the same botanists were active together with Treub, Guignard, Campbell, Duncan Johnson, Benson and others. The discovery of chalazogamy by Treub in 1891, and of double fertilisation in *Lilium* and *Fritillaria* by Nawaschin in 1898 provided two thrills.

Cytology.—This progress was accompanied by that of detailed cytology which immediately reacted to the conception of Strasburger and Weismann that the nucleus is the bearer of heridital qualities (1884). The reduction in the number of chromosomes was discovered in the parasitic worm *Ascaris* by van Beneden in 1883; the next year Strasburger observed it in the angiosperms and this was followed by similar observations by Overton and by Farmer in other groups. Thus was initiated a period of intense cytological study, as the results of which were the revelation of the main cytological facts and the nuclear correlations in the sequencies of life histories of the main groups of plants and of aberrant species. Interest gradually faded, and it was not until after the rediscovery of Mendel's work and the consequent impetus given to plant breeding and genetics, that cytology again came to the fore. One further remark has to be made: the cytological investigation of malignant growths by

Farmer, Moore and Walker influenced greatly histological study in human pathology.

Anatomy.—The condition of anatomy in 1882 is shown by De Bary's Comparative Anatomy of the Phanerogams and Ferns: the fundamental facts were well ascertained and the application of those facts determined the direction of increasing knowledge. In Germany the trend, initiated by Radlkofer, was taxonomic in which Solereder took the leading part; in Austria, Haberlandt developed physiological anatomy, whilst in France the drifts were various. The study of purely descriptive anatomy was continued by Hovelacque, for example, on the Bignoniaceæ and other families; Sauvageau, Costantin and others studied structure in relation to environment, especially aquatic conditions; Bertrand essayed a mathematical expression of vascular organisation, and van Tieghem was the father of stelar theories. In Britain the main morphological interest was phylogeny and this gave the bias to anatomical study. Its principles were first applied by Williamson and Scott in their studies of the fossil plants of the coal measures and then by Scott in his continued palæontological investigations.

In 1883 van Tieghem and Duliot published their theory of the stele which was based on the morphological status of the endodermis. Their deductive conclusions were not checked by ontogenetic studies with the result that their theories were discredited soon after the intensive investigations on vascular anatomy were begun in this country. But although van Tieghem's ideas were wrong, to him is due the credit of seeing the problem and essaying its elucidation. In remembering those who contributed most to this aspect of vascular anatomy, Gwynne-Vaughan comes first, not only on account of his sustained work, but also for the fact that he it was who recognised and demonstrated the importance of the leaf trace. Amongst other notable workers were Boodle, Jeffrey, Tansley, Brebner, Schoute, Lang and Farmer. The reading of Tansley's Filicinean Vascular Anatomy will give some idea of the results achieved.

During this period, the phyletic relationship between the monocotyledons and dicotyledons was keenly discussed; Campbell, from the organisation of the female gametophytes, together with certain facts of structure and floral organisation, found the connection between the Piperaceæ and Araceæ, whilst Sargant (1903) from the evidence of seedling structure made the connection between the Ranunculaceæ and Liliaceæ. Van Tieghem (1870) was one of the first to investigate the transition phenomena in seedlings, and he systematised the various types; to our knowledge of the pure anatomy of the subject many others contributed, amongst whom may be named Strasburger, de Bary, Gérard, Chauveaud, Dangeard, Clements, and Dorety. The application of the facts of the transition phenomena to phyletic problems led to extensive investigations carried out by Tansley, Thomas, Lee, Compton, Shaw, Holden, de Fraine and others.

Genetics.—In the year 1900 there happened a remarkable thing: the independent discovery of Mendel and the confirmation of his work by De Vries, Correns and Tschermak.

Mendel published the results of his hybridisation experiments on the pea in 1866 and on *Hieracium* four years later. His work was known to

his friend Nägeli with whom he corresponded : the amazing fact is that neither Nägeli—Nägeli of all men—nor Focke, who was himself an investigator of hybrids and knew of Mendel's work, nor anyone else showed any interest and, therefore, could not have realised its great significance.

The circumstances attending Mendel's introduction into botanical circles could not have been better. Correns was busy with his hybridisations, and de Vries was working out his theory which was published in 1901. So it happened that Mendelism and the Mutation Theory were half brothers. The effect was immediate ; for the first time genetics had a definite law, relatively easy of application, which clarified many problems and disclosed others. It is doubtful if any other botanical work caused such an outburst of activity over so wide a field : a period of intense plant breeding set in ; the transmission of colour factors stimulated the investigation of the flavones and other petal pigments and also the distribution of oxidases ; and a new field of vast area was opened to the cytologist.

Œcology.—This third epoch is remarkable in that it saw the growth of that consort of facts and concepts which are included in the word œcology. The first œcologist was that extraordinary man, Humboldt, who was the first to realise the units of vegetation and to essay their ordering (1805). His system, however, was unsound ; it could hardly be otherwise for the available knowledge was inadequate. Many years elapsed before interest in the subject was resuscitated by Grisebach in 1872, the chief merit of whose work was the stimulation of others. Warming published his first classification of plant forms in 1884 which soon was followed by that of Drude, who had already contributed much. Œcological study now became very active : in France, Bonnier was at work on the influence of edaphic factors on plant form and Flahault on vegetation surveys. In Belgium, Massart was studying littoral vegetation (1893), and in Switzerland there was Schröter. Karston and Schimper were on their travels, Karston in the Malay Archipelago and Schimper in the West Indies. A few years later saw the entry of the American vanguard—Clements, Cowles, Harshberger, Livingstone and Pound, and in New Zealand Cockayne followed his lonely bent (1901). The study of œcology thus was international but, characteristically, we were late. Robert Smith was the pioneer in this country : in 1898 he published his observations on the plant associations of the Tay Basin, and this was succeeded by various other communications. He was accompanied and followed by W. G. Smith, his brother, Moss, Rankin, Lewis, Tansley, Yapp, Oliver and others. The British Vegetation Committee came into being and was the origin of the British Œcological Society. Compared with other countries the volume of British work does not appear great, but many considerable surveys have not been published owing to the great expense of reproducing detailed maps and for other reasons.

In considering this aspect of botany, two facts emerge : the almost immediate popularity of the new orientation, and the rapid accumulation of information which resulted in the founding of new journals. The change from the laboratory to the open air, where problems other than botanical had to be solved, sharpened men's wits, with the result that

the conceptions of œcology correlated a mass of isolated facts and made possible a logical presentation of the subject within twenty-three years of the publication of Grisebach's 'Vegetation of the Earth in relation to Climate'; Warming's Œcology of Plants first appeared in 1895, and Schimper's Plant Geography followed three years later.

The direct and great value of the œcological study of plants has been the recall of botanists to the field and sending them back from whence they came with fresh problems for investigation; in addition, it has stimulated the study of taxonomy, causal morphology, and physiology. Laboratory work was one of the reasons for the decline, at any rate, in this country, of the study of taxonomy in that it attracted men who otherwise might have been pure systematists. In physiology much recent work is a direct outcome of œcology, the considerable investigations on the water economy of the plant is but one example.

Physiology.—At the beginning of this period the problem of the ascent of sap was still moot. Sachs persisted in his imbibition theory, notwithstanding the many facts ranged against it. Strasburger's experiments pushed home the idea that the phenomenon was entirely physical, a view which was adopted by Askenasy, and Dixon and Joly (1895) who realised the significance of the tensile strength of water and on it founded their well-known explanation. Concurrently, the splendid lead given by Pfeffer and de Vries was vigorously followed and the problems of permeability, osmotic pressure, and, in general, the water relations of the cell were the subject of hundreds of papers. Amongst the scores of workers engaged, these were prominent: S. C. Brooks, G. Clowes, Czapek, Dixon and Atkins, Girard, Lepeschkin, Loeb, Osterhout, Ostwald, Ruhland, Stiles, Ursprung and Blum, and MacDougal.

It is impossible here to attempt a survey of the progress of this wide and difficult branch of physiology. The mechanism of permeability and the physico-chemical problems involved are still matters for discussion, but, on the other hand, the considerable forces available in osmotic pressure, the plasticity of the living cell in the adjustment of its osmotic machinery and the nature and importance of turgidity have given us a reasonably clear understanding of the uptake of water and its movements in the tissues.

Mention also must be made of the progress of knowledge of the gaseous diffusion into and out from the ordinary leaf. This had been a matter of dispute and some thought that the main path was through the intact walls of the leaf epidermis (cuticular diffusion) rather than through the stomates. The varying experimental results obtained by different workers were due, in the main, to faulty technique. Stahl (1894) demonstrated that if the stomates are effectively blocked, no carbon assimilation will take place. His results were confirmed by F. F. Blackman, who at this time (1895) was beginning his well-known investigations on carbon assimilation and respiration. Blackman showed that the evolution of carbon dioxide during respiration and its intake during carbon assimilation was, in general, proportional to the number of stomates on the leaf surfaces. These results were confirmed by Brown and Escombe (1905) who had been investigating the subject for some years. Their classic work on the static diffusion of gases was published in 1900, the kernel of

which, as is well known, is that the rate of diffusion through the pore of a stomate is governed by the law of diameters.

Turning to metabolism, this third period is remarkable for the clearer focussing of the circumstances attending metabolic activity and especially of the action and interaction of those factors which influence the rate of particular activities. Before 1905, these factors were considered separately and various optima were given for various functions. The insufficiency of this was soon appreciated by F. F. Blackman, and his application of Liebig's law of the minimum to physiological processes led to his conception of limiting factors, the immediate result of which was a clearer understanding of the activity of the green plant and of the factors which determine it and led to a stricter quantitative procedure in plant physiology. Following this conception, there was a renewed activity in the investigation of metabolic processes especially carbon assimilation and much discussion centred around the shape of the inflexion of the curve representing such activities. The truth of the doctrine is generally admitted, but the observations of Boysen-Jensen, Harder, Lundegardt, Warburg and others indicate that a particular factor is only strictly limiting when it is very much weaker than the others, and then the inflexion of the curve will be abrupt. When, however, the intensity of two factors are about equal and are limiting, the curve will be logarithmic.

It has been mentioned that Hoppe-Seyler established a chemical relationship between chlorophyll and hæmoglobin. His work was carried much further by Marchlewski, Nencki and Zaleski (1896). Schunck and Marchlewski (1899), by their use of alkali and acid on chlorophyll, isolated a number of decomposition products such as alkachlorophyll, phylloxanthin, phyllocyanin, phyllotaonin and phylloporphyrin. Although these substances were not pure and although their work did not throw much light on the changes effected on the chlorophyll by their treatment, it paved the way for the many investigations of Willstätter. Willstätter and his fellow workers by improved methods of experiment, especially fractional separation, obtained a number of products the phytochlorins and the phytorhodins. The fact was established that magnesium is an essential constituent of chlorophyll and that the pigment is an ester of the alcohol phytol. These facts led to the great discovery of the existence of two chlorophylls, their composition, and, later, of the chemical composition of the associated pigments carotin and xanthophyll. A few years after, Willstätter and his collaborators completely elucidated the composition of many anthocyanins and crowned the earlier work of Overton, Molisch, Grafe, and others. Here, again, this was made possible by Willstätter's genius for refinement of technique which reached its apex in his recent, 1926, work on enzymes.

Of the sequence of events in the elaboration of food stuffs, but little real progress was made, notwithstanding a plethora of theories and an abundance of test tube observations. The work of Brown and Morris (1893) on the chemistry and physiology of foliage leaves, in which they recognised sucrose as the sugar first formed in carbon assimilation, is outstanding and was the first of many investigations amongst which the work of Parkin, Davis, Daish and Sawyer, Gast, Kylin and Weevers may be mentioned.

The work of the Darwins, Sachs and Pfeffer on irritability was continued throughout this period and, indeed, to the present day. The use of Fitting's intermittent klinostat made possible a quantitative analysis of reactions, especially of geotropism and phototropism, and there emerged clear conceptions of presentation, excitation, reaction and relaxation times. Concurrently, the search for the seats of perception went on; the problem was the subject of much experimentation and considerable discussion, ultimately general agreement—our present opinions—was reached.

The natural sequence of this tropistic work was an enquiry into the mechanism of the sense organ. The radial pressure theory of geotropism was mooted and not denied, but it was hardly favoured possibly because it is intractable to experimental proof. The statolith theory, due to Haberlandt and Němec, proved more attractive and, after many observations and much discussion, was, in general, accepted. In addition to those mentioned, Noll, Czapek, Jost, Vöchting, Oltmanns, de Vries and Rothert contributed much to the elucidation of the problems. There remained the question of the transmission of the stimulus from the perceptive organ to the motive region. Boysen-Jensen (1910) repeated some experiments by Rothert on the transmission of stimuli when the veins in the coleoptile of *Avena* were severed. Boysen-Jensen obtained some discordant results which, on analysis, showed that a stimulus could pass through a water gap but not through a thin plate of mica. Thus arose the idea that definite bodies, hormones, were generated in the perceptive region by the action of the external agents and travelled to the motive organs to activate the visible reaction. These observations were confirmed by Purdie and others and the usual 'gold rush' happened. Boysen-Jensen continued his work, and amongst those who contributed were Nielson, Paàl, Stark, Dreschel, Snow, and especially Went. Mention also must be made of the work of Ricca (1916) on the transmission of the hormone in *Mimosa*; his work was carried forward by Snow and Ball who showed that there is a high speed as well as a low speed conduction. Details of this and cognate investigations are omitted; they are the theme of our current lectures. It remains to be remarked that that brilliant man, Errera, was the first to postulate the action of hormones in regulating the growth and development in the plant.

The Present.

The period beginning in 1919 is an history of our own times, and need not long detain us; indeed, the progress in some branches has already been alluded to. The rate of advance in morphology and anatomy is perforce slower for the obvious reason that with continued activity, new material becomes less and opportunities fewer. Kidston and Lang's discovery of and work on *Rhynia*, *Hornea* and *Asteroxylon* was a noteworthy advance and in anatomy the investigations on the cambium, especially those of Bailey, are of particular merit. The period is chiefly remarkable for the great output of work in those branches of botany which have an applied aspect. Thus mycology, plant pathology, genetics and cytology occupy a prominent position. In physiology but little progress has been made in the elucidation of the serial events of the various

metabolic changes; so many are indiscernable, that a scientific inference is impossible; indeed, in a recent paper, Lubimenko remarks that our knowledge of the mechanism of carbon assimilation is as obscure as in the days of Ingen Housz! But the closer examination of the governing factors, the conditions of growth and the more detailed analyses of these activities have secured our knowledge, inadequate though it be, more firmly and also have disclosed certain relationships of first-rate importance, hitherto unsuspected—photo-periodism, in which Garner and Allard were the pioneers; the carbohydrate nitrogen ratio, to our knowledge of which Kraus and Kraybill have contributed much; and the principle of predetermination so clearly demonstrated by Ball. The application of the principle of the hydrogen ion concentration has given an instrument of great precision capable of use in the investigation of a wide range of problems. The cell wall constituents have been the subject of intensive investigations, some dictated by the requirements of industry, and important discoveries and elucidations have been made, but this chemical side of physiology, highly important though it be, is rather too specialised for the present occasion.

A survey of this period shows two well-defined tendencies: specialisation and the obliteration of the artificial margins between botany and cognate studies.

Specialisation, although regrettable, is inevitable: it is a duty incumbent on all those who can exercise control to co-ordinate specialisations lest Botany should degenerate into a series of narrow compartments.

The study of physiology in particular and of many aspects of œcology require the application of the technique and conceptions of physics and especially of chemistry: this will increase in the future; it illustrates a saying of Bayliss, 'There are no separate subjects in science: there is but one science and that is not a subject but a method.' If the approach to such problems be made from the biological point of view, progress will be more assured.

A consequence of specialisation is the often unnecessary multiplication of journals. In my introduction, the importance of the printing press in aiding investigation was indicated. Nowadays, contact with, and knowledge of the work of our fellow botanists through the press is becoming less and less, for very few libraries can afford to subscribe, bind and give shelf room to so many periodicals, and when they can so do, no one has time to read them. The help of the printing press is thus a diminishing quantity. A journal once started is difficult to end; there is but one immediate solution, a much wider distribution of separate copies and for this I do appeal.

Here I end this slight sketch of the progress of botany during the last hundred years, and pass on to an introduction to present and future needs and policy.

In 1914 came the war and we were not entirely prepared, with the result that there was a serious waste and misapplication of specialised knowledge. Our governors and masters had failed to realise that the sustenance and development and occasional co-ordination of science is a prime factor in the governance of the modern state and that its lack will be revealed in times of national stress. Nothing had been learnt from past

history. Here is the peroration of an address given by Norman Lockyer in 1898: 'The French École Normale was the result of a revolution, I may now add that France since Sedan has been doing, and in a tremendous fashion, what . . . Prussia did after Jena. Let us not wait for disastrous defeats, either on the field of battle or of industry, to develop to the utmost our scientific establishments and so take our proper and complete place among the nations.' The stress of the war soon forced lessons home, and amongst other things learnt was the dependence of man on the plant. The experience of the war was, curiously enough, not forgotten when peace came, with the result that the training of young botanists for economic work and for the investigation of definite problems became with time a settled policy. But other nations also have learnt the lesson, low living ever has been an incentive to high thinking, with the result of gross over-production—some prefer to call it under-consumption—of essential commodities such as wheat, sugar and rubber. This is mostly due to the unconsidered use of the great advances of applied botanical knowledge and of agricultural engineering without regard to economic consequences. The ultimate remedy may be that taken by the people of Erewhon, but this for the nonce is not practical politics: the present malady must be attacked by more research, for it is in times of economic depression that research is most essential. The present tendency in States and industries to curtail expenditure on research and expert knowledge is a wrong policy: 'They that be whole need not a physician, but they that are sick.'

Turning to other requisite crops, I am told by an expert friend that a world famine of soft timber may be expected in about forty years unless afforestation is established on a large scale, and I need not remind this audience that such work requires the closest co-operation between the forester and the botanist. Some afforesting is taking place in this country, but at the present rate of consumption we can never supply our own needs; search must, therefore, be made for quick-growing exotic trees; if it could be acclimatised, *Eucalyptus globosus* would appear to be one such, for de Baufre has estimated an annual production of 355 cubic feet of timber by a 20-year old tree. Substitutes for timber also must be sought. British railway companies are experimenting with steel sleepers, but the paper manufacturers, who consume many square miles of forest annually in the making of 'news print,' have not, as far as I can discover, moved. Wheaten straw, of which thousands of tons are wasted annually in the great wheat producing countries, would appear to be a source worthy of investigation.

Of other developments the cultivation of cotton in various regions of Africa and of fruit in South Africa, Australia, and Canada are to be mentioned. Then there is the banana industry and also the production of vegetable oils which may become of far greater importance than at present. The successful conduct of these and many like activities, is impossible without expert advice. In this country this is provided first and foremost by that institution of typical English origin, the Rothamsted Experimental Station; the schools of agriculture, horticulture and forestry of our Universities; the cold storage research laboratories at Cambridge and South Kensington; the timber research station at Princes Risboro';

the seed testing station at Aberystwyth; and the stations at Long Ashton and East Malling. This incomplete recital shows no dearth of institutions and their development is all to the good. But there are a few dangers. The founders of new research institutions naturally expect results and may be unmindful of the fact that results cannot be commanded. This may react on the investigators who may be tempted to justify themselves by the dissemination of unripe fruit. Again, the promotion of applied research may tend to obscure the value of pure research. In this congregation of botanists there is no need to stress the adjectives, but there are some who are not entirely familiar with science and, therefore, do not always appreciate the potential value of a pure fact. They ask the question: 'What's the good of it?' which was countered by Faraday by the question: 'What's the good of a baby?' May I, in illustration, indicate the life histories of two babies?

The German chemist, Marggraf, discovered in 1747 that the white beet contained cane sugar.

Some fifty years later, his pupil, Achard, cultivated the beet for the sake of its sugar. Thus on a small scale began the beet sugar industry in 1801. In the last phase of the Napoleonic wars, France was cut off from her colonies by the British blockade and thus was caused a sugar famine in France. Napoleon placed 70,000 acres of land under beet and founded schools for instruction in the methods of extracting and preparing the sugar.

Many years ago Pasteur observed that glycerol was one of the products of the fermentation of sugar by yeast.

During the Great War the Central Powers experienced a serious shortage of fats and thus of glycerol which is necessary for the making of dynamite. The simple fact was remembered and re-investigated by Connstein and Lüdecke with the result that by the addition of sodium sulphite to the fermenting liquor the yield of glycerol was increased to 20 per cent. of the sugar used.

Lastly, there is the comparative isolation of the workers from their academic brothers so that both lose a source of stimulation and ideas. These possible dangers will, for the most part, be eliminated if there be some connection, the closer the better, between the botanical departments of Universities and the research Institutions, accompanied by an occasional chiasmatypy of the workers in both.

In the immediate past the supply of adequately trained men would appear to have fallen short of the demand: thus, a plant disease has been ascribed to an animal parasite which, on further investigation, proved to be nothing more than the elongated nucleus of a tissue element of the phlœm; the identification of certain drug-plants has been based on the anatomical characters of the leaves which characters vary in the individual plant and in plants grown in varying conditions of humidity; and, finally, here is a quotation: 'Trees are usually classified into two groups: (*a*) exogenous, those growing from a central sap supply, and (*b*) endogenous, those in which the sap flow is external. . . . The process of growth of a tree involves the formation of an annual layer of sap-wood immediately beneath the bark during the descending passage of the sap. . . . Radiating from the centre pith towards the bark are the 'medullary rays' which

form the channels of transfer of fluids absorbed by the root system.' This is not from an ancient author, but from a paper on Timber published in a technical journal in 1930.

The British Commonwealth of Nations is, in the main, an agricultural Empire: the great need for trained botanists for its administrative and technical service is patent; the problem is their supply and their training.

The increased demand is slowly having its effect in the Universities, and more students are taking botany for their finals, but their numbers are too few: no one wants an undue specialisation in the schools, but I would point out that the fundamental problems of the world are biological problems, for which reason I do most strongly urge that every encouragement be given to those who show a biological trend of mind to follow their bent especially if they be of the right type, for much of the work to be done requires qualities in addition to botanical equipment. In the University the training of young botanists for the first three years must be in pure science, this is absolutely essential, and should be followed by a period of appropriate specialised training. This raises the problem of the manning of our botanical departments: Universities cannot compete with industry in matters of salary, and this in the past has resulted in good men being lost to academic work, especially in plant physiology. If good material for the service of the State and of industry is to be provided and pure research maintained, an adequate flow of recruits of the highest quality into academic life is essential. For this we must look for those who want to study the plant for its own sake without regard to reward, monetary or otherwise: the work must be its own reward, and to these young botanists, if they be of the right stuff, can be promised a moderate recompense, good fellowship and a happy life in so far as in them lies. All this has in essence been said before, and it will be said again, for it is only by reiteration that needs in the end will be met.

EDUCATIONAL DEVELOPMENT, 1831-1931:

A CENTENARY SURVEY AND A FORECAST.

ADDRESS BY

SIR CHARLES GRANT ROBERTSON, C.V.O., M.A., LL.D.,

PRESIDENT OF THE SECTION.

The honour of presiding over the Education Section at this Centenary Meeting of the British Association is no mean one, and I gladly acknowledge the pleasure that the invitation to be your President has given me, even though it inevitably carries with it the duty of inflicting on my audience a Presidential address. My life as far back as I can remember has been concerned with one form of education or another, although I have successfully avoided on the whole the dubious duty of adding to the burden of printed books on the theory or practice of a subject, which has been more than once described as more dismal and arid than even the dismal science of Political Economy, and more vulnerable even than Theology to the charge that, in all such contributions to theory or practice from Zoroaster to, shall we say, Mr. Bertrand Russell, whatever is true is not new and whatever is new is flagitiously and demonstrably untrue. The world, as far as I can make out, has always teemed with educational reformers. The continuous puzzle for the historian has been to find out where the Reforms of the Reformers have gone to, or in what the Reforms actually achieved have precisely consisted. At this moment in particular, standing alone in the tumbril which we politely call the Presidential Chair, I am acutely conscious that as a small boy I was part of an historic experiment in educational reform when I was sent to the school of R. H. Quick in Orme Square, Bayswater. Quick deservedly has, by a notable book on 'Educational Reformers,' a secure niche in the history and practice of education; but neither at the time nor since have I ever been conscious that under Quick I was learning new things in a new way. And the only convincing reason that I can give for this deplorable failure on my part is that if, in that misty past, I had been subjected to a Binet Test I should have been at once shown up as, for my age-level, much below the normal metrical scale of intelligence.

In selecting the subject of my Presidential Address, I was confronted with one obvious difficulty. Presidents of this and other Sections, I understand, usually can press into their addresses the fruits of their own independent and original research. But though for more than thirty-five years I have been an investigator, a teacher, and an administrator, I

have no fruits of research to offer you, unless a prolonged experience and still more prolonged reflection could be dressed up to look like the majestic divinity of Research. I propose, therefore, to offer you some of these considered reflections in the form of a Centenary Survey which on this occasion is, I hope, both pardonable and appropriate.

My title, I agree, may mean little or nothing or much. Permit me, therefore, to explain briefly that I am not going to waste your time with a synthetic, unpalatable but probably familiar summary of the evolution of educational theory, practice, development and administration between 1830 and 1931 ; I am not going to drench you with dates, names and statistics, scientifically labelled and sorted with platitudinous or provocative adjectives dogmatically attached ; least of all have I any desire to gut the large historical text-books and the still larger squadrons of blue books and compress the vivisection into a miniature panorama of an astonishing century of effort and achievement. For astonishing it surely is. The more you study the situation round about 1830 ; the more you know from first-hand study of the sources of all that has happened after 1830, and the more that you know of the situation and tendencies of to-day, the more you will be impressed with the quality, quantity and scope of the work done and the results achieved. It is a commonplace to emphasise in the last hundred years the progress of physical and natural science. That progress has been equalled and in some respects surpassed by what has been done in Education—and, to anticipate one of my conclusions, with the same broad general effect. Just as in physical and natural science, so in Education the most striking result of a century of unflagging and remarkable progress has been the revelation of the extent of our own ignorance and of the difficulties of the fundamental and as yet unsolved problems.

My modest task, therefore, in the limited time at my disposal, is to invite you to accompany me on what the Higher Command of the Army calls a Staff ride. Our ground is a Century ; our object is to make both a strategical and tactical map—with future operations in our mind.

First, we must drive 1931 out of our consciousness, to be suppressed in a subliminal limbo until we are ready by and by to invoke it back for sublimated treatment ; we must see the England of 1820 to 1830 as that generation saw it.

With Castlereagh's death in 1822 began the reform movement in every branch of the nation's life which culminated in the great Reform Act of 1832. The waters which for thirty years had been dammed up in England since the French Revolution began slowly to pour through the dykes, until finally the whole structure of our institutions and habits seem to be submerged. I emphasise in this intellectual and political ferment three general points, the significance of which will be, I hope, more apparent later on ; first, the power of influence of the utilitarian group, the Benthamites, because they provide one more striking example of what can be accomplished by a small body of able men with a definite creed, a definite objective and the high and passionate seriousness that high and passionate purposes alone can inspire ; secondly, the forces and personalities which made the Oxford Movement ; Keble's Sermon on 'National Apostasy' in 1833 which, in Newman's judgment, started the

Oxford Movement proper, only gave expression to a new interpretation of national life that had been fermenting for years, above all, in Oriel College, and Oriel College is for the Oxford Movement what Assisi is for the Franciscan revolution: in a word, the Oxford Movement brought into the arena a new conception of the Spiritual as a formative force in life, and we shall fail to understand the Nineteenth Century problem of religion in Education if we do not remember this: thirdly, I put the organised and widespread campaign against 'ignorance' as an evil and a national vice, which led to the foundation in 1827 of the Society for the Diffusion of Useful Knowledge. There is no more entertaining guide to this period in our history than Peacock's novels, every one of which is a roman à clef. You will recall particularly in *Crotchet Castle* the delicious play that Peacock makes with 'the March of Mind,' that slogan, as we should say to-day, which captured the intelligentsia of England and was the direct creation of the Society for the Diffusion of Useful Knowledge and many other similar organisations. In the irresistible 'March of Mind,' England was to become united, happy, prosperous and free. Is it not the characteristic of all revolutionary movements that they start from an infallible faith in the perfectibility in a short period of the human species and end with a remorseless tyranny over a human species that refuses to perfect itself?

Study the correspondence of the leading figures in the decade from 1822 to 1832, and you invariably find either an extravagant fear that England is stumbling to her doom or a no less extravagant expectation that in a few more years of drastic reform England will have entered the Promised Land. To the one type of mind the question is: What can be saved from the coming cataclysm? To the other type the question is: What is the next Jericho whose walls will fall by the sword of the Lord and of Bentham?

Let us focus our attention on the strictly educational field. The primary sources are particularly rich, for apart from numerous pamphlets we have the Parliamentary Debates, the Quarterly and Edinburgh, Reviews and (after 1824) the Westminster Review, the organ of the Benthamites, and for 1831–1835 the six volumes (and no more) of the Quarterly Journal of Education, supplemented by the letters in many biographies, and many volumes of sermons and pamphlets. Remember, please, that Raikes had started his Sunday Schools as far back as 1780, that Bell's 'Madras System,' Lancaster and Owen's epoch-making school experiments had long been topics for admiration, imitation and ferocious controversy; that Froebel at Keilhau, Pestalozzi at Yverdun had accomplished their work, and that before 1830 Herbart had thought out and published in German the substance of his system. But neither in the educational literature nor in the furious controversies of the time will you find that Froebel, Pestalozzi or Herbart were known or that anyone supposed that in fifty or sixty years these would be spell-binding names in every text-book. The explanation may be, as Pusey maintained of the theology of this period, that only about three persons, of whom he was one, could read German with ease. At any rate, Froebel, Pestalozzi and Herbart, like the early plays of Bernard Shaw and like Mendel in Biology, were to smite England after travelling from Germany to the United

States and thence, somewhat battered rather than bettered in the process, back to our own country. In psychology, at any rate, if any book or system held the field, it was the elder James Mills' 'Analysis of the Phenomena of the Human Mind' first published in 1829—and condemned to-day as a decayed fortress of the superstitions of Faculty Psychology and a discredited associationism.

That everything educational was in a profoundly unsatisfactory state everyone at pains to investigate took for granted. So gloomy is the picture drawn that one is driven to wonder how there were any educated persons at all, and particularly so many able to write such good English or construct such closely thought out and impressive arguments. The two Universities and the only two, Oxford and Cambridge, were targets for savage criticism; the endowed public schools such as Eton, Winchester, Harrow, etc., were held to be expensive nurseries of vice, incompetence, and pedantry; the old Grammar Schools were dilapidated, starved and useless; elementary education simply did not exist. Every established institution was threatened; we were recklessly flinging our doors open to an ignorant, irreligious and irresponsible democracy, without any adequate institutions or administrative machinery to cope with the dangers and needs of a new political nation.

The England that created the British Association concentrated, you will find, its attention on four main educational problems—what was to be done with the Universities? How were the public schools to be reformed? How can this illiterate democracy be cured of its intolerable and dangerous ignorance? Where and how was the money to be found? And immediately, by this concentration, leaped straight into the educational vortex. Of the University aspect of the problem I will say practically nothing now or hereafter, because meeting as we do to-day in London, the foundation of the University of London and its significance naturally deserves separate treatment on this Centenary occasion, and I leave it to the competent hands of Dr. Deller. But nothing illustrates better than the creation of University College and King's College the power of the utilitarian group and the reaction of the established Church of England to the challenge that University College implied.

But go back to that contemporary literature, even if your mind is full of 1931 and its controversies, and at once, if the vocabulary and the environment be very different, you are on familiar—painfully familiar—ground. Here, for example, are three questions always coming up in the literature of the day and on which tons of ink were spent: What is the object of Education? If it is not mere knowledge, what is it? The formation of character or the formation of a Christian character and a Christian citizen, and if not, what then? To whom is this duty to be left—the individual conscience of the parent, the Church as the depository of Christian truth, or the State, with a compulsory power and the right to represent the civil mind?

Let me quote here a couple of sentences from the Quarterly Review of July 1829: 'Does any man believe that to furnish the future weaver or carpenter with the education of a scholar or a man of science will make him more contented in the sphere in which he is thrown? The more fitted he is for a higher station in Society, the greater the effort of mind to

keep him happy in that which fortune has fixed him.' And observe the implications in the quotation. Or, take again, these sentences from Dr. Arnold, written in 1837: 'the whole good that the University (*i.e.* of London) can do towards the cause of general education depends on its holding manifestly a Christian character; if it does not hold this, it seems to me to be at once so mischievous, from giving its sanction to a most mischievous principle, that its evil will far outweigh its good . . . . . I have not the slightest doubt that it is better to go on with our present system, with all its narrowness and deficiency, than to begin a pretended system of national education on any other than a Christian basis.' For in those sentences you have the essential core not only of Arnold's work at Rugby, but of his most passionate convictions on every form of education from the nursery to the universities and the life of the nation as a whole.

The years from 1825 to 1840 were a period of bitter but stimulating controversy; of positive achievement to be registered they may seem to be scanty, if not sterile; but three points of immense importance stand out from the dust that has been laid and the ashes, still treacherously hot. First, as regards religion and its part in the general conception of education as a whole, we note the transformation of an educational problem into a savage political warfare. The privileges and legal status of the established Church of England were challenged by the political and religious disabilities of Nonconformist Dissent. And the political battle was embittered by the controversies within the Church of England itself, which cut deep and wide into the fundamentals of the relations of Church and State. It is profoundly significant that Scotland was at the same time rent by the issues which culminated in the epoch-making Disruption of 1843, and that in Ireland the whole cause of Education was thrown back for at least one generation, if not two, by the storm that centred in what may be conveniently called the 'Maynooth Grant.' In the political welter in all three countries the real educational issue was either submerged or driven on to the surf-smitten rocks.

Secondly, with the first Parliamentary Grant from National funds in 1833, the State stepped into the arena, with the reluctance of a man unable to swim and pushed into cold water out of his depth.

Abstract political theory—and I mean by that the definition of the functions and powers of the State in the sphere of intellect and of morals—at once became linked with the issues of controversial party politics. As early as 1840 it was grasped by all clear thinkers, irrespective of the school of thought to which they belonged, that henceforward the control of Parliament and of the power of the purse could be made, indeed must become, not the sole but the most powerful force in deciding educational issues.

Thirdly, all the issues contained in the term 'curriculum,' were in full and fierce debate from 1825 onwards, the real significance of which has often, I think, been missed, and largely because both attackers and attacked in a singularly copious literature failed to distinguish the real educational issue that had been raised. The champions of what to-day we should call modern studies—history (other than ancient history), modern languages, science, and even of mathematics—as indispensable elements of any sound curriculum—only too frequently urged their case

on the intrinsic merit of the subject as a branch of knowledge, and without any reference to the result in the type of mind or character that a reformed system was to produce. And a dreary study of much dead controversy has left on my mind a depressing impression that conservatives and reformers alike completely forgot that the method and the amount of any subject in a curriculum may be even more important than the subject itself, intrinsically considered. Be that as it may, this period from 1825 to 1840 brought the whole question of curriculum into the disconcerting light of day; and if it is the duty of every generation when it cannot solve a problem to make it impossible for its successors to evade it, the generation of the Reform Act at least did that part of its duty very faithfully.

The next thirty years, ending roughly in Forster's Education Act, were to be not less abundant in controversy but more fruitful in positive and lasting achievement. Superficially regarded, they might be called the age of Royal and Governmental Commissions. The State was, therefore, proclaiming its right, bitterly contested and resented, to inquire into the working of educational institutions--Universities, Endowed Public Schools, Secondary Grammar Schools—over which it had neither administrative nor educational nor financial control. The transition from education as an optional function, to education as a national duty of, the State was being rapidly effected in these thirty years.

But apart from this really momentous evolution of applied political theory, what else are we bound to note, in our Staff ride, as peaks ? Three points in particular. First, then, the slow revolution effected in the Endowed Public Schools. We owe this to four pioneer Headmasters who, in an age of remarkable personalities, stand out pre-eminent. For the acid test and infallible criterion of the pioneer in all spheres of human activity is that after his work has been done, the sphere in which he has done it is qualitatively different. Only too often so decisively does the result pass into the texture of everyday thought and action that we can only judge its originality by a study of the conditions prior to the pioneer. It is thus that we judge the work of a Newton, an Adam Smith, a Niebuhr, a Darwin, or a Pasteur. And it is thus that we can safely regard Arnold at Rugby, 1828–1842, Benjamin Hall Kennedy at Shrewsbury from 1836–1866, Edward Thring at Uppingham, 1853–1887, Haig-Brown, the Second Founder of Charterhouse, 1863–1897 : and if I extend my period considerably, I add a fifth, in Sanderson of Oundle, 1892–1922. These were all men of very different fibre, outlook on life, intellectual power and quality of scholarship. But they had one uniting link and characteristic, they were great teachers because they were great personalities ; they were great organisers because they had the gift of leadership : and they left on their generation, on their staffs who knew them, on their boys, on all indeed with whom they came into contact an ineffaceable stamp of power and inspiration, the combination of individuality and experience, and they created an inexhaustible tradition for the institutions that they remoulded. In the firmament of education there have been, there are, and there will be, many lamps shining with different rays and varying intensity when focussed in the spectrum of spirit and mind, but these five great names—Arnold, Kennedy, Haig-

Brown, Thring and Sanderson—will always swing in our English firmament with a rare and undimmed splendour.

The second point is the advent of science. The nineteenth-century renascence of science may correctly date from 1931 and Faraday's discoveries, the centenary celebration of which has just preceded our Centenary Meeting, but the period from 1840 to 1880 is studded with great names and memorable discoveries. As far back as 1830 even the Quarterly Review was indicting the British neglect of science as compared with the state of things on the Continent; and I would remind you that as late as 1864 at Rugby alone of the older Public Schools and at the newly founded Cheltenham, was any Science taught, and that it was unknown for example, at Eton, Harrow, St. Paul's, Shrewsbury, Charterhouse, Merchant Taylors, and so forth. It was in the late 'sixties that the great educational struggle began seriously, which I term the advent of science, and I mean by that the battle for the principle that an adequate knowledge of physical and natural science is advocated not merely for its importance as knowledge or for its vocational or utilitarian value, but for its cultural indispensability. In other words, an adequate knowledge of, and training in, physical and natural science was proclaimed as an essential element in any education claiming to be liberal. If Huxley unquestionably is the Achilles and protagonist of this twenty years' battle for the capture of the classical Troy, do not let us forget that one of the first trumpets sounded in the fray was in the famous *Essays on a Liberal Education*, published in 1867, in which J. M. Wilson, then a master at Rugby, afterwards Head Master of Clifton (1879–1890) urged the claims of Science. Wilson died this year, aged ninety-six, fighting to the very end for high and noble truths. *Clarum et venerabile Nomen*, indeed, whom in a meeting such as this we can salute with affectionate and grateful homage.

Thirdly, to the renascence of the Public School, the renascence and advent of Science, we can add neither a renascence, nor an advent, but the creation of a new element in the Educational problem—the education of girls and women. The present generation can only reconstruct with difficulty and astonishment the conditions of girls' education in the first half of the nineteenth century. What a German Educator said as early as 1786 was not wholly untrue sixty years later: 'As to the feminine sex, especially that of the better classes, it seems as if the State cared little whether they grew up into human beings or into monkeys.' 'Madam,' wrote a business man in 1858 to the head teacher of one of the better girls' schools, 'as my daughter is not going to be a banker, I see no purpose in her being taught arithmetic.' 'Everything,' said Miss Cobbe of her school in 1836, 'was taught in the inverse ratio of its true importance. At the bottom of the scale were morals and religion, and at the top were music and dancing.' To-day, if we choose, in page after page of the Report of the Taunton Commission of 1864, which but for the pressure of five or six devoted and gifted women would never have illegitimately extended its reference from boys' to girls' schools—we can read the devastating facts and no less devastating comments of the Commissioners. The dawn of the new era coincided with the year of political revolution, 1848, when Queen's College, in direct imitation of King's College, was started in Harley Street to 'hold classes in all branches

of female learning.' Two of the first students in Queen's College were Frances Mary Buss and Dorothea Beale. To-day, when we visit the Frances Mary Buss Schools in North London, and the Ladies' College in Cheltenham, do we realise that we are seeing as great a revolution in education as when we visit Wykeham's foundations at Winchester and Oxford, or stand in the early quadrangle of Merton College at Oxford or of Peterhouse at Cambridge. In Miss Buss, whose creation of the North London Collegiate School came first and who was also the creator of the Head Mistresses' Association, and in Miss Beale, we have two Pioneers who stand and always will stand, in a class by themselves. They are as great and creative revolutionaries as were St. Francis and St. Dominic in the thirteenth century. And yet Frances Mary Buss is omitted from the Dictionary of National Biography—perhaps because omission is a more eloquent testimony to distinction than four columns of obituary epitaph.

To these two I would add one other name, Sarah Emily Davies, who, if any one person founded Girton College, was that one person : and with the foundation of Girton College began the second part of the revolution —the admission of women to a University education.

I mentioned the Forster Education Act of 1870 casually because I have never been able to regard that Act as a revolutionary or a creative measure in the true sense of the term. It was essentially a compromise ; it gathered together into a single statute forces, movements and principles which had been operating for at least twenty years previously, but it expressed no new principle and enshrined neither really new methods nor new and constructive ideals. And its most conspicuous defect was that it made no attempt to correlate the Elementary Education that it reorganised with the existing system of Secondary Education, so as to bring the inevitable development of both into an organic and fruitful union. This is only another way of saying that Forster was not a great Education Minister ; he was a high-minded Statesman of remarkable ability, courage and independence, who showed greater imagination and vision in imperial policy than in education. And it is remarkable that from 1815 to 1916 we have no Education Minister or Statesman of the first rank, no political mind of the first order devoting supreme powers of brain and imagination to Education and leaving it permanently in his debt.

What would have been revolutionary in 1870 would have been the translation into legislation and administration of the principles of the Birmingham group who fought to make primary education 'Secular, compulsory, and free,' and who really compelled the Liberal Government of 1870 to legislate and were given the Act of 1870, which they denounced as a betrayal by the Government and a treacherous surrender by the Minister. If England does not love Coalitions she loves revolutions still less, and least of all, revolutions in education inaugurated by governments. In 1871 England was as unready for primary education, 'Secular, compulsory and free,' as were the United States for Prohibition in 1917.

I am getting near the end of my Staff ride. After 1870 the two most conspicuous new elements (apart from the Act of 1902, of which my old chief, Sir William Anson, was one of the main authors) surely are first, the Adult Education Movement, and the advent of Psychology.

The first great chapter of the recognition and development of extra-mural teaching as a function of the University is contained in the history of University Extension which started in Cambridge in 1872, spread to London in 1876, and to Oxford in 1878. The roots of this new growth go back to the Movement for Mechanics' Institutes which began in 1799, and took a fresh shape in the ideals of Frederick Denison Maurice and the foundation of the Working Men's College. The second great chapter dates from the foundation of Ruskin College at Oxford and the creation of the Workers' Educational Association with which the name of Albert Mansbridge will always be associated. Well can I remember both those events, and, to use the familiar formula of our Oxford University Bidding Prayer, more especially am I bound to mention here that my College of All Souls—that reputed home of reaction and lost causes—was the first in any University to provide not only from its corporate revenues the first extra-mural University tutor, Mr. Tawney, but to allocate its hall in the summer term for his extra-mural classes. What the fusion of the older University Extension Movement with the principles, organisation and ideals of the Workers' Educational Association has become and wrought is known to-day—even in Fleet Street.

The Statesman who said thirty years ago that we were all Socialists now, would assert with more truth that to-day we are all psychologists. We do not indeed talk psychology (as Bourgeois did his prose) without knowing it. On the contrary, we do not know psychology and therefore we all talk it; in fact, it is better for a modern citizen to be guilty of klepto-mania, which I understand is really only a functional parapraxis due to imperfect motivation or an intermittent disassociation affecting a poly-locationary consciousness, than to deny a universal addiction in our friends, but, of course, not ourselves, to complexes and neuroses. The advent of psychology was ushered in by the rediscovery, in the eighties of last century, of Herbart and the application or misapplication of his principles to educational theory and still more to educational practice, to which the foundation of Training Colleges for Teachers and the mania of publishers for small text-books gave an unlimited scope. It was fertilised by the astonishing advance in physiology in which British physiologists have played so memorable a part, and by the tremendous impetus given by Wundt, James and Ward, to name only three of those no longer living, who with their successors and disciples have literally created a new and definable branch of science, requiring a specialised technique.

What material, I must now ask, has our rapid Staff ride provided for a summing-up in the shape of a forecast? Let me summon to my aid my aged friend, Rip Van Winkle, versed in the educational problems that vexed the England of 1830, put to sleep when the British Association came into its cradle, waking up in this year of grace, and taking stock of this world so new to him after a century of blissful oblivion.

'I see,' he tells me, 'a vast and complicated administrative machine, spread like the electric grid over the whole country, and co-ordinated, if at all, by a State Department of Education, as remarkable for its executive comprehensiveness as for its intellectual timidity, which, after all, is the characteristic of all bureaucratic organisations; this new President of the Board of Education, so strange to me, is a member of a Party Cabinet,

with the inevitable political bias that his membership in his party involves, and dependent for the tenure of his office on the fate of his party on purely political issues: the century proves that Education Bills do not destroy Ministries, but neither, however excellent, can they save them; hence, most party Education Ministers with political ambitions have to make their reputations elsewhere than at the Board of Education, because whether they are good or bad ministers of Education, they will only stay in their office if their party stays in power. In 1931, as in 1831, Education can only be kept out of politics by the determination of every party in turn not to allow an educational issue to jeopardise its general political fortunes. One issue, the so-called religious issue, alone threatens this. In 1931 as in 1831 I observe that no Government will tackle it fairly and squarely, because a courageous solution of it would be unlike successful operations in surgery; the operation would succeed, the Nation would recover, and the operating surgeon, the Minister, would die.

'In 1831 we talked and dreamed of a national system of education. You are still talking and dreaming of it, but I begin to suspect that you (as we) were deluded by a catchword, cribbed by misunderstanding France and Germany. For national can have two very distinct interpretations which we jumble up—it is either a comprehensive unitary system, embracing every branch of educational effort controlled by a single national authority, however differentiated and delegated the powers of the sub-authorities and local organisations may be, or a system expressing the national character of the political community in accordance with its traditional principles and attitude to all national problems.

'One feature in this hundred years,' Rip Van Winkle proceeds, 'is amazingly significant—the reform of the old, and the development of the new, Universities, almost wholly endowed by private and voluntary benefactions. They are all literally co-educational. Mrs. Rip Van Winkle is even more astonished than I am, for she was with Miss Becky Sharp at Miss Pinkerton's Academy. But what puzzles and grieves her is not that girls should have their Etons and Winchesters or go to a University and become a Mistress of Arts or a Bachelor of Commerce, but that in a century which has laboured so incessantly to secure rights for married women and which rightly regards motherhood as one of the fundamental bases of what you call sane and sound citizenship, you have, also, laboured so hard to secure that girls and young women should almost universally be taught and controlled by spinsters. You seem to regard marriage in a man as enriching, but in a woman as impoverishing, the experience of life and capacity for service. You put married men at the head of your Universities, Colleges and Schools, because you like the best of them, in the interests of the race, to marry; but you do not put married women in the same position, because apparently you wish to encourage the fools and the fribbles to marry, while the ablest women are subsidised to remain single—of course in the interests of the race.

'To proceed; why when we are still a commercial and industrial nation have you done little or nothing scientifically to educate boys, girls, young men and young women for commerce and industry, particularly when you have done marvels for scientific research and all the professions, law, medicine, engineering, teaching, the religious ministry,

and so forth? As my friend, Adam Smith, once said in another connection, that omission is "altogether unfit for a nation of shopkeepers, but extremely fit for a nation whose education is influenced by shopkeepers."

'Since I woke up' Rip Van Winkle continues, 'I have been studying hard the best books from a vast collection of what I find is called Psychology, and I have run through dozens of smaller books for teachers which profess to boil down—as you call it—scientific research into a practical compendium for the course called Pedagogy, or how to teach every subject to every kind of person, and this Pedagogy based on psychological commandments seems to be compulsory in the training of all teachers in State schools, though why it is not compulsory for other teachers I do not know. The pupils learn the whole of psychology in a year, which shows how clever are your professors of education and the would-be teachers, compared with the professors and would-be teachers of my generation a century ago. This psychology delights me extremely. For it teaches me precisely how I get all my thoughts and feelings and what I do with them when I get them. It is explained with lovely diagrams that I cannot do with the external stimuli anything but what I actually do; and if I seem to do something different from what I ought to do, it is either a conditioned reflex in which I have forgotten to obey the conditions, or it is an unconditioned reflex which means that the conditions were there all the time, or ought to have been there, but that I forgot to give the right, or was rattled into giving the wrong, response—and Nature very properly spanked my Ego. I have, in fact, at last discovered, what used to puzzle me a hundred years ago, that I am what I am because I am not someone else. I learn, too, that I am the sum of all my responses and reflexes, together with those that I repress and tuck away in my subconsciousness, because I do not like them or they do not like me, and which therefore wait either to pounce out on me unawares or pop up just to show me they are there; and then I sublimate them into something which I ought to like much better, and if I do not sublimate them, they sulk and fester into a complex. But while I am simply the sum total, I am also the unifying something that collects, and labels, and separates, so that I am at one and the same time the man who gives you the ticket, the passenger who uses it on the train by which he must travel, and the collector who takes it from the passenger when the journey is done, to prove that the passenger has come by that train and not by an illegitimate motor-nerve bus. But as I am only a beginner at this modern psychology, I have not yet found out whether my mind—if it is my mind and not merely a function of the physical stimuli—was there before I had any stimuli and responses, or whether the stimuli came first from somewhere and created my mind in order that I might recognise that it was a stimulus and give it the right label. What comforts me, however, is to be assured that I have an Ego which is always different from, and yet the same, as all my reflexes and reactions—both those which I have had in the past, am having to-day, and those I am going to have presently.

'Finally,' Rip Van Winkle concludes, 'I observe with immense interest that after a hundred years you are still arguing precisely all the fundamental questions that perplexed us. A friend sent me a book called "Education at the Cross-Roads," by a gentleman who had been a Minister of Education,

and I thought I had by mistake been sent one of the many books of my own time, because we all declared then that Education was at the Cross Roads, and now a century after, I find Education is still at the same Cross Roads. We did not know what was the purpose of Education—whether it was purely utilitarian to enable the educated to earn a better living than the uneducated, or to make them better craftsmen, or to train them for a profession, or merely to become as virtuous and happy as they could make themselves or others would allow them to be. And I find you are arguing just as passionately as we did all these things to-day. In my time someone solved all our difficulties by saying that education was not for any of these things but for life. No one could say, unfortunately, what life was for which we were to be educated, and if he defined it, everyone else said life was something quite different. Everything was altering so rapidly, and there were so many different lives and no one could say which of them was going to be the life of each different boy and girl, that we got very angry with each other and before anything could be decided, I fell asleep. And now that I am awake again, I am terrified at the world round me, not because it is so wonderful and strange with its motor cars, and aeroplanes and wireless and electric light and cooking, and gas and fountain pens and typewriters and telephones and telegraphs and your wonderful newspapers which come out every hour, but because it is changing so rapidly every month and every day. People to-day are such a perplexing compound of the primitive—for you thieve and murder and tell lies and get drunk and run away with each other's wives, just as we did—mixed up with the purely artificial which is the result of all your inventions—which are like a very tight corset all over a savage's body, that I do not know how you can educate for life to-day, because by the time that you have educated the boy and the girl the life will be absolutely altered. And so I end with a concrete question. For what kind of life do you educate a girl, just to be a voter, which she certainly will be if she lives till twenty-one, whether she wants a vote or not, or to be a film actress or a cook or a mother or an old maid, none of which she may ever be ?'

Thus far Rip Van Winkle. Hard questions, if I may venture to say so of my venerable friend, and going to the root of many problems.

Let me conclude by inviting you to concentrate on four points which are bound to find a place in any forecast of the future.

First, the Science of Psychology is obviously only in its infancy—the stage which Chemistry had reached when Dalton formulated his celebrated law which modern chemistry no longer accepts. What can we expect from Psychology judging from what it has already done ? I am not concerned here with the inexhaustible possibilities opening up in the medical, and particularly the pathological, sphere and the field of therapeutic action. No sound educational results, as far as I can see, are going to come from applications from the abnormal to the normal, unless we accept an assumption that it is the abnormal that is really normal, which is like assuming in medicine that disease is the rule and health the exception. I look to much real help coming from Intelligence Tests, however unsatisfactory they may be at present, and in two supremely important directions.

For, if we can once really establish what everyone from Aristotle to

the present Board of Education has assumed, that children and adolescents and adults in varying categories and at different age-levels have definite limits of educability and that it is waste of time, effort and money to treat one category as if it were another, the doctrine of equality of opportunity will come to be regarded as a devastating superstition, and the grading of the categories on the educational ladder will be the beginning of an unparalleled social revolution.

Secondly, human activities, professional or otherwise, will have to be regarded socially and economically in proportion to the degree of trained intelligence and revealed aptitude that they require for their discharge. It is here that social and industrial psychology will find their true field. The difficulty will be the correlation of the industrial or occupational categories with the purely professional in the social organisation, which means that the education, with the aid of psychology, will necessitate a revaluation of social values. Such a revaluation will at once raise the issue of the purely ethical values in the scale qualitatively considered. Through education, we shall decide potential function and then train, in Aristotelian language, potentiality into actuality.

And that raises my second issue—the functional differentia between the sexes. The nineteenth century revolution in the position of women went through two main stages. The first was purely educational. If girls had brains, they justified as good training as the brains of boys—hence, the revolution in Secondary, which reacted on the Primary, education of girls, and in its turn led up to the demand for admission to the Universities. Simultaneously, the demand for careers for the educated girl was a logical consequence. The legal or social obstacles to opening the professions had to be removed: this, in turn, involved political rights: and the intellectual demand for equality in careers was merged with the demand for equality in citizenship and political rights. The movement was consummated with the grant of the vote in 1918 and with the grant of the degree at Oxford and the quasi-degree at Cambridge—the two last University strongholds of male monopoly to surrender. Women now have a virtual equality both in civic and educational status. Until 1921, it was inevitable that in the struggle for this dual equality, differentiation of function should be ignored or rejected. If women were to be able to do exactly what men did, their training must obviously be a copy of that which men had deemed necessary. But since 1921, when equality of opportunity for all careers had been conceded, a slow reaction began. Differentiation and specialisation of function, based on differential sex-qualities, reasserted their directive force—and will reassert it with increasing momentum.

Girls no longer feel it their duty to choose a particular career in order to emphasise a claim to equality of rights or to extirpate traditional social taboos. It must be the privilege of education to stimulate this marked tendency, and thereby to reduce a stupid competition of the sexes and cut down a costly social waste. For the social revolution, through which we are now passing, is slowly learning from the preceding political struggle for the so-called emancipation of women, that in a well ordered society there are no monopolies of civic function or of intellectual or imaginative activity based on sex, but that there are limitations

imposed on all in the form of physical, intellectual and moral qualities and aptitudes, inherent in the individual as such. Whether it be surgery or poetry, acting or nursing, teaching in a kindergarten or research, domestic administration or scavenging, aviation or dressmaking, a trained woman may be the equal of a trained man or she may be a great deal worse than an untrained man. But it is, also, becoming clearer every day that for certain activities the average woman, if trained, is better than the average trained man, and *vice-versa*, and the difference in each case rests on a functional sex differentiation, of the criteria of which we are as yet amazingly ignorant. But until this obscure and baffling field of vast inquiry has been cleared up, social and, therefore, educational reconstruction continues to be like the game of billiards on an Atlantic liner in a storm—the truly hit ball may go into the pocket at which it is aimed or into the eye of the rival competitor, with a ripped tablecloth into the bargain.

Mark, I beg you, the educational consequences. Already many wise teachers are questioning seriously whether the education of girls from eleven plus ought not to be freed from the barnacles that have accumulated, since the ship of female education was brought to a static anchorage in the centuries-old harbour of male education. One of our greatest needs, therefore, to-day, is another Miss Buss or another Miss Beale, as free as were those great women from the inherited superstitions of their own sex and from the cramping complexes that obsess the male mind, with a new mission to start a second and even more revolutionary chapter in the emancipation of women and the reorganisation of Society.

This does not mean, of course, that any sane man or woman desires to wrench the clock back to tick out demoralising hours in the dreary wastes of 'accomplishments' or to substitute an amateurish sloppiness for a bracing intellectual discipline: still less does it imply that there can be feminine, as distinct from male, mathematics, Greek or logic. But when that woman reformer comes—or perhaps it will be a man—we shall, as usual with all reforms, not be surprised at the results but only wonder that the reform had not been made fifty years earlier.

I repeat that psychology, like physiology, is only in its infancy. Fifty years hence, neither the Behaviourists nor the Subjectivists nor any other of the present camps which end in an 'ist' will demand unfaltering subscription to provisional and half-worked-out hypotheses dressed up as infallible decrees of a Nature which may prove to be neither a radioactive force definable in mathematical symbols, only intelligible to a Newton or an Einstein, nor an accidental entity leaping into a void, after a fortuitous collision of electro-magnetic units in an imaginary etheric universe.

Thirdly and lastly, as the preachers are supposed to say, the end and purpose of education has not yet been settled and, in the nature of things, can never be settled once and for all. We may, if we choose, hold differing views as to what mind is or how it originated or how, in the terms of a really scientific psychology, it works and can be distinguished from its manifestations. We may refuse to believe that mind can operate apart from the material medium analysed by the pathologist, and the neurologist, or we may be convinced that the material medium is simply an

imperfect instrument through which a spiritual Reason alone can work for a structure of human society composed of imperfect physical units, which we call men and women, and that mind, apart from matter, is both prior to, and part of, a rational universe. But the one clear conclusion that no one can evade is that every society everywhere and, therefore, all such societies together on this tiny physical globe are and will continue to be the result of purposive human action, which by an increasing control of *all* the elements at its disposal has made things to be what they are, and is daily altering the process of adaptation, to fit the purposes, wise or illusionary, that it selects as ends worth pursuing. My friend, Rip Van Winkle, had obviously taken from Huxley the conventional distinction between the process of natural selection and the arbitrary interference with that process by man the ethical artificer—*homo artifex*—for his non-natural but rational purposes—the relentless antagonism between a Nature with one purpose and a human reason with a contrary purpose—between Evolution and Ethics. Rip Van Winkle could not be expected to know that that conventional distinction has long been shattered and that it only survives to-day, like the human appendix, because the majority of us are not affected by its vestigial existence, but when it tries to exercise an atrophied function, we have it cut out as an intolerable nuisance.

It is like the old metaphysics or the old psychology, when mind was regarded as working inside a self-sealed box and outside was the whole objective and material world, which surged upon the box, while the something inside the box exercised cognitive and apperceptive powers on the surge—a phenomenal world playing hide-and-seek with mind, which took off its blinkers, from time to time, just to see who was playing the fool with it. Except in Fleet Street, and some old-fashioned laboratories of science and some dusty class-rooms of, shall we say, German philosophy, we educationalists (a horrible word) have long recognised that mind is as integral a part of Nature and the processes of Nature as the so-called natural forces. And that if, for example, Nature has a pruning hook which we call war, it was forged by mind, put into Nature's hand, *i.e.* the hand of purposive men by mind, and that, as with other pruning hooks or scythes, when human minds are, as they can be, bad craftsmen or lose control, what was intended to cut down weeds gashes the user to death. You can make a razor and use it either to cut a human throat, or remove a carbuncle with an aseptic technique, or, like Peter the Great, shave your nobles as the first stage towards reconstructing your system of government, and in so doing you are natural and ethical at one and the same time, and are taking the most appropriate step, as you conceive it, to achieve another stage in your interpretation of life, which is an inextricable mixture of the spiritual, the intellectual, the moral and the physical—a jumble which neither Piltsdown man nor Mr. Bertrand Russell nor anyone in the centuries between them has ever been able to disentangle into separate packets, much less prove that some of the packets are precedent in order of time and are, therefore, 'natural,' while others followed later and must, therefore, be termed 'artificial'—or 'anti-natural.'

Beethoven and Dante are useless to the Patagonian because he cannot fit them into his conception of social life, which really means his scale of

social values. In the twentieth century, Fra Angelico may be for us only an item in a museum of dead junk, much as a modern housewife hangs a Tudor warming pan above her electric refrigerator—unless the vision of life that inspired Fra Angelico continues to have a value which can supplement or intensify the values determining the pattern of the life that we are making with the help of wireless telephony, the songs of Shakespeare or any other material or instrument that seems appropriate. The thin, starved, anæmic and retarding life, whether of to-day or of the age of Tut-an-Khamen or of Siegfried and Brunhild, is the life where purposive man let the scale of values shrivel and, in consequence, the aptitudes that might have flowered withered up, and human appetites were degraded to a limited and purely carnal satisfaction; the fat, well-nourished, rich and expanding life of any epoch has always been when purposive man let the scale of values soar and measured an illimitable horizon, not by what mind co-operating with body had done or were doing, but what they could do if each of the two could be trained by an appropriate technique completely to work together for a unified end—above all, when he became intoxicated by the profoundest of all truths that the spiritual and the material are not in the nature of things antagonists, but allies.

To-day, a social revolution, largely due to the educational progress of the last hundred years, is steadily regrading and reshaping the whole Commonwealth that we call the British Empire. Education has extended the scale of social values, and increasingly intensified in millions of new recruits the power to devise and the desire to will the means to action. But the aid that our educational system can increasingly give to this complicated social and economic transformation is being limited because we have refused to solve the fundamental problem of religious instruction, and to allocate to institutional religion its harmonious place in the task of training for life. Until we have done that, no matter how scientifically planned may be our educational machinery, or how loyally it may be worked, there will be a steady flow of grit clogging the gears and causing all the bearings to run red-hot. This is not the place, nor am I the person, to indicate how and where the true solution can be found; but if it be the supreme function of education to see life as a whole and to train every boy and girl, according to their powers and aptitudes, to a maximum of vision and of a willing reason, so that they can ultimately achieve their truest happiness and their highest efficiency in the new social order, based on the correct allocation of differentiated function, it is my unshakeable conviction that the fundamental place of religion in life must be regarded as an essential preliminary to any further educational advance.

A hundred years hence, Section L may be meeting in London to celebrate a second Centenary of the British Association. Whatever else the President may have to say on that occasion, I trust that he will be able to record this as a mortgage inherited from 1931 and paid off to make a triumphant overture to another century of success.

THE CHANGING OUTLOOK IN AGRICULTURE.

ADDRESS BY

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PRESIDENT OF THE SECTION.

Our survey of the changing outlook in agriculture must begin somewhat earlier than the 100 years over which we are this week looking back. The system of agriculture which dominated Great Britain till recently was developed in the eighteenth century, when the great landowners, such as Lord Townshend and Coke of Norfolk, had brought in new crops, devised new rotations, and, above all, had shown how to combine the production of human food and animal food in one system—the best the world had yet seen. There was as yet no definite agricultural education, and no organised agricultural shows, but the more enlightened landowners invited farmers periodically to such gatherings as the Holkham or Woburn[1] sheep shearings, where new things could be seen and talked about. Much of mediæval England remained; it was essentially an agricultural country, governed by the landowning class, and almost entirely self-supporting in the matter of food production.

Agricultural progress continued during the prolonged wars against the French Republic and Empire, when prices rose to abnormal heights. Many landowners and farmers spent on the land much of the money which they received so bountifully in war-time, when wheat stood for three years at average prices of £5 6*s*. 0*d*. to £6 6*s*. 0*d*. per quarter. There were other contributory causes besides the rise of prices. Encouraged by the writings of Arthur Young and others, local agricultural societies were springing up and organising their own shows, which were more educative than the rather condescending demonstrations of the great patrons, whose day was over. Farmers, too, were beginning to travel. Many a young man took Arthur Young's advice, repeated in each edition of his 'Farmer's Calendar,' that, after having safely got in his hay, and while waiting for his corn, he should 'take his nag for a summer tour, to view some farms in well-cultivated counties and to introduce himself to the conversation of his intelligent brethren, from whom he will be sure to learn something useful.' This educative travel was stimulated by the great improvement in the roads at this time. Till late in the eighteenth century England was

[1] The first Woburn sheep shearing was in June 1797, in the time of Duke Francis. Arthur Young says: 'It continued to be held in the same month every succeeding year, but with increasing numbers and éclat, till it became at last by far the most respectable agricultural meeting ever seen in England, that is, in the whole world—attended by nobility, gentry, farmers and graziers from various parts of the three Kingdoms, from many countries in Europe, and also from America.' The well-known print refers to this later time; Duke John is there the central figure.

not a pleasant country to travel in, except on horseback. Little progress had been made in the means of locomotion since the days of the Romans. It is true that Dr. Johnson declared that 'If I had no duties, and no deference to futurity, I would spend my life in driving briskly in a post-chaise with a pretty woman.' The Doctor was wrong; he would have found himself unpleasantly shaken and occasionally bogged, and he did well to stick to the neighbourhood of Fleet Street. But Macadam changed all this, and the new roads made travelling an enjoyment instead of an unwelcome necessity. We may perhaps recapture something of the spirit of the English countryside from Borrow's 'Lavengro' and 'Romany Rye' (1825), with its alehouses, its comfortable inns, the strange people on the roads, the swaggering coachmen, the love of horses and of boxing, the fear of Popery and of the French.

This progress, however, was largely confined to the southern part of England, where wheat grew easily. Agriculture was much less advanced in the northern counties. As late as 1832, when Cobbett visited Durham and Northumberland, he was very scornful about the pursuit that was there called farming: 'The unnatural efforts,' as he called them, 'to ape the farming of Norfolk and Suffolk; it is only *playing at farming*, as stupid and "loyal" parents used to set their children to *play at soldiers during the last war*.' If they wanted to see the real thing they must come south: 'Tom Baring's farmers at Micheldever had a greater bulk of wheat stacks standing now than in all the North Riding of Yorkshire and one half of Durham.' For all that, however, Cobbett admired the Durham ox, the good pastures, the turnips, and the absence of potatoes, a crop he detested.

Also the economic troubles which beset England after the close of the war in 1815 affected agriculture as well as industry. For the next fifteen years times were very bad. The great war debt entailed heavy taxation, which fell very largely upon the farming class; inflation came to an end with the resumption of cash payments; in spite of the Corn Laws, passed in 1815 to check this, there was a terrible slump of prices, except in times of scarcity, when farmers had little to sell. Select Committees of the House of Commons sat in 1820, 1821, 1822 and 1833 to inquire into agricultural distress, and their reports, especially the last, are depressing reading, far worse than anything we have to-day; there was scarcely a solvent tenant farmer left in the Wealds of Kent and Sussex, and many farmers had lost everything and were working on the roads. The condition of the labourers was pitiable. A usual good rate of pay was 2*s*. per day of 10 hours for a man, 1*s*. for a youth, and 6*d*. for a child. Wages were subsidised out of the rates on the Speenhamland system, the subsidy depending on the price of bread and the number of children. Perhaps even worse than the poverty was the fear of doing anything to improve it. For there was real fear in the land—fear, born of the French Revolution, that any relaxing of the firm hold of the governing classes might plunge England into the horrors of a revolution here. In November 1830 some of the labourers had banded themselves together to try to secure 2*s*. 6*d*. a day; they had rioted but done little damage, and taken no life; yet so great was the fear that several of them were hanged and 450 were transported to Australia.

During these hard years there was a growing struggle between town and country. The Industrial Revolution, which had been going on for some sixty years, was now producing epoch-making results. Village industries were moving into the towns, thus narrowing the range and activities of village life and taking away many of the skilled craftsmen. The Revolution affected political as well as economic conditions. The towns had no desire to be governed by the landowners, and were seeking adequate representation in Parliament; agitation for the Reform Bill was proceeding at an alarming pace, and might have led to civil war, but, as usual in England, common sense prevailed, and in May 1832 Lord Grey was recalled by the King, the Reform Act was passed, and the government of the country passed out of the hands of landowners into those of the middle classes, whose interests were then largely urban. England ceased to be an agricultural country, and became definitely industrial and commercial. The towns had won.

This new orientation of the national life was the dominating factor in 1832, when our hundred-year survey begins. Henceforward the trend of legislation was to be always in favour of the towns whenever their interests conflicted with those of the countryside. The new powers were exercised at once. The rate-aided wage was abolished in 1834 when the new Poor Law system was set up. Agitation was started for the repeal of the Corn Laws. Farmers thought the end was coming.

There was, however, a hopeful feature. The towns were growing and requiring more food; importations from overseas were not serious; labour was abundant and cheap, and there was more and more tendency for the unemployed to move to the towns, thereby reducing the farmers' rates. The path of prosperity lay open to those who could produce more food.

Science now began to come into the picture. It was introduced in England by Sir Humphry Davy, whose lectures at the Royal Institution from 1802 to 1812 on Agricultural Chemistry had attracted widespread attention. He dealt particularly with manures and soils, and discussed the problem, then much agitating farmers, why some soils were so much more favourable to crop production than others; he also introduced a method of soil analysis which was speedily taken up by chemists. The Bath and West, our oldest agricultural society, founded at Bath in 1777, set up an agricultural laboratory in 1806—the first in this country—and appointed Dr. Archer as unpaid 'Chemical Professor to the Society.' Within a few months, however, death, to quote the Society's records,[2] 'put a period to the exercise of his private virtues and public exertions,' and Cadwallader Boyd was appointed as chemist to analyse soils, limestones and other things for their members—the first appointment of this kind I have been able to find. But for all the scientific advocacy of soil analysis one cannot see how the data of these days would help the perplexed farmer. The methods showed the percentages of silica, alumina, lime, magnesia, carbonic acid and 'vegetable fibres and extract,' but no interpretations were possible. It is not surprising that no important results were achieved.

[2] Bath Society's Papers, 1807, vol. ii, pp. xiii and 275.

The second entry of science into agriculture was entirely different, and achieved an astonishing, even dramatic, success. Several quite independent movements led up to it. Farmers were themselves beginning to formulate their problems more distinctly. A definite and precise statement was published by the Royal Agricultural Society on its formation in 1838, setting forth the problems which had been perplexing the leading agriculturists for some time, and insisting on the need to 'inquire after causes.' [3]

The programme was extensive, indeed we have not completed it yet. Although its framers may not have known, the 'inquiry after causes' was already well on the way. The striking feature of the new work was the demonstration that the carbon which formed about half the dry matter of the crop came from the carbon dioxide of the atmosphere, not from humus, as the older philosophers had thought; the agricultural significance was not at first recognised. The question of rotation of crops was also being investigated, and in this the British Association had taken a leading part. At its first meeting at York, in 1831, Prof. Lindley had been asked by the Botanical Committee to present 'an account of the principal questions under discussion in Botanical Science,' and in his report he included this of root excretions: 'the necessity of the rotation of crops,' he said, 'is more dependent upon the soil being poisoned than upon its being exhausted.' Daubeny, Professor of Botany at Oxford, was invited to study the question, and at the 1834 meeting he described his plan. Eighteen different crops were to be grown for a period of ten years on the same ground, some continuously and some in rotation. The crops were to be weighed and analysed, and the effects of continuous growth compared with those of the rotation. This was done; it was the first continuous and systematic plot experiment ever made. These various

[3] *Jour. Roy. Ag. Soc.*, 1840, vol. i, p. li.

The problems were:—

1. *Classification of Soils.*—Chemical methods having achieved no decided success, would geological methods be better? To test the possibilities, a survey of the Weald of Kent and Sussex was proposed; this was afterwards put into the hands of William Topley, the founder of Soil Surveys, whose Memoir is one of the great classics of the subject. (*Jour. Roy. Ag. Soc.*, 1872, vol. viii (2nd series), p. 241; also *Memoirs of the Geological Survey.*)
2. *Permanent Improvement of Soils.*—The most effective method of draining.
3. *Productiveness of Seeds.*—Comparison of the productiveness of different crops and varieties on different soils, including the nutritive and other values of the crops.
4. *Manures.*—Studies of farmyard manure, of town wastes, bones, rapecake, &c., and 'mineral manures,' lime, chalk, gypsum, marl, saltpetre, peat ashes, salt, &c.
5. *Rotation of Crops.*—'The influence, sometimes favourable and in other cases hurtful, which various crops exercise on others by which they are followed and which is now supposed to be occasioned by an excrementitious deposit left by the roots of plants in the soil.'
6. *Mechanics of Agriculture.*—Studies of implements and machines.
7. *Management of Grassland.*
8. *Physiology of Agriculture.*—'More abstract questions, as for instance, that bone manure is beneficial on certain soils, and inefficient on certain other soils—under this head we should inquire after causes and endeavour to answer the question, what is the constituent element of bone that promotes vegetation on some soils, and how is that element rendered inoperative elsewhere?'
9. *Livestock and Veterinary Problems and Diseases of Plants.*—No details are given; too little was known about any of them.

investigations into causes did not directly influence agriculture, but they provided the basis on which further development soon came. In 1837 Liebig had attended the Liverpool meeting of the Association, and he urged upon British men of science to study organic chemistry, 'which, when taken in conjunction with the researches of physiology, both animal and vegetable, which have been so successfully prosecuted in this country, may be expected to afford us the most important and novel conclusions respecting the functions of organisation.' The very shrewd promoters of the meeting replied by asking him to prepare a report on the state of Organic Chemistry and Organic Analyses. This never came; instead, in 1840, he published a volume, 'Chemistry in its Application to Agriculture and Physiology,' stating in the introduction that it was a report presented to the British Association, though it is not mentioned in the proceedings of any of the meetings. Without exaggeration this can be described as the most important publication in the whole history of agricultural science. It brought together the results of the plant physiologists and deduced from them the principles underlying the nutrition of plants, emphasising the fundamental importance of the ash constituents, phosphates and potassium, magnesium and calcium compounds, which no one had previously noticed. Prior to that farmers had been told to supply humus as the source of carbon. Liebig pointed out that carbon was one of the things farmers need not supply, as it was present in unlimited quantities in the air; nitrogen also, like carbon, came from the air, and need not be supplied. On the other hand, the ash constituents came from the soil, and might easily be lacking; these, therefore, must be supplied. The proper way of manuring was to provide ash constituents, not organic matter. The new ideas were exceedingly simple; agriculture suddenly became a branch of chemistry backed by the great Liebig himself. The feeding of crops became almost a matter of arithmetic; the ash of a crop contains so much of a certain element, therefore so much must be present in the soil or added in the manure. Men of science rose to the situation. Murchison, President of the Association in 1846, urged agricultural members to make use of the Association for the solution of their problems. 'And if, above all, they wish us to solve their doubts respecting the qualities of soils, or the effects of various manures upon them, our chemists are at hand.' We are grateful to our distinguished President for making no such promise on our behalf at this Meeting. Meanwhile, a much younger man was beginning his work. John Bennet Lawes, the owner of Rothamsted, had studied at Oxford from 1832 to 1834, attending the lectures of Prof. Daubeny and seeing his continuous plot experiments. On his return to Rothamsted he began pot experiments with various plants, and soon found that growth was improved by sulphate of ammonia, a waste product from gas works. This was not a new discovery, but it was not widely known. Further, he tried another waste product, animal charcoal (which contains much calcium phosphate), and found that it too was effective, especially after treatment with sulphuric acid when the soluble phosphate, then called superphosphate of lime, was produced. A neighbouring landowner put to him the Agricultural Society's problem: Why are bones effective on some soils and not on others? He showed that treatment with sulphuric acid was all that was necessary to make them

generally useful. Further, he showed that mineral calcium phosphate gave the same product, and so could be converted into a valuable manure. All this was done before Liebig's report of 1840 appeared; the work was so novel that Lawes was able to take out a patent for it and so to found the artificial fertiliser industry. He did not at once do this, being dissuaded by his friends—in 1838 a gentleman and a landowner did not embark in trade, least of all in the manure trade—he waited till 1842. He set up a factory at Deptford Creek, though I am unable to find the source from whence he obtained his phosphates in the early years, nor indeed can I recover much information about those years; fortunately there is some record, for in 1851 he took proceedings against various persons for infringing his patents, and the papers preserved at Rothamsted tell us much about the history of the discovery.

Simultaneously with all this, however, the field experiments at Rothamsted were developed, and of these the records are very full. They arose out of the pot experiments, but were quickly expanded to controvert Liebig. Lawes recognised that he could not look after both these and the factory, and in June 1843 he brought in Gilbert to have charge of them, giving him as laboratory the barn in which the chemical work had hitherto been done. Lawes, and especially Gilbert, had all the Victorians' love of controversy. They did not attempt to rehabilitate the old humus theory, nor did they dispute the necessity for potash and phosphates; they showed, however, that these so-called mineral manures were not sufficient, nitrogen must also be given; Liebig had denied this. Secondly, they showed that the composition of the plant afforded no guidance as to its manurial requirements. Turnips contained but little phosphate and much potash, yet they responded to phosphatic far more than to potassic fertiliser. Lawes and Gilbert remained faithful all their days to their first love, nitrogen; and both at Rothamsted and many years later at Woburn, the whole scheme of field experiments revolved round this need for supplying nitrogenous fertiliser. The fame of Rothamsted, however, grew up on the three field experiments; on Broadbalk wheat, the most important crop of the time, showing on the untreated land the 20 bushels per acre familiar to the farmers of the 40's, and on the plots treated with the new artificial fertilisers, especially with sulphate of ammonia, the unusually large yields of 35, 40 or even 50 bushels; the Barnfield, where Lawes' superphosphate gave remarkable increases in yield of turnips, the next most important crop; the increases were at least as good as could be obtained with the best farmyard manure, which then, as now, was scarce; and the adjoining Agdell field showed the great value in the rotation of clover, a fact which was not new, but sufficiently little known to make the demonstration very interesting. Never before had an experimental farm such a striking display of new discoveries; never before had it been possible to show how this wonderful science about which people were talking so much, could do so much for agriculture. The Rothamsted fields were the first effective demonstration grounds, and so well did the farmers of the day appreciate Lawes' work that they not only bought his superphosphate, but after only ten years, in 1853, they subscribed £1,160 to build a laboratory which should take the place of the old barn that had been in use for some fifteen years. This laboratory was the first of its

kind, and it remained in use till 1914. Unfortunately the original barn was pulled down by Lawes, so that we are deprived of what would otherwise have been a wonderful historic memorial.

Had Rothamsted simply been a place for the demonstration of artificial fertilisers, its usefulness would soon have passed. But from the outset it was much more. Like Daubeny's plots at Oxford, to which their general plan seems to owe a good deal, and like the very important farm at Bechelbronn, where Boussingault was carrying out his fundamental researches on agricultural science, the purpose of the work was a search 'after causes,' a search for knowledge. Lawes emphasised this very clearly in his speech in 1855 at the opening of the new laboratory. 'I must explain to you, gentlemen,' he said, 'that the object of these experiments is not exactly to put money into my pocket, but to give you the knowledge by which you may be able to put money into yours, to enable you to judge the properties of all your several crops . . . to give you that knowledge which will enable you to pursue that course which would be most profitable to you.' Throughout the stress is on the gaining of knowledge. This early recognition that the purpose of agricultural experiments is to provide information which farmers can use for themselves accounts for the rapid success achieved.

Armed with this new knowledge and the new fertilisers, British farmers continued to increase their production, and the towns continued to buy still more food. The Repeal of the Corn Laws in 1849,[4] while it lowered corn prices on the whole, did not bring them lower than farmers had known, and improved transport and growing demands made sales much easier.

One of the great obstacles of the day was lack of drainage, but in 1845 Scragg had invented the pipe-making machine, and by 1850 there was sufficient money in the countryside to begin those extensive drainage schemes which did so much for our countryside.

All this time the livestock of the country was steadily improving; the Shorthorn was displacing the Longhorn, other important breeds were defined and their special qualities developed. The standard of farming rose high, prosperity increased, land was brought into cultivation, and if there ever was a golden age for agriculture it was in the 60's and 70's of the nineteenth century. Experts came from many other countries to see and to learn. In 1872 the area of land under arable cultivation in Great Britain was no less than 18·4 million acres, the highest it ever reached. The nation was made as nearly self-supporting as was possible. The system required a considerable demand for wheat at 50*s.* to 55*s.* per quarter, and a considerable supply of good agricultural labour at about 10*s.* to 12*s.* per week; so long as these conditions were satisfied it could continue successfully and indefinitely.

But at the height of its glory the system collapsed. Two causes operated. Labour was not content with the standard of living implied

[4] The Bill was passed in 1846, but did not become operative till February 1849. Trevelyan states that the chief factor was the potato blight in Ireland, which had destroyed the potato crop on which the peasants fed and made cheap wheat vitally necessary. He records Wellington's comment: 'Rotten potatoes have done it; they put Peel in this d—— fright.'

in a weekly wage of 10*s*. to 12*s*. and a 55*s*. price of wheat, and Joseph Arch started his Union in 1872. Even more important, transport was developing and the new countries were opening up.

The fall began in 1874. Wheat had been 55*s*. 9*d*. per quarter on the average for the year. In 1875 it was down to 45*s*. 2*d*., a price at which many farmers could hardly grow it, in spite of the low wages. The United States was sending wheat here in quantity and greatly underselling our farmers. Prices in '76 and '78 were hardly any better (though '77 had been), and then in '79 came a terribly wet year, the worst in the century, when wheat all over the country was badly lodged and badly harvested. Farmers' resources had been depleted by the low prices, and now came low yields and a very expensive harvest. In the old days the price would have risen and righted matters, but now importations increased so much that prices fell below 44*s*. Many farmers were ruined; some hung on hoping for better times, which, however, never came. Another Royal Commission was appointed, and pronounced the distress to be of 'unprecedented severity.' But worse was to come. More and more wheat came from the United States at still lower prices, till in 1894 and '95, through a financial crisis in the Western States, wheat fell to 23*s*. per quarter as the average for the year, while many farmers had to sell for much less. These very low prices did not benefit the townspeople, and they ruined the countryman, causing terrible distress among labourers and farmers, and shattering completely the wonderful system of agriculture that had taken 100 years to build up. Lawes had to confess that science could do nothing to help; it had increased yields per acre and could do so again, but the trouble was too deep-seated to be cured by higher farming.

How had all this come about? For 200 years American farmers and English farmers had never seriously competed, and now all of a sudden the competition became terribly severe. But there had been this difference between American and British farming. Over there man-power had never been abundant, and from the outset American and Canadian engineers had invented machines to do the work with less labour; they did not, like the British engineers, aim at doing it better or at increasing output per acre; their aim was to increase output per man, and in the struggle between the two the higher output per man had won. These developments had been proceeding for many years, and had been much helped by the admirable system of agricultural education that had grown up in the States. So great was American faith in education that even in 1862, during the anxious days of the Civil War, Justin Morrill had been able to get the Morrill Act passed and signed by Abraham Lincoln, establishing in each State a College of Agriculture. The scope of these colleges was widened in 1887 when another great Act, the Hatch Act, provided federal funds for setting up agricultural experiment stations at each of them; further funds were provided by a supplementary Act in 1890. The American farmer of 1894 was therefore well provided with information. He suddenly became an effective force in the world because the chain of transport arrangements from the prairies to the British ports was then completed. British farmers tried in several ways to meet the situation. Some, like Mecchi of Essex, struggled manfully with the

old system, working it more intensively, but they only failed the worse, as Lawes had told them they would. Instinctively most farmers turned to livestock and laid the land down to grass, but as their capital was exhausted they were unable to do it well; nevertheless much of it by good management came off satisfactorily. Many arable farmers went bankrupt and simply gave up the struggle; many Essex farms became almost derelict. They were taken up by young Scots farmers, attracted by the irresistible lure of getting something for almost nothing. They knew and cared nothing about wheat growing, but they were very competent dairy farmers and potato growers, and by dint of hard work and simple living they succeeded in creating a new agriculture that made the farms solvent once more. Gradually it became recognised that specialisation offered the best way out of the farmers' troubles. Now that transport was so efficient, it was no longer necessary for each country or district to aim at being self-sufficing; instead, each region could confine itself to what it could best produce, and import the rest of its requirements from elsewhere. Specialisation allowed of much more efficient production per man, of the introduction and the fullest utilisation of improved methods, and it required that the farmers should be intelligent, mentally alert, fully cognisant of the properties and peculiarities of the crops or animals they were handling, and organised for successful buying and selling.

Fortunately, just at this time agricultural education was spreading in England. There had been since the middle of the eighteenth century spasmodic efforts at agricultural education at the older universities, Edinburgh having the credit for the most sustained teaching; and in 1842 the Agricultural College at Cirencester was founded, which had a great influence in training landowners and land agents. But there had been nothing to reach the farmer; the great link between science and practice had been the Royal Agricultural Society, with its wonderful experts, Augustus Voelcker, Miss Ormerod, and others. A beginning was made in 1888 when the Departmental Committee, presided over by Sir Richard Paget, reported in favour of State-aid for local centres of agricultural education. In 1889 the Board of Agriculture was founded, and from the outset it adopted the policy of establishing agricultural colleges or departments of universities. The great event, however, was the earmarking for technical (including agricultural) education in 1890 of the tax on whisky imposed in the first instance for the suppression of licences, but not so used. This so-called 'whisky money' provided the funds out of which the colleges and farm institutes were set up, beginning with one only, Bangor, in 1889, and ending with eighteen in 1900; more have been added since. The movement spread into the village school; for twenty years it had been a common and legitimate cause of complaint in the countryside that rural education had nothing in common with rural life, that it fitted children only for clerical occupations, and was of little or no help to the future farm worker. The Board of Education appointed a special inspectorate to put this matter right; school gardens were set up and courses designed to help the teacher draw on the countryside for educational material. The purpose was not to make farm labourers, but to develop the power of observation, of recording, of thinking, to show the child something of the infinite wonder and glory of the English countryside,

and to impart a background of knowledge that would enrich its life whether it remained in the country or went to the town.

The pioneers of those days—Middleton, Hall, Wood, Gilchrist, Somerville, Percival, F. B. Smith—to name only a few, had a strenuous uphill task. There was teaching in the college to be done, field experiments to supervise, lectures to farmers in those pre-motor days when there were only open traps and long dreary waits for slow trains; often no chance of getting a decent meal, and, what was worse, sometimes an unsympathetic audience hoping that the local funny man sitting in the back row would be able to score off the unfortunate lecturer. People would write to the newspapers protesting against the idea that a college could possibly teach farmers anything of value. News of this got back to the universities and gave agricultural science a rather bad name. But the pioneers kept on with their struggle, and, inspired by the faith that was in them, they carried agricultural education through the length and breadth of the countryside; their teaching has become part of the light by which we now walk.

Then came the system of County Agricultural Organisers. These now play so great a part in British agriculture that one is apt to forget that they began only about 1900[5]; with them have grown up the farm institutes, and now there are springing up everywhere discussion societies where farmers meet to discuss technical and other matters of importance. At first no provision was made for research; then it was realised that agricultural education could not be carried on without research; one could not go on repeating the same lectures year after year without testing the statements and seeking new knowledge. Research on any important scale became possible only after 1909, when the Development Fund of £2,000,000 was set up at the instance of Mr. Lloyd George for a variety of purposes, including research. The Development Commissioners at the outset adopted the wise policy of allocating the several sections of agricultural science to existing institutions, making grants on an adequate basis, and so ensuring a widespread interest and, perhaps more important, a widespread net to capture young and capable research workers. Crop production (soil, plant nutrition and plant pathology) was placed at Rothamsted, animal nutrition at Cambridge and the Rowett Institute, plant genetics at Cambridge and Aberystwyth, animal genetics at Edinburgh, agricultural botany at Cambridge, dairy research at Reading, fruit at Long Ashton and East Malling, economics and engineering at Oxford, horticulture and low temperature research at Cambridge, veterinary research at Cambridge and Weybridge, helminthology at the London School of Tropical Medicine, glasshouse horticulture at Cheshunt. The scheme is worked through the Ministry of Agriculture, and it is one of the best instances of successful combination of Government supervision of finance with adequate freedom of action for the research worker. The general result of all these activities has been that farmers have learned to cheapen production, to seek profitable outlets for their industry. to use

[5] The present widespread system was set up only in January 1919, when the Board of Agriculture, as it then was, circulated to the counties proposals for a comprehensive system of agricultural education, offering to pay 80 per cent. of the organiser's salary and 66⅔ per cent. of all other approved expenditure.

machinery and any other aids to production. Results soon appeared. When Hall, in the years 1910–1912, made his classical pilgrimage of British farming, he records as his general impression that 'the industry is at present sound and prosperous. . . . Rents have definitely risen with the demand for land that cannot be satisfied, and in all parts of the country men are obtaining very large returns indeed on the capital they embarked in the business.' This was less than twenty years after the deep depression of the early 'nineties!

Then came the war. For the English countryside (so far as any men were left), for the overseas Empire and the United States, it was a time of feverish activity to raise more food to sell to the Allies. Prices were fixed in England, so that money never abounded in the countryside as it had done in Napoleonic times. In spite of the sadness of the war years the farmers of Great Britain put up a wonderful fight to produce food. The history of the time has been written by Middleton.[6] After the war came three years of high prices; in 1920 wheat averaged 80*s.* 10*d.* per quarter, the highest since 1818. Then just as suddenly there came the slump; by 1922 wheat was down to 47*s.* 10*d.* The high prices had done farmers very little good, and in the end they lost all that they had gained. Many landowners proceeded to sell their estates. The high price of produce induced many to bid for the land, and the sitting tenant had either to outbid or be dispossessed. Frequently he had to pay more in interest on loans and mortgages than he had paid in rent, and in addition he has also to maintain the buildings, gates and roads which formerly the estate had done; moreover, as a landowner he has incurred the hearty dislike of some of the town dwellers, who now promise him extra taxation. He is therefore in a far worse position than the farmer of 1821 in the slump after the high prices of the Napoleonic wars. But much worse has come. When the first rush of cleaning up after the Great War was over it was realised that the world's power of producing food had grown far in excess of its power of consuming food. The population had increased but the power of food production had increased much more. In consequence, prices of farm produce have fallen far more than costs of labour and of other commodities. British farmers have turned, as in the 1890's, to livestock, raising lamb, young pigs and milk as far as possible on grass with an increasing acreage of lucerne, thanks to the success of Thornton's inoculation method. Those who cannot produce grass cheaply and easily, but who have to depend on arable land, are in a sorry plight, and the difficulty is not confined to this country; arable farmers in all civilised countries are deeply depressed.

This certainly is not the result that was expected; on the contrary, experts had confidently predicted a food shortage. Sir William Crookes, in his presidential address to the Association in 1898, forecasted the probable world requirement of wheat for the next three decades, and showed that the sources and methods then available would continue to suffice only till 1931, when the world would begin to feel the pinch of hunger. It seemed a tragic ending to the magnificent triumphal march of the nineteenth century. Crookes' figures were remarkably

[6] *Food Production in War:* Oxford (Carnegie Endowment).

accurate, and there can be no doubt that, had science and practice stood still since 1898, we should now be facing the horrors of world starvation. But they have not stood still, and the present position of farm prices is a measure of their advancement.

Two new and closely linked factors have come into play since 1898 and are largely responsible for the present position: the widening of the scope of science in agriculture and the agricultural development of the British Empire and of South America. In the nineteenth century agriculture had been mainly a branch of chemistry; its professors had been chemists, its laboratories chemical. Crookes suggested more chemistry as the way out of what he called the 'colossal dilemma' of world starvation; he proposed the manufacture of more nitrogenous fertilisers from the air—a fantastic idea at the time, yet now our chief source of supply.

The new scientific developments came from the biological side, and the new practical developments from the engineering side. The first great biological triumphs were in plant breeding. There had always been an empirical art of plant breeding and selection which had given to farmers in the nineteenth century the Hallett barleys, Browick, Red Standard and other good wheats, Magnum Bonum potatoes, and sugar-beets of successively higher sugar content; but the results came by accident and not by design. With the discovery of Mendel's laws and the development of the science of plant genetics, the production of new varieties was largely under control; within limits the breeder could work to a specification with considerable hopes of success. The greatest success has been achieved in producing varieties with some special quality such as drought resistance, shortened growing period or stiffer straw; this has proved far more fruitful than the quest for generally improved varieties. For by developing some special quality it has been found possible to cultivate the crop in regions where the older varieties would not grow.

Animal breeding is following the same lines: the empirical work of Robert Bakewell of Dishley, John Ellman of Glynde, the Collins brothers and a host of others, has given us our unrivalled breeds of livestock. Crew and his colleagues at Edinburgh are now introducing the science of genetics into the industry: they have made a promising start: let us hope they will achieve as great results as their colleagues have done with plants.

Canada affords some of the best examples of the plant breeder's success in opening up new regions of the world for settlement. Up to the middle of the nineteenth century the Canadian wheats were suited only to the eastern provinces, Ontario and Quebec; they were uncertain on the prairies. About 1842 David Fife, in Ontario, received for trial from a Glasgow friend several packets of wheat which he sowed. Among the resulting plants was one that differed entirely from the rest, and also escaped damage from rust and frost, two destroyers of wheat in those times. How the seed got there, or whence it came, can never be known. It was a Galician variety. But the accident was a fortunate one for Canada, and did much to build up her wealth. The wheat plant was so good that Fife saved the seed and multiplied it, and in course of time it was widely taken up by farmers under the name of Red Fife. It proved

to be eminently suited to the prairies, and as soon as the railway was completed in 1886 it was taken there by the new settlers and became the basis of their prosperity. So strange an accident could not be expected again, nor did Canada count upon it, yet it happened. The Dominion Experimental Farm was set up in 1886 and its director, William Saunders, began the breeding of new varieties. Many of these, while not sufficiently promising to justify multiplication, were kept alive, and one of them, after ten years of seclusion, was picked out in 1902 by his son Charles, who, regardless of much mild chaffing, applied to all wheats within reach his rapid chewing test for quality. This variety was multiplied, and from 1910 onwards was distributed under the name of Marquis to the prairie provinces and the United States. It ripened earlier than Red Fife and so could be grown further north and west; thus it greatly extended the wheat belt of Canada. But even more good fortune was in store, for its earlier ripening enabled it to escape the worst ravages of stem rust. It has in consequence spread southwards into the United States, and it is now probably more extensively grown than any other variety of wheat in the world.

The Canadian plant breeders continued their search for still earlier maturing varieties; they produced Prelude and Ruby, and now Reward, best of all of them in earliness and in resisting stem rust, requiring only about 100 days from seed-time to harvest, and therefore capable of growing much further north than Marquis. Thus has the plant breeder exploited the first lucky chance that gave the prairies a suitable wheat, and he has produced varieties better and better suited to the northern margin of cultivation, and so has pushed the wheat belt into regions counted as waste in 1900.

Man-power was long the limiting factor in Canadian farming, and this problem of saving labour has been attacked with devastating thoroughness by engineers all the world over. The reaper had come in the 60's, and the binder in the 80's, but the internal combustion engine has made changes vast and dramatic beyond the wildest stretches of the pre-war imagination. The tractor and the new cultivating implements at and before seeding-time, and the combine at harvesting, have revolutionised wheat-growing by dispensing with enormous numbers of men and greatly increasing the area of land needed per man as an economic unit for wheat farming. Not long ago 160 acres was the economic unit for the family farm; now 320 acres is the lowest limit, and 640 acres is nearer the most profitable size. C. W. Peterson in his recent book, 'Wheat,' gives some startling figures. In 1911 sixteen persons were needed on the average to cultivate 1,000 acres of land in the three prairie provinces. By 1926 this number had been reduced to eleven. Further reduction has gone on; during the past two years, he says, mechanisation has displaced over 25,000 men from western farms. Fortunately, there is still land to which they can go, for the new machines and the new varieties have enabled land hitherto unsuitable to be brought into cultivation; between 1911 and 1926 the area under crops had risen in the three prairie provinces from 17·6 millions to 35 million acres. Already Canada has far outstripped the limits set by the experts of thirty years ago, excepting only those of the arch optimist, William Saunders; and no one would now risk his

reputation by predicting the limit to Canada's future accomplishments. The result of the new methods is, according to Mr. Peterson, that wheat can already be produced at 43 cents per bushel, or 14*s.* per quarter (at 25 bushels per acre), and the cost can be further reduced.

Australia also has developed as the result of the activities of the plant breeder and the engineer; the problem here was the conquest of the drought. Farrer began by producing wheats more resistant to rust and drought than the older sorts, and his pupils, Sutton and others, have continued the work. Agriculturists showed the great value of superphosphates for all crops; they further improved the methods of cultivation, and now, as A. E. V. Richardson has shown, for each inch of rain falling during the season, the farmers of Victoria obtain one bushel of wheat, while forty years ago they obtained only half a bushel; further improvement is possible, for with perfect utilisation of the rain one inch should yield 3·5 bushels of wheat. Every new improvement enables the wheat grower to push the wheat belt a little further into the drier inland region, just as in Canada it enables him to push a little further into the northern regions of shorter summers. Some of the most striking agricultural developments of modern times have been in Western Australia.

South Africa owes much of its advances to two other branches of biological science—veterinary science and parasitology. No part of the white man's habitation seems so suitable for insects, and especially parasites, as South Africa. So long as the white man occupied the country only thinly he could do it without difficulty, but trouble began as soon as he wished to increase his hold on the land and multiply his flocks and herds. The first to attack the problem seriously was Arnold Theiler. It is difficult to overrate the value of the service he has rendered to South Africa as a country, and to farm animals the whole world over. He began at the time of the rinderpest plague of 1895, a virus disease which killed almost the entire cattle population of South Africa; the country was also devastated by horse sickness, blue tongue of sheep, heartwater of cattle, sheep and goats, and other terrible diseases. With almost uncanny precision he diagnosed the causes of these diseases and discovered curative measures; he founded the Veterinary Research Laboratories at Onderstepoort, of which not only South Africa but the whole Empire is proud, and he trained up a body of veterinary research workers and officers who now, under the distinguished leadership of P. J. du Toit, are extending the good work. Dr. du Toit, in his brilliant presidential address to this section last year, set out the history and present position of the achievements in veterinary science. These discoveries have had their counterpart in the veterinary services of India and other countries, and animal diseases are now much more under control than they were. However, the task never ends, for as soon as one disease is controlled another seems to rise into prominence. We are still far from security; in the past twelve years foot and mouth disease has cost the British Government over 5½ million pounds sterling paid to the farmers of Great Britain as compensation for animals compulsorily slaughtered, while the farmers themselves have suffered vastly more. Veterinary research is now developing in this country at Cambridge and elsewhere, and the relationships between nutrition and disease are studied at the Rowett Institute.

The engineer has perhaps been the greatest force in the development of New Zealand agriculture. In 1831, the time of our first meeting, the only export from New Zealand was a little flax (with an occasional preserved human head elaborately tattooed); wool was not exported till 1835, and then only from two farms; there was no organised settlement till 1840, when Wellington was founded, and no real movement till 1843, when numbers of sheep were brought over from Australia and established on the Wairarapa plains near Wellington. Wool rapidly became the chief export, followed for a short time after 1870 by wheat, the result of Vogel's development policy, till the invention of refrigeration paved the way for the great dairy and lamb industries, which are now among the most remarkable and efficient agricultural industries in the world. The invention came from Australia; in 1873 James Harrison had been awarded a gold medal at the Melbourne Exhibition for his method of freezing meat. But the method was not developed till 1879, and then it was not successful. The first satisfactory cargo of frozen mutton and lamb came to London from New Zealand in 1882 in a sailing ship fitted with refrigeration appliances; ten years later steamers were introduced, and continuous improvements have since been made. On the agricultural side also the industry has developed remarkably, and from 1921 onwards it has been the subject of a good deal of legislative control, for the New Zealand farmer has learned to combine freedom of action in producing with united action in grading and marketing, and in consequence he has been able to send over here large and regular supplies of uniform high quality, and so to secure an enviable position in our markets. He does this at a profit in spite of his great distance from our markets, and of having to pay wages much higher per man (though not per job) than are paid here; the exports are rapidly rising. In 1929 that of butter was valued at £13·2 millions, of cheese £7 millions, frozen meat (mutton and lamb) £9·9 millions; in all more than £30 millions by refrigeration transport, as against £15 millions of wool—a truly remarkable progress.

The development of the dairy industry, however, was not simply a matter of transport: it is a triumph for the bacteriologist, who has reduced to an exact science the art of producing clean milk, good butter, and cheese true to type. In this country good work has been done at the Dairy Research Institute at Reading by Stenhouse Williams, Golding and their colleagues.

Australia has recently made great progress with the dairy industry, and is now going into the question of lamb. Canada has a highly developed dairy industry. These new developments require compact units, and therefore intensive farming. The natural herbage, supplemented where necessary by mineral licks, had sufficed so long as wool and low-grade beef alone were produced, but with intensification came the necessity for improving the grazing lands. Treatment with phosphate, which Wrightson, Somerville, Gilchrist and others had shown to do so much for British pastures, proved equally effective in New Zealand, the enclosed paddocks of Australia and parts of South Africa; indeed, few results are more striking than those obtained with phosphate on almost any crop in these countries. These problems are now being studied by Orr and the staff of the Rowett Institute. In the moister areas the striking results obtained

with nitrogenous manures on hay at Rothamsted during the past 80 years have been obtained also on grazing land, and intensive methods such as that proposed by Falke and Warmbold in Germany, and developed by Imperial Chemical Industries, are being tried in this country and in the British Empire. Stapledon has shown the marked differences between different strains of grass. The grass lands of the Empire can be considerably improved, and vast increases are possible in the output of meat and dairy products.

Beef production is in a somewhat different category from mutton or pig meat. In the Norfolk rotation it was linked to intensive farming, but this has long been uneconomic, and it is now moving back to the extensive grassland systems. It does not join up well with the systems of producing dairy produce and mutton practised in New Zealand, Australia and Canada, and it requires different refrigerator arrangements. The future supplies appear at the moment to be less extensive and less extensible than those for other products. There are, however, two great regions of the British Empire where great extension will be possible whenever the need arises: the northern part of Australia, and the grass region of Africa lying between latitudes of 20° South and 15° North—roughly between the Limpopo and the Sahara group of deserts—it includes the Rhodesias, Tanganyika, Kenya, Uganda, Somaliland, the Southern Sudan and the Western Colonies. There are, of course, entomological and veterinary difficulties, for insects are in possession of much of this country; there are also sociological problems, for many of the natives do not wish to sell their cattle, holding them as marks of honour and distinction; there are transport problems and many others; probably none, however, is insuperable.

Another result of improved storage during transport has been a great development of Empire fruit growing. Apples and oranges were formerly obtainable in England only in winter; they are now obtainable in spring and summer, thanks to the marked developments in Tasmania, the Murray region in Australia, and South Africa. Plums, peaches, grapes come in abundance from South Africa, bananas from Jamaica; not only are the total imports of fruit increasing, but the proportion from the Empire increases; it had averaged 24 per cent. for the five years 1925–9, and rose to 33 per cent. in 1930; home growers supplied 26 per cent.; usually their share is nearer 30 per cent. The Empire still, however, supplies less than one orange out of every four that we eat, only 39 per cent. of our bananas, 16 per cent. of our grape fruit, and 10 per cent. of our pineapples; there are therefore considerable possibilities of further development. Demand is increasing; in 1930 the consumption of fruit per head of population in Great Britain was nearly 83 lbs., as against 70 lbs. in 1924. Other countries are improving their production and transport. In Great Britain, Barker, Wallace, and their colleagues at Long Ashton, and Hatton at East Malling, have greatly strengthened the fruit-growers' position, and for fruit the outlook is, as for other commodities, a power of production growing greater than the power of consumption. Another important factor in the fruit industry has been the development of canning, which affords a satisfactory way of dealing with excess produce.

Engineering science has further intensified agricultural production by

developments in irrigation. This ancient art originated in Mesopotamia and Egypt, and then almost died out. It was then taken up by the Americans and the British, and is now almost an Anglo-American science. The engineer provides the water and the drainage, the agriculturist devises the appropriate system of husbandry, finds the most suitable varieties and the ways of growing them, and shows how to obtain the maximum value for the water used. The soil expert distinguishes those areas that can advantageously be watered from those that should not, and discovers also the effect which the water will subsequently have on the soil, and the interactions likely to occur between the soil and the soluble salts almost invariably present. The plant pathologist deals with the plant diseases that inevitably occur, and the medical authorities must keep a close watch for malaria. It seems a formidable technical staff, but constant watchfulness is imperative; success in the first ten or fifteen years is easily enough attained, but serious troubles sooner or later dog the steps of those who change a natural desert into an artificial garden. *Naturam expellas furca, tamen usque recurret.* You may drive out Nature with a pitchfork, but she always comes back again. The Spirit of the Waste is not too easily conquered.

The greatest triumphs of irrigation in our time have been in India, There British engineers have set up the greatest dams, the greatest canals, the greatest schemes the world can show. The cultivable area of India has been enormously increased, and land provided for millions of peasants who would otherwise have had none. Since the British introduced the great modern schemes famine has been banished from India—not only famine but even the memory of famine and of the self-sacrificing labours of those who finally overcame it. These Indian irrigation schemes are an unmixed blessing; they are largely used for local food production, and they raise the standard of life for the peasants without lowering the standard of life of anyone else by flooding the world market with cheap products. Irrigation schemes worked by white men are so costly that only valuable products can be raised. The Murray River basin in Australia, the largest white man's scheme in the Empire, produces dairy produce, oranges, peaches, raisins and other fruits for the world market, and rice, which largely goes to the East. The main purpose in Western Canada is fruit and dairy produce, in the White River and other settlements in South Africa, oranges. In hotter regions the schemes are worked by natives under British supervision, but usually for costly crops; in the Gezira cotton is the purpose. In all cases irrigation has greatly increased the output from the land and greatly increased the supplies for the world market. If time permitted, it would be possible to go through the whole list of products of the earth and show how modern science has increased output far beyond human needs, with a resulting fall in demand and lowered prices. One could dilate on the achievements of the Dutch in Java in producing their new sugar cane, which quadrupled the output and so lowered the price of sugar that the West Indies are in terrible distress, the sugar-beet industry of this country is threatened, and all Europe would be in trouble but that they artificially keep out the new sugar. Or again, one could speak of the achievements in rubber growing, of the change over from wild rubber to plantation rubber, of the extra-

ordinary improvements in technique, which have in the past thirty years so enormously increased the output that even the most extensive new demands of modern civilisation—rubber tyres, rubber floors—have failed to keep pace with supplies, so that the price, which in 1910 was 12*s.* 6*d.* per lb., is now reduced to 3*d.*, and may fall still lower, causing great distress to the rubber growers.

Modern science, in short, has been so successful in increasing man's power over Nature that it has brought us harvests far more bountiful than we know what to do with. Science is still advancing, and no one can tell what it will achieve next.

In these circumstances, with this plethora of the products of the soil, with these gifts of Nature poured upon us not merely bountifully but torrentially, so that many of our farmers are likely to be submerged in the process, one might well be tempted to ask should not the scientific workers halt for a time ? It sounds a reasonable question and it is easily answered : they cannot do so even if they wished. Their purpose is to gain knowledge of Nature, especially of soils, crops, animals and their relations to one another, and in this quest there can be no halting. Three reasons will suffice. The march of civilisation is inextricably bound up with the search after knowledge, and all history shows that, when intellectual advancement ceases, civilisation rapidly comes to a standstill. The pursuit of knowledge is a human necessity ; it is part of our make-up, and we owe to it much of what dignity we possess. We could no more suppress it than we could suppress human emotions or physical needs. Secondly, the knowledge so gained furnishes the only possible material for agricultural education. Empiricism alone is never a sound basis ; it may arouse, but it never satisfies, intellectual curiosity, and it does not open up those vistas of promising investigation which a well-designed experiment so often reveals, the exploration of which calls forth and develops some of the finest intellectual qualities in mankind. The necessity for agricultural education is now universally admitted ; only the intelligent, mentally alert, well-trained farmer has much chance of success ; and one cannot have agricultural education without constant research to test and expand the body of knowledge which the teacher imparts, ruthlessly cutting out anything false or unfounded.

And lastly, although we may think in our pride that we have achieved a wonderful control over Nature, yet our control is really very limited, our tenure uncertain, and our margin of safety very exiguous. Crookes' disquieting forecast of 1898 failed to eventuate not because it was false, but simply because new powers were won by mankind in the form of plant genetics and the internal combustion engine. How long mankind will have the wit to go on developing more powers we do not know ; human activities hitherto have gone in cycles, and it may be that the period of scientific activity is nearly ended. It is quite certain that any slackening of control or failure to utilise scientific discovery by any one group of cultivators would speedily eliminate them through pressure of more enlightened and therefore more successful competitors. It is, however, not so much human competition as the opposing natural agencies that must continuously be watched. The weather can still defeat our best laid farming plans. Irrigation schemes, however impressively they seem

to conquer the waste, are always liable to fail through soil troubles, plant diseases or insect attacks. Over large parts of our Empire there is a continuous struggle for possession between insects and men, and the margin of victory, even when we get it, is never very great. And there are new troubles as yet only dimly seen that may easily cause great difficulty in future. The remarkable development of rapid transport has carried all over the world not only the blessings but also the evils of this earth. Pests and diseases of animals, and particularly of plants, have only to appear in one corner of the globe to spread elsewhere with great rapidity despite all regulations to the contrary, often causing enormous losses. Among the most serious troubles of modern times are the virus diseases of plants. These diseases are apparently not caused by any recognisable living organism, nor are they simple physiological disturbances; they cannot yet be attributed to any definite causal agent. They spread rapidly, being frequently carried by small insects, sometimes by mere contact, and they cannot be cured, one can only stand by and see the plants perish. All kinds of crops are affected: sugar-cane, tobacco, cotton, sugar-beet, groundnuts, bananas, potatoes, maize, timber trees (*e.g.* Sandal), large and small fruits (*e.g.* peach and raspberry) and most greenhouse and horticultural plants. And it is not so much sickly plants as healthy ones that suffer; the disease may come suddenly and with great virulence into a healthy prosperous region and devastate the most important crop. In Gambia the Rosetta disease cut down the crop of groundnuts to about one-third of the normal yield.[7] In the United States in 1926 two virus diseases reduced the crop of potatoes by no less than 16 million bushels. In this country the total loss cannot be estimated, but the figures recorded for various attacks vary from 35 to 75 per cent. loss of crop. Worse still is the deterioration of stocks: stocks apparently healthy and vigorous may become worthless in two to four years. Cotton growers are becoming seriously perturbed. In the Gezira last year the losses were considerable, although until recently the leaf-curl disease was unknown there. Sugar-beet in the south-western region of the United States is so seriously imperilled by the curly-top disease that the Government has set aside $300,000 for its investigation. In this country special grants are made to Rothamsted, Cheshunt, Bangor, and other institutions to study these diseases. Tobacco is now being badly attacked, also tomatoes and potatoes; the latest sufferers are the narcissi and daffodils in our own gardens; these cease to flower and shortly perish. Virus diseases are quite recent as serious plagues; if they are old they have hitherto been unimportant or unnoticed. Clearly Pandora's box is not yet empty.

Now a cynic might say that it is no bad thing thus to discover a way of making one blade of grass grow where two grew before. If these troubles affected only certain areas or groups of growers the more fortunate producers might regard them with sympathetic equanimity. Unfortunately, however, they may befall any farmer, good or bad, and the better the farmer the greater the loss may be. Plant pathology has not yet had its Arnold Theiler to show the way of insuring health to farm crops,

[7] Gambia Report, 1925.

nor has it had the success won by the fruit investigators in dealing with their pests.

It is indeed not less knowledge but more knowledge that we want. Every country now recognises this. The United States stands easily first in elaboration of agricultural research, organised not only by the Government but by private endowment. Both in England and in the United States men who have made fortunes in the city have spent their money in developing agriculture or agricultural science—following the advice given by one of Plato's people—having acquired wealth, begin to practise virtue. But there has been this interesting difference. The American patron has spent his money on a college or research station, setting up a laboratory or some other new building, or endowing fellowships, so that a succession of vigorous young people could develop the subject, adding also greatly to their own value as workers for agricultural progress. So the gift has fructified and enriched the community in ever-widening circles. The British patron, on the other hand, has usually spent his money on his own estate, making his own experiments in farming. Some have rendered service by carrying pedigree livestock over periods of depression when the commercial farmer might perforce have had to let them go. But many have simply experimented on no very definite basis and with none of the continuity essential to the success of agricultural investigation. While no doubt getting much amusement out of it themselves, they have not achieved results commensurate with the time and money expended, and in any case their successors promptly stop the whole enterprise, whether good or bad, so that the work soon passes out of memory. Without disputing the inalienable right of the Englishman to spend his money in any way he may think fit, and remembering, too, that the pursuit of agriculture is one of the most honourable ways in which a man can lose money, we can still commend to the English patron the wonderful possibilities of the endowment of agricultural research. To say nothing of Lawes and Rothamsted, think what the world has gained through John Quiller Rowett's gift in 1920 of land near Aberdeen, and of £10,000 to erect buildings, thus founding the Rowett Institute, and how much poorer the world would have been had he simply, like many another man of wealth, spent that money in so-called farming experiments. We in England are proud to think that he was an Englishman. Scotland has recently had a further benefaction in the Macaulay Soil Institute set up to study the peat soils of Scotland and to help the farmers there so long as any men farm in Scotland. We remember with gratitude, and we know that our children will do so, the names of Molteno, William Dunn, Thomas Harper Adams,[8] Charles Seale-Hayne,[9] John Innes for their foundations in this country; Peter Waite and John Melrose for the Waite Institute in Australia, William Macdonald for the Macdonald College in Canada,[10]

[8] Left £26,640 in 1892, but this was allowed to accumulate till 1900, when it was worth about £40,000; the college was then built.

[9] This gift of £141,443 was left in 1903, the college was built in 1914, and formally opened in 1919.

[10] This gift of 8 million dollars in 1904 was only part of Sir William's benefaction for Canada.

Thomas Cawthron[11] for the Cawthron Institute in New Zealand. To-day the need is not so much for new Institutions as for the strengthening of some of those already in existence.

Agricultural science has now widened so much that it is co-extensive with the whole range of science, and this has necessitated considerable expansion of staffs and full interchange of ideas and knowledge between the workers. This has proceeded in two different directions.

Within the Empire all agricultural experts are now in touch with the central clearing houses in Great Britain, the Imperial Agricultural Bureaux, whose function it is to search the world for information likely to be useful and then pass it on to the persons likely to want it. These bureaux were set up at the request of a conference called as the result of the address delivered from this chair in 1924. The system is working well.

World organisation of scientific investigation is proceeding rapidly. It is done on the basis of subjects; its method is the holding of international conferences of the technical and scientific experts who to-day control the machine that works the material part of our civilisation.

Of the three factors involved in the agricultural situation—production, marketing and the scientific advisory and technical system—the last is by far the best organised.

Much has recently been done, however, in developing better and more efficient marketing by the Empire Marketing Board and the Ministry of Agriculture. Happily there is a good demand for high quality produce, for small young animals not too fat; as our civilisation advances the expectancy of human life increases, but that of the farm animals decreases. One difficulty is the elusive British housewife, for whom all this elaboration of effort is made. In the main she knows little about the food she buys, and, having glanced at the bewildering display, she usually chooses whatever is cheapest or gives least trouble—not because she is idle, but because in these days it is impossible for her to get domestic help. So there is a great increase in consumption of tinned and preserved foods, of margarine and of imported chilled or frozen meat; the consumption of fresh food shows no increase per head of population, while that of preserved food does. One of the needs of the day is a definite experimental inquiry to find out whether the freshness of food, of which the British farmer has almost a monopoly, is or is not an advantage to the consumer. So far we have only the Scottish experiments which showed the superiority of fresh over pasteurised milk.

Our greatest need, however, is a better organisation of agricultural production. A beginning has been made by the overseas farmers; the necessity for sending all produce through one or two ports has compelled them to work through large organisations for grading, transporting and selling the produce, with skilled representatives in this country. Dealing in hundreds or thousands of tons they reduce all costs and all wastage to a minimum. Gradually the British farmer is organising; the difficulty is to do this without destroying his sturdy individuality, one of his greatest assets, the loss of which would irretrievably damage our country life.

But greater organisation is possible and is highly desirable.

[11] Born in Camberwell 1833; died at Nelson, N.Z., 1915.

At present British farmers, Empire farmers, and farmers from all over the world indulge in deadly competition in the British market. In the end they obtain wholly inadequate prices. But the community as a whole does not gain because they lose. The final cost of food to the consumer is profoundly affected by costs of handling, transport, preparation and distribution, all expensive services. Better organisation of production, while benefiting the countryman, would not injure the rest of the community.

Thanks to the inquiries made by the Ministry of Agriculture and the Empire Marketing Board, the food requirements of this country are pretty well known. Our next great step forward will be to organise production on a contract basis so as to satisfy these requirements with a reasonable margin of safety, but without the terrible waste involved in those large excesses which injure the grower without benefiting the consumer.

Something of the sort is essential if farming is to survive as an occupation for the best of our people, offering a reasonable standard of living to farmer and worker. The advantages would be incalculable. Organised production and the development of the contract system which has done so much for the milk producers, would permit of a renewal and development of country life to the fullest extent now made possible by scientific and technical advances. By common consent many of the ills of to-day arise from the fact that for nearly a century the industrial side of our national life has been fostered at the expense of the rural side, producing an over-industrialised town population peculiarly susceptible to world economic disturbances, and now largely without employment or prospect of employment. The rural population, on the other hand, is far less sensitive to economic disturbances ; the low rate of unemployment in the countryside shows the greater independence and resilience of the conditions of country life, and points clearly to the fact that improvements in our rural life would benefit not only the countryman but the whole community.

REPORTS ON THE STATE OF SCIENCE, ETC.

Seismological Investigations.—*Thirty-sixth Report of Committee* (Sir Henry G. Lyons (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Mr. W. Hall, Dr. Harold Jeffreys, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Prof. E. A. Milne, Dr. A. Crichton Mitchell, Mr. R. D. Oldham, Prof. H. C. Plummer, Prof. A. O. Rankine, Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Capt. H. Shaw, Sir F. E. Smith, Mr. R. Stoneley, Sir G. T. Walker and Dr. F. J. W. Whipple).

Since the last report was drawn up our esteemed chairman, Prof. H. H. Turner, has passed away. He died at Stockholm on August 20, 1930, where he had gone in his capacity as president of the Seismological Section of the International Union of Geodesy and Geophysics, and also as chairman of this committee. He was elected to the latter office on February 21, 1907, and during the twenty-three years that he has occupied the position it would have been difficult to find one who had more assiduously promoted the interests of seismology. It would be inappropriate to attempt to give here a record of his many activities in the interest of the science. His work in developing and increasing the usefulness of the summary, and his researches on Deep Foci are well known; but perhaps not the least service he rendered was the collaboration he secured by the encouragement he gave, not merely to those at home, but also to observers in all parts of the world. During the year Mr. F. A. Bellamy and Miss E. F. Bellamy have compiled and issued, as a labour of love, a most valuable *brochure*, detailing the extensive nature of Prof. Turner's seismological work. A paragraph in their review may be quoted here as the hope of this committee—that 'the amount of work, energy and interest that Turner threw heartily into Seismology, its continuance as a tribute of respect to John Milne, and his great desire that that part of the seismological work—the digest of the world's earthquake records—should be maintained at Oxford and be associated with the University Observatory, are points worthy of consideration for placing this part of seismological work upon a permanent basis in this country as a lasting memorial to both John Milne and Herbert Hall Turner.' He was a man whom it will be difficult to replace. In the interregnum Sir Henry Lyons has kindly consented to accept the chairmanship. Up to the present time circumstances have delayed the appointment of a successor to Prof. Turner at Oxford, but the University expressed a wish that the seismological work should continue without any alteration; therefore, until the University fills the vacancy it would be inadvisable for this committee to make any plans for the future.

Instrumental.

The two Milne-Shaw seismographs at Oxford have continued to function satisfactorily, but the instability of the masonry pier, which has been referred to in previous reports, still continues.

It may be well to place on record particulars of the five instruments which belong to the Seismological Committee.

No. of Machine.	Location.	Date of Installation.
4	Oxford . . .	1918
6	Cape Town . .	1919
3	Edinburgh . .	1919
27	Perth, W. Australia .	1923
1	Oxford . . .	1926

The machine at Edinburgh was placed originally at Eskdalemuir in 1915 for comparison with the Galitzin seismographs and removed to Edinburgh in 1919. No. 1 was supplied to Mr. W. E. Plummer at Bidston in 1914. It was transferred to Oxford in exchange for No. 32 (see report, 1927).

The four additional seismographs for use in India by Dr. Banerji for further research on microseisms have been constructed at West Bromwich and delivered, as have also the two for the Jammu and Kashmir Government. India is now well equipped with Milne-Shaw seismographs, from Kashmir in the north to Ceylon in the south. The locations are Srinagar, Agra, Bombay, Alipore, Hyderabad, Colombo and Madras. At the last-named station the seismograph is awaiting erection. This will form a valuable distribution of stations for recording the shocks in the Far East.

Bulletins and Tables.

Mr. Bellamy, who has been in charge of the University Observatory during the year, reports that the International Seismological Summary has been issued to the end of September 1927. The first, second and third quarters were posted to 298, 307 and 317 stations respectively. The quarter October–December is in the hands of the printers, and the proofs have been read. The manuscript for January-March 1928 is also ready. Seismological bulletins have been received from fifteen additional stations during the year, and eleven new applicants for the Summary have been added to the list. There is also a steady demand from publishing firms for the publications. The copies thus sold are in addition to the foregoing. It will be seen that the progress of the work is being maintained, notwithstanding that the valuable help of Prof. Turner has been lost, and that the work becomes greater as new stations are added to the list. It is a source of relief that Miss E. F. Bellamy and Mr. J. Hughes have been able to perform the extra work entailed. On fourteen occasions films or copies have been sent to other observatories for the purpose of special research. The discussion of the revision of seismological tables by Dr. Jeffreys in vol. ii., No. 7, of the Monthly Notices of the R.A.S. has been reprinted and issued by the Committee.

Copies of a Catalogue of Earthquakes in Persia compiled by Sir Arnold T. Wilson were also distributed by the Committee. A number of university students have made use of the Milne Library during the year.

Revised Seismological Tables.

By Harold Jeffreys.

During the past year a revision of the Zöppritz-Turner times of transmission of P and S has been carried out. The data used were the residuals of 85 of the most fully observed earthquakes reported in the International Summary for 1923 to 1927. A statistical discussion has shown that the times for P require a reduction running up to 20s. at a distance of 32°·5, falling to 8s. from 60° to 70°, and rising again to 28s. at 107°·5. Similarly the present times for S require a reduction reaching 26s. at 37°·5, falling to 8s. from 60° to 70°, and rising again nearly to 50s. at 107°·5. The standard errors of the results seem to indicate that the new times are correct within one or two seconds at all distances, apart from real variations as between different earthquakes. For P the agreement with a preliminary set of corrections given by Prof. Turner from the earthquakes of 1918 to 1922 is very good. Smoothing and interpolation have been carried out by Dr. L. J. Comrie.

Prof. Turner always refrained from making a change in the tables used as a basis for the reductions in the International Summary on account of the probability that any new tables adopted would themselves be superseded in a few years; but there seem to be no grounds for further delay, and the errors of the present tables are a definite hindrance to understanding the results of the reductions for any individual earthquake, because they exceed the real differences due to the effects of local structure, which must be one of the next objects of seismological study.

At distances between 10° and 20°, especially for S, a complication was found which, for a time, produced a difficulty in interpretation. It is found that the true P and S are followed at intervals of about 8s. by other and somewhat larger pulses, which appear to have been reflected up and down once, twice, or oftener within the upper layers of the crust. These are shown most strikingly in Byerly's study of the Montana Earthquake, and should provide a valuable check on other methods of

finding the thicknesses of the upper layers. It appears that in the reports of many stations these later pulses are given as S. For earthquakes in oceanic regions their behaviour should be markedly different on account of the absence of the granitic layer.

Deep-focus Earthquakes.

By R. Stoneley.

In the preceding report Prof. Turner called attention to the distribution of deep-focus earthquakes. These earthquakes are relatively rare occurrences, only 141 being noted during the years 1918 to 1926; the epicentres nearly all lie on an oval curve which approximately girdles the Pacific.

There has been some reluctance on the part of both geophysicists and geologists to accept as genuine these great depths, amounting in one instance to 0·09 of the earth's radius below the normal focal depth, for at great depths it is generally believed that yield occurs through plastic flow and not by fracture of a material of considerable strength. Further, Dr. Jeffreys has pointed out that, in view of a general dynamical theorem, the amplitudes of the surface waves of earthquakes of great focal depth should be small or insensible; that this crucial test had not been applied, and that the occurrence of readings for L and M in the alleged deep-focus earthquakes appeared *prima facie* to disprove the occurrence of great focal depths.

The occurrence of deep-focus earthquakes has in the last few months been definitely established by two quite independent methods. Mr. F. J. Scrase (*Proc. Roy. Soc. A*, Vol. cxxxii., 1931) has recently obtained a very beautiful confirmation of the reality and order of magnitude of Turner's additional focal depths by noticing that, in general, additional phases should be produced by reflection at points near the epicentre. The additional waves are designated by Mr. Scrase as pP, pPP . . .sP, sS . . . p[P], p[S], &c., in order to distinguish them from the corresponding PP, PPP . . . SP, SS . . . P[P], P[S], &c. Mr. Scrase has made the lengthy computations required to obtain the travel times of the new waves, and has found the pulses to be prominent in the Eskdalemuir and Kew records examined by him. The agreement with observation is extremely good. The phases pP and sS are the easiest to recognise; in the case of a focal depth of 0·06 of the earth's radius below normal and at epicentral distances greater than 60°, pP follows P by an interval of about 80 sec., while sS arrives about 150 sec. after S. For other focal depths the time intervals are roughly in proportion to the depth. Mr. Scrase states further that the surface waves of the deep-focus earthquakes examined by him are very small in amplitude. The International Summary shows that these waves have in the past often been mistaken for P and S; some seemingly anomalous waves given in the 'Additional Readings' fit well into the predicted places.

The smallness of the amplitudes of the surface waves has been noted by Mr. R. Stoneley (*Gerlands Beiträge zur Geophysik*, 29 (1931), 417) from an examination of records taken at Kew, Eskdalemuir, Oxford and Stonyhurst. The effect is strongly marked for earthquakes given by Turner as 0·04 and over. Nearly the whole of the energy is transported by the body waves, so that P, PP, PPP . . . S, SS, SSS . . . have enormous amplitudes, and the large amplitudes of S, SS, SSS, &c., might easily be mistaken for those of surface waves. A graph of the L and M readings in the International Seismological Summary shows at once that this is what has happened. At the calculated time of arrival of M there is usually a movement of rather small amplitude, quite unlike the characteristic long-wave phase, and this is attributable to the dying down of the body waves. Further, there is a great scarcity of L and M readings, especially of M, in these very deep-focus shocks. The scarcity of normal M readings confirms the assumption of great focal depth. Theory indicates that sudden beginnings for L, corresponding to the arrival of infinitely long Love waves (0·41 min./degree) and Rayleigh waves (0·47 min./degree), should appear, though possibly of smaller amplitude than in a normal shock; actually the former seems to occur more frequently than the latter, and their infrequency doubtless arises from the absence of the typical train of long waves that follows these two onsets in normal shocks.

The genuineness of Prof. Turner's deep foci may accordingly be considered as established; the actual values of these additional depths, which depend in the end on the Zöppritz-Turner tables, may require some modification.

The depth of focus of a normal earthquake was believed by Prof. Turner to be about 0·04 of the radius of the earth. The agreement of the Byerly-Jeffreys time

curve (calculated on the basis of a focus near the surface) and the recorded times of transit of well-observed earthquakes is excellent. Further, the corrections obtained by Prof. Turner and those independently found by Dr. Jeffreys, both from material derived from the International Seismological Summary, are in close agreement with the Byerly-Jeffreys time curve. This depth of 0·04 for the normal focal depth will therefore almost certainly have to be abandoned.

Some Recent Shocks.

By A. W. Lee, R. Stoneley, and F. J. W. Whipple.

The North Sea Earthquake of 1931, *June* 7.

Very early on Sunday, June 7, 1931, at about 1.26 a.m. Summer Time, an earthquake shock was felt in most parts of Great Britain. In Hull and Bridlington the movement displaced chimneys and tiles, and there was some panic in the streets. In the north of Norfolk most people were awakened and windows rattled violently. In London, on the other hand, but few people woke up; two distinct pulses were felt by individuals who had not retired for the night. The shock was felt in the Channel Islands, in the north of France, in Belgium, Holland and Denmark, and in Germany at places as far away as Hamburg and Brunswick. As far as is known no earthquake has ever been felt over such a large area in the neighbourhood of the British Isles.

The records of the earthquake are being studied by Dr. H. C. Versey and Mr. R. Stoneley, of Leeds University, who would be glad to receive reports of personal experiences. A seismic map has been prepared from about 400 observations, and on this the isoseismal lines are well defined. The boundary of the area in which the strength was 4 or greater on the Rossi-Forel scale (*i.e.* movable objects shaken, windows rattled) passes approximately across Somerset, the Irish Sea, and northwards across Scotland to Banffshire. Isoseismal 5 (shock felt by most people, large objects shaken) crosses the Firth of Forth, Morecambe Bay, and across the Midlands to Harwich. The line of isoseismal 6 (most sleepers awakened, with some people rushing in terror out of houses) was from the mouth of the Tees, through Yorkshire, passing to the east of Leeds and meeting the coast again near Yarmouth. There is a small area round Hull, Bridlington and Filey in which damage was done to houses (strength 7). The shock was felt as far west as Waterford in Ireland.

Excellent seismographic records of the earthquake were obtained at all the eight British stations as well as at numerous stations on the Continent. Disturbances were also detected at a few stations in the United States.

The interval between the P and S phases on the seismograms at Kew Observatory was 30 sec., corresponding to an epicentral distance of 270 km., and the bearing (obtained by comparison of the north and east components of P and P_g) was estimated as $22° \pm 2°$. These data imply an epicentre with co-ordinates 53·8° N., 1·2° E. By comparison of the times of arrival of the first pulse at certain stations Father Rowland found the co-ordinates 53° 57′ N., 1° 25′ E. Mr. Stoneley, using the data from twenty-five stations, finds that the known velocity of transmission of P to distances less than 1,000 km. (7·77 km./sec.) gives close accordance between the calculated times and the observed times of arrival of P, provided Father Rowland's epicentre is used. This epicentre therefore cannot be greatly in error, and it is interesting to note that for a near earthquake an accurate epicentre can be obtained from the times of arrival of P alone. The epicentre indicated by the seismic map agrees closely with that obtained by Father Rowland.

The epicentre was under the North Sea near to the Dogger Bank, and about 60 miles from the coasts of Yorkshire and Norfolk. There is no record of any previous earthquake with this epicentre. The last considerable earthquake under the North Sea was on January 24, 1927, about 400 miles away and close to the coast of Norway. This earthquake was felt in Scotland and the east of England.

The shock was felt on several ships in the vicinity. The navigator of a motor-boat, about 25 miles S.E. of Flamborough Head, reported that a sound like that of a distant lightship gun was first heard; followed by a series of underwater explosions, like those of depth charges four to six miles away. The sea was then calm, but fifteen or twenty minutes later a heavy swell developed, and became very confused, appearing to roll from all directions.

The spacing of the isoseismal lines suggests that the focus was decidedly deeper than in the case of most of the small shocks felt in the British Isles ; these latter most probably originate in the sedimentary rocks. The North Sea earthquake, however, is not to be classed among the so-called deep-focus earthquakes, as the appearance of the records, in which P_g is evident, shows that the focus was not below the granitic layer.

New Zealand Earthquake.

In our last report Prof. Turner drew attention to two severe shocks which had occurred, with loss of life, at Wellington, New Zealand. Unfortunately these were merely the precursors of a disastrous shock which visited Napier, Hawkes Bay, and Wellington at 22 h. 46 m. G.M.T. on February 2. Great loss of life was occasioned, and the town of Napier was practically destroyed ; property was seriously damaged over a wide area. By coincidence Mr. J. J. Shaw was inspecting a smoked paper seismograph at West Bromwich, and saw the record commencing. He watched it through, and on noting that the MAX-P difference was the greatest possible, realised before the record was complete that the shock was in the vicinity of New Zealand.

An earthquake which produced even greater disturbances on the seismographs of Great Britain occurred on August 10, 1931. The epicentre was near the Great Altai Mountains in Mongolia.

British Earthquakes.

In addition to the North Sea earthquake the following tremors were reported :—

1. 1931, March 21. Earth tremors were felt in the district of Stoke-on-Trent, and these were recorded at West Bromwich at 6 h. 10 m. and 7 h. 47 m. G.M.T. Houses were rocked, furniture was displaced and crockery rattled, but there were no personal injuries or serious damage to property.
2. 1931, April 14. A disturbance in South Carnarvonshire shortly before noon was at the time attributed to earth tremors. Subsequent information has shown that it was due to a meteor falling in the district. Information regarding the travel of the meteor and the distribution of the sound waves has been collected by Mr. John R. Owens of Llanrhystyd.
3. 1931, May 3. An earth tremor occurred in Manchester on the morning of May 3, 1931 ; there were no personal injuries, and damage to property was slight.

English Channel Earthquakes.

Mr. A. E. Mourant has published (M.N., R.A.S., Geophys. Suppl. II, 374) the results of a detailed study of three earthquakes in the English Channel. The calculations follow the method adopted by Jeffreys in discussing the Jersey and Hereford earthquakes of 1926. The travel times of P, P*, P_g, S, S*, S_g, agree closely with those found by Jeffreys and so confirm his estimates of the thickness of the granitic and intermediate layers. Waves are found that are probably propagated in the sedimentary layer ; they are designated P_s and S_s, as had already been suggested independently by Mr. E. Tillotson.

The energies of the three earthquakes studied were roughly 5×10^{17}, 10^{17}, 10^{18} ergs respectively. It is suggested that the much lower values of the energy derived from the Zürich records arise from the scattering of energy by the roots of the Alps.

Seismological Work at Kew Observatory, Richmond, Surrey.

Microseisms.—The continuous movements known as microseisms are recorded by all sensitive seismographs. The movements are much greater in winter than in summer, and appear to be associated in some way with atmospheric storms. The nature of the connection is, however, still in doubt. To provide more intimate knowledge of this subject comparisons of microseismic activity at the seismological observatories in Great Britain have been undertaken. To initiate this investigation the seismograms of one month—January 1930—were examined, and the microseisms for four hours daily (1 h., 7 h., 13 h. and 18 h.) were tabulated.

The means of period and amplitude, together with the maximum amplitudes, are set out in the following table :

Microseisms, Great Britain, January 1930.

Observatory.	Type of Seismograph.	Component.	Mean period. sec.	Mean Amp. μ	Max. Amp. μ
Dyce (Aberdeen) . . .	Milne-Shaw	N—S	6·4	2·9	10·8
Edinburgh	,,	E—W	6·4	5·5	16·6
Durham	,,	N—S	6·1	3·3	10·7
Stonyhurst . . .	,,	E—W	6·3	3·7	11·2
Liverpool	,,	N—S	6·3	4·6	16·0
Oxford	,,	N—S	6·5	4·8	16·8
,, ...	,,	E—W	6·5	2·9	9·7
Kew	Galitzin	N—S	6·7	2·3	7·2
,, ...	,,	E—W	6·8	2·5	9·4
,, ...	,,	Z	6·7	2·7	10·2

What geographical, geological or instrumental causes can account for the large differences between the characters of the microseisms at different places, is a mystery.

It is held by some seismologists that microseisms are generated when sea waves break against the coast, whilst others believe that the oscillations of the ground are due to wave motion of the sea causing variations in pressure on the sea bed.

The outstanding feature of the tabulation of microseismic activity at British stations during January 1930 is a rapid increase in activity on the 11th and a rapid fall on the 12th, the maximum amplitudes being in every case about three times the average. A deep depression was centred between Scotland and Iceland at the time, and an intense secondary travelling rapidly from the west crossed England during the afternoon of the 12th. Neither on the Atlantic coast of the British Isles, nor round the North Sea, nor on the coasts of Norway or Iceland, nor round the Bay of Biscay, was there a conspicuous maximum of sea disturbance or of wind force during the night when the microseisms were so much exaggerated. As far as can be seen the observations give no support to either of the current theories.

The Focal Depth of Earthquakes.

An innovation in practical seismology was made on February 20, 1931, when Mr. F. J. Scrase, who had been studying the characteristics of the seismograms of earthquakes with abnormal focal depth, saw that a disturbance recorded at Kew Observatory was of this nature, and was able to announce that the focal depth of the earthquake, which had an epicentre near the Sea of Japan, was probably about 250 miles. The records of this disturbance are being collected at Kew, and a detailed investigation of the phases is in progress.

An account of the research on earthquakes with deep foci appeared in the Proceedings of the Royal Society, A, Vol. cxxxii., 1931. The following papers of seismological interest have also been published :

F. A. Bellamy and E. F. Bellamy. Herbert Hall Turner : A Notice of his Seismological Work. County Press, Newport, I.O.W.

H. Jeffreys. The Revision of Seismological Tables. Monthly Notices. *Geo. Supp.*, January 1931.

A. E. Mourant. Earthquakes of the Channel Islands Neighbouring Countries. Société Jersiase, 1931.

F. J. Scrase. Two notes on the Operation of Galitzin Seismographs. *Meteorological Office Geophysical Memoirs*, V. 49, 1930.

F. J. Scrase. The Instrumental Phase-difference of Seismograph Records : an Illustration of the Properties of Damped Oscillatory Systems. *Proc. Phys. Soc.*, Vol. xliii. 3, No. 238, 1931.

R. Stoneley. Some near Earthquakes reported in the International Seismological Summary. *Monthly Notices. Geo. Supp.*, January 1931.

R. Stoneley. On Deep-focus Earthquakes. *Gerlands Beiträge zur Geophysik*, Vol. xxix. (1931), pp. 417–435.

F. J. W. Whipple and A. W. Lee. Studies in Microseisms : (*a*) The Question of Diurnal Variation ; (*b*) The Variation of Amplitude with Period. *Monthly Notices R.A.S., Geophys. Supp.*, January 1931.

Calculation of Mathematical Tables.—*Report of Committee* (Prof. J. W. NICHOLSON, *Chairman*; Prof. E. H. NEVILLE, *Acting Chairman*; Prof. A. LODGE, *Vice-Chairman*; Dr. L. J. COMRIE, *Secretary*; Drs. J. R. AIREY, A. T. DOODSON, R. A. FISHER, J. HENDERSON, J. O. IRWIN, Prof. A. E. H. LOVE, Dr. E. S. PEARSON, Mr. F. ROBBINS, Drs. A. J. THOMPSON and J. F. TOCHER, Mr. T. WHITWELL and Dr. J. WISHART).

General activity.—Seven meetings of the Committee have been held in London. Dr. R. A. Fisher resigned as General Editor, and Dr. J. Henderson was appointed to edit the volume described in detail in the Committee's last report. Dr. E. S. Pearson and Mr. F. Robbins were co-opted by the Committee.

The grant of £65 has been expended as follows:—

Calculations connected with Emden's equation . . .	£60
Clerical and postal expenses	£5

Volume prepared.—The sum of £200 was voted in November 1930 by the Council for the printing of this volume, and arrangements were immediately made for its publication. Great care has been taken with typographical details in the endeavour to set a standard for the printing of tables of this nature. It is hoped that the volume will be published shortly before the Centenary Meeting, and that it will be the first of a series.

Cunningham Bequest.—In October 1930 the Royal Society was asked by the Secretary of the Association to give a ruling for the guidance of the Committee on the terms of this bequest. The reply was, in effect, that (*a*) it would be very difficult to state in general terms what tables could be regarded as coming within the terms of the bequest, but not difficult to give a ruling in the case of any particular table suggested, and (*b*) the proposed tables of elliptic, Bessel and confluent hypergeometric functions were not regarded as falling within the terms of the bequest.

The Committee has been in touch with various authorities on the theory of numbers, in an endeavour to establish a suitable programme of tabulation. Final suggestions are not yet available.

Calculating machines.—Two machines, a Brunsviga-Dupla and a Nova-Brunsviga IVA, have been purchased from the funds of the Cunningham Bequest. These machines are of general utility, and will be used in whatever programme is ultimately undertaken with the Cunningham Bequest. Meanwhile they have been used in research on interpolation formulæ, and in the solution of Emden's equation.

Emden's equation.—Tables have been prepared of the solutions of this equation together with certain auxiliary quantities derived from the fundamental solutions. Prof. Sir Arthur Eddington, at whose request these tables were undertaken, has arranged with the International Astronomical Union for a grant of £100 for printing expenses. It is anticipated that a volume of about 50 pages will be issued before the end of this year. A description of the methods employed will be given in the preface to the published tables.

Interpolation formulæ.—The Committee has devoted considerable attention to the question of the best methods of presenting tables so that values of the functions for arguments intermediate between those tabulated can be readily obtained, regard being had to two conflicting interests, namely the cost of printing and the convenience of the user. The standard practice has been the printing of even order differences as far as necessary so that these may be used in conjunction with a formula such as Everett's, the coefficients of which have been tabulated.

The attention of the Committee was drawn to a method proposed by Jordan (*Metron*, Vol. vii, No. 3, 1928, pp. 47–51) which consists of linear interpolation between the tabular values u_0 and u_1 adjacent to the required interpolate, then between u_{-1} and u_2, u_{-2} and u_3, and so on as far as required. If these linear interpolates are represented by $a, b, c, d, e \ldots$ the final interpolate is

$$a - G''(b-a) + G^{\text{iv}}(c - 3b + 2a) - G^{\text{vi}}(d - 5c + 9b - 5a)$$
$$+ G^{\text{viii}}(e - 7d + 20c - 28b + 14a)$$

where G'', G^{iv}, etc. are the coefficients of the even order differences in Gauss's formula and have been tabulated up to G^{vi} in Chappell's *Interpolation Tables*. The required functions of a, b, c, . . . are easily obtained as the even differences of the symmetrical series . . . *cbaabc*. . . .

The Jordan formula is therefore of an interesting new type which does not require the previous preparation of differences, and it is well adapted to routine computation. It is not claimed that an interpolate is obtained as quickly with the formula as by using Everett's formula with even differences already provided. The Committee gave some consideration to this point, and finally decided that for the tables in the volume now being published, which are all single-entry tables, the expense of printing the necessary number of even order differences was fully justified. The formula is worthy of consideration in the case of tables not provided with differences.

It should be mentioned that interpolation in a table not provided with differences may be performed more quickly by the aid of the Lagrange formula, given tables of the coefficients and a calculating machine of sufficient capacity. The general use of Lagrange's formula is, however, not convenient for three reasons: (1) No tables of the coefficients have been published, although the Committee has manuscript tables of the 4-point and 6-point coefficients at intervals of 0·001; (2) the tables required are more extensive than those for Everett's formula, because separate tables must be used according to the number of tabular entries employed; (3) it is not possible to tell by inspection of a table how many values should be used in order to interpolate to the full accuracy of the table.

In connection with the expense of printing differences a further consideration is that the fourth, sixth, eighth and tenth differences, if less than 1000, 10,000, 100,000 and 1,000,000 respectively, can be dispensed with entirely if they are multiplied by the factors −0·18393, −0·20697, −0·21803 and −0·22456 respectively, and the products added to the difference of the next lower even order, which is then used in exactly the same way as an unmodified difference. This method of "throw back" has been adopted where applicable in the volume prepared, resulting in the saving of six pages of printing. In each case where it is applied (and this will be indicated) the user is saved the trouble of finding two coefficients from the tables of coefficients, and of performing two multiplications. The modification of the differences, however, leads to more labour on the part of the proof-reader, who is responsible for the accuracy of every printed column. It also complicates another problem that sometimes arises—namely that of finding differentials from the differences.

In cases where the functions tabulated are related in such a way that they are successive derivatives of one function, no differences are needed, except perhaps at one end of the series of functions, as Taylor's theorem can be applied directly. An instance of this will be found in the *Hh* functions (integrals of the probability integral) in the forthcoming volume.

The Committee is indebted to Dr. A. C. Aitken, of the Mathematical Department of the University of Edinburgh, for help given in investigating the Jordan formula. A paper on "Interpolation without Differences" is being presented to the Mathematics Sub-section of Section A by Dr. J. Wishart (see p. 17).

Future programme.—The programme outlined in last year's report is still before the Committee, but each of the propositions there put forward will require considerable financial assistance. The Committee would like to be able to publish in the near future the tables of elliptic functions (described in the last report) computed for the Association under the superintendence of the late J. W. L. Glaisher. For the purpose of checking these by differencing (the original 10-figure calculations are in the possession of the Committee in the form of thirty bound manuscript volumes, but exhaustive inquiry has failed to reveal any trace of systematic checking, especially by differencing) and preparing the manuscript of 200 pages for the printer, a sum of £100 would be required. The printing would cost about £450–£500.

A request has been received from Prof. S. Chapman for the calculation of the associated Legendre functions $P_n^m(\nu)$ and $Q_n^m(\nu)$ for values 0, 1 and 2 of m and values of n that are half an odd integer. These are required for numerical work in connection with anchor ring problems.

Reappointment.—The Committee desires to be reappointed, with a grant for general purposes of £100, which would be expended on the tables of the associated Legendre functions and on work for the proposed volumes of Bessel and confluent hypergeometric functions.

Parachors.—*Report of Committee* (Dr. N. V. SIDGWICK, *Chairman;* Dr. S. SUGDEN, *Secretary*; Dr. N. K. ADAM) *appointed to collect and tabulate all available data on the Parachors of Chemical Compounds with a view to their subsequent publication.*

THE Committee met in November 1930 to consider the arrangement of the list of parachors. It was decided that compounds containing carbon should be listed in the order used in Richter's 'Lexikon,' and that inorganic compounds should be arranged in alphabetical order of their symbols. The preparation of the list in card index form was left in the hands of the Secretary.

The card index is now almost complete and contains entries for more than 500 substances. A number of references have, however, to be verified before the list will be ready for typing.

No expenditure has yet been incurred; the chief cost will be the typing of the list from the card index. It is requested that the grant be carried over for another year to cover this expense and enable the list to be brought into a form suitable for publication.

During the year a list of parachors has appeared in Landolt-Börnstein (Physikalisch-Chemische Tabellen, 5th ed., 2nd supplementary volume, part 1, pp. 172–188). This list is arranged in groups of chemically similar substances; it is by no means complete and contains a number of errors, hence it cannot replace the proposed B.A. list.

Photographs of Geological Interest. — *Twenty-sixth Report of the Committee* (Professors E. J. GARWOOD, *Chairman, and* S. H. REYNOLDS, *Secretary*; Messrs. E. G. W. ELLIOTT, J. F. JACKSON *and* J. RANSON, Professor W. W. WATTS *and* Mr. R. J. WELCH.)

IN the present report 121 photographs are listed, bringing the number in the collection to 8,403. A further series of 11 prints comes from the Reader series, 1,144 photographs having been added to the collection from this source.

The present series includes an excellent set from Cornwall contributed by Mr. E. Jones, a series illustrating washouts in the Chalk of Kent from Mr. A. L. Leach, others from Herefordshire and Pembrokeshire contributed by Mr. G. Macdonald Davies, from Antrim sent by Mr. A. R. Dwerryhouse, from Cardigan by Mr. J. Challinor, and from Somerset by Dr. F. B. A. Welch. Miss Spencer has kindly presented some remarkable photographs of 100- and 200-ton blocks of concrete shifted by the sea at Holyhead, these having been taken by the late H. E. Spencer. The Hon. Secretary contributes sets from Dorset and Cattybrook, near Bristol.

The new series of geological photographs the publication of which has been in preparation for some years, is now ready. Application should be made to the Hon. Secretary for information.

The Reader negatives being the property of the Committee, prints (¼-plate) may be obtained through the Hon. Sec. at 4*d.* each, lantern slides at 1*s.*

The Committee recommend that they be reappointed.

TWENTY-SIXTH LIST OF GEOLOGICAL PHOTOGRAPHS.

FROM AUGUST 1, 1930, TO JULY 1, 1931.

List of the geological photographs received and registered by the Secretary of the Committee since the publication of the last report.

Contributors are asked to affix the registered numbers, as given below, to their negatives, for convenience of future reference. Their own numbers are added in order to enable them to do so. Copies of photographs desired can, in most instances, be obtained from the photographer direct. The cost at which copies may be obtained depends on the size of the print and on local circumstances over which the Committee have no control.

The Committee do not assume the copyright of any photograph included in this list. Inquiries respecting photographs, and applications for permission to reproduce them, should be addressed to the photographers direct.

Copies of photographs should be sent, unmounted, to

Professor S. H. REYNOLDS,
The University, Bristol,

accompanied by descriptions written on a form prepared for the purpose, copies of which may be obtained from him.

The size of the photographs is indicated, as follows:—

L=Lantern size.	1/1=Whole plate.
1/4=Quarter-plate.	10/8=10 inches by 8.
1/2=Half-plate.	12/10=12 inches by 10, &c.
P.C.=post card.	E signifies Enlargement.

ACCESSIONS.

England.

CORNWALL.—*Photographed by* E. H. DAVISON, B.Sc., *School of Mines, Camborne.* P.C.

8288 Hayle Towers . . . . Sand Dunes.

Photographed by T. C. F. HALL, *Dunedin House, Basinghall Avenue, E.C.2.* 1/4.

8289 Nelly's Cove, N. of Porthallow . Raised beach on Veryan series.

Photographed by E. JONES, *St. John's Lane, Bedminster, Bristol.* 3×2.

8290 (1) The Logan Rock . . . Weathering of granite along joints. 1930.
8291 (2) 'Dr. Syntax rock' near Logan rock Weathering of granite along joints. 1930.
8292 (3) Land's End . . . . Marine erosion of granite. 1930.
8293 (4) Lamorna Cove . . . Valley eroded along fault in granite. 1930.
8294 (5) Gurnard's Head from S. . Epidiorite sills. 1930.
8295 (6) Gurnard's Head from N. . Epidiorite sills. 1930.
8296 (7) Land's End . . . . Marine erosion of granite. 1930.

Photographed by the late T. W. READER, *and presented by* F. W. READER. 1/4.

8297 Near Mullion . . . . Foliation of Hornblende Schist. 1913.
8298 Porthleven . . . . Contorted Mylor beds. 1913.
8299 Mullion Island, Lizard . . 1913.

DEVONSHIRE.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* 1/4.

8300 Axmouth or Dowlands Landslip . General view, looking W. 1928.

Photographed by the late T. W. READER *and presented by* F. W. READER. 1/4.

8301 Axmouth or Dowlands Landslip . Large foundered mass. 1914.
8302 Axmouth or Dowlands Landslip . Foundered masses of Chalk. 1914.
8303 Axmouth or Dowlands Landslip . Foundered masses of Chalk. 1914.
8304 Axmouth or Dowlands Landslip . Foundered masses of Chalk. 1914.
8305 Axmouth or Dowlands Landslip . Foundered masses of Chalk. 1914.

DORSET.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* 1/4

8306 Handfast Point near Swanage . Isolation of Promontory. 1912.
8307 (1931·4) Just N.W. of Portland Bill Coast erosion. 1931.
8308 (1931·9) Black Nore, Portland . Section Portland Stone and base of Purbeck. 1931.

8309	(1931·16) Nothe Section, Weymouth	Sea-worn surface of *Trigonia* beds. 1931.
8310	(1931·20) Lulworth Cove, E. side.	Section 'Broken Beds' to 'Cockle Beds.' 1931.
8311	(1931·22) Lulworth Cove, E. side.	'Broken Beds.' 1931.
8312	(1931·24) Lulworth Cove, E. side.	'Stools of trees' conspicuous. 1931.
8313	(1931·25) Lulworth Cove, E. side.	Purbeck Section. 1931.
8314	(1931·26) Fossil Forest, Lulworth.	Section Portland Stone to 'Broken Beds.' 1931.
8315	(1931·31) 'Fossil Forest' Lulworth	Irregular breaking of 'Broken Beds.' 1931.
8316	(1931·34) 'Fossil Forest' Lulworth	Caps and 'Broken Beds' (Lower Purbeck). 1931.
8317	(1931·36) 'Fossil Forest' Lulworth	Lower Purbeck section. 1931.
8318	(1931·37) 'Fossil Forest' Lulworth	Lower Purbeck section. 1931.
8319	(1931·39) Osmington Mills . .	Bencliff Grit doggers. 1931.
8320	(1931·41) Coast just W. of Lulworth Cove	Portland and Purbeck section. 1931.
8321	(1931·43) Stair Cove, Lulworth .	'Caps and dirt beds.' 1931.
8322	(1931·44) Stair Cove, Lulworth .	Dirt beds and adjacent strata. 1931.
8323	(1931·45) Man of War Cove, Lulworth.	Slickensided Chalk. 1931.
8324	(1931·47) Man of War Cove, Lulworth	Crushed Flints in Chalk disturbed by Isle of Purbeck fault. 1931.
8325	(1931·48) Man of War Cove, Lulworth	Crushed flints in Chalk disturbed by Isle of Purbeck fault. 1931.

GLOUCESTERSHIRE.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* 1/2 *and* 1/4.

8326	(28·27) Cattybrook cutting, E. of bridge	D_2 section. 1928. 1/2.
8327	(28·28) Cattybrook quarry . .	Trias on Coal Measures. 1928. 1/2.
8328	(28·29) Cattybrook quarry . .	Trias on Coal Measures. 1928. 1/2.
8329	(28·31) Cattybrook old quarry .	Highly disturbed Coal Measure grits and shales. 1928. 1/2.
8330	(23·28) Cattybrook cutting E. of bridge	Carboniferous Limestone (D_2). 1928. 1/4.
8331	(24·28) Cattybrook cutting, E. of bridge	Streaky D_2 limestone. 1928. 1/4.
8332	(21·28) Cattybrook cutting, E. of bridge	Streaky D_2 limestone. 1928. 1/4.
8333	(20·28) Cattybrook cutting, E. of bridge	Streaky D_2 limestone. 1928. 1/4.
8334	(28·34) Cattybrook quarry . .	Disrupted grit band in Coal Measure. 1928. 1/4.
8335	(28·30) Cattybrook quarry, old workings	Synclinal fold in Coal Measures. 1928. 1/4.

Photographed by J. E. LIVINGSTONE, *The University, Bristol.* 1/4.

8336	Bristol	Cotham Marble.
8337	Bristol	Upper surface of Cotham Marble.
8338	Bristol	Abnormal Cotham Marble.
8339	Aust	'Crazy' Cotham.

HAMPSHIRE.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

8340	W. of Boscombe Pier . . .	Boscombe sand with slip in the foreground. 1910.

HEREFORDSHIRE.—*Photographed by* G. M. DAVIES, M.Sc., 104, *Avondale Road, South Croydon.* 1/4.

8341	(30·1) Park Wood quarry, Colwall	Wenlock Limestone. 1930.
8342	(30·5) Knap Lane quarry, Ledbury	Upper Ludlow. 1930.
8343	(30·6) Tunnel quarry, Ledbury .	Aymestry Limestone. 1930.
8344	(30·7) Eastnor looking E. to Malverns	S. end of Malverns.

Photographed by S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol* 1/4.

8345	Old Perton quarry, Woolhope .	Aymestry Limestone. 1931.
8346	Ledbury tunnel, W. end . .	Highly inclined Ledbury beds. 1915.

HERTFORDSHIRE.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

8347	Ayot	Section of Reading Beds. 1910.

KENT.—*Photographed by* A. L. LEACH, *Giltar*, 74 *Shrewsbury Lane, Shooter's Hill, Woolwich, S.E.*19. 1/4.

8348	(1) Lewisham, Ballast Pit . .	Gravel filling a wash-out in lower peaty silt. 1928.
8349	(2) Lewisham Ballast Pit . .	Gravel filling a wash-out in upper silt beds. 1928.
8350	(3) Lewisham Ballast Pit . .	Peaty silt showing seasonal or varve lamination. 1928.
8351	(*a*) Farmingham Hill . .	Eocenes filling pipe in Chalk. 1922.
8352	(*b*) Two miles E. of Dartford .	Mixed infilling, loam, clay and rubbly chalk in Chalk. 1922.
8353	(*c*) Two miles E. of Dartford .	Pipe in Chalk. 1922.
8354	(*d*) Watling Street, Rochester way	Piping of Chalk under Thanet Sand. 1922.
8355	(*e*) Watling Street, Rochester way	Block jointing in Chalk. 1922.
8356	(*f*) Watling Street, 9 miles E. by S. of Dartford	Sarsen with mammillated surface. 1923.
8357	(*g*) Shorne Wood . . .	Lignite band in Woolwich Beds. 1922.
8358	(*h*) Shorne Wood . . .	Lignite with selenite in Woolwich beds. 1922.

MIDDLESEX.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

8359	Ponder's End Station . .	Low level gravels. 1909.

SOMERSET.—*Photographed by* Miss E. W. GARDNER, 47A *Dartmouth Park Hill, N.W.*5. E.

8360	Bowldish quarry, $\frac{1}{3}$ mile N. of Welton railway station	Attenuated Lower Lias with non-sequence. 1927.

Photographed by the late T. W. READER *and presented by* F. W. READER. 1/4.

8361	S. of Woodhill Bay, Portishead .	O.R.S. with cornstone development 1919.

Photographed by S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* E. *and* 2×3.

8362	Vallis	Inferior Oolite on Carboniferous Limestone. 1930.
8363	Vallis	Inferior Oolite on Carboniferous Limestone. 1930.
8364	Vallis	Inferior Oolite on Carboniferous Limestone. 1930. 2 ×3.

Photographed by F. B. A. Welch, Ph.D., 28 *Jermyn Street, London, S.W.* 1/4.

8365	(1) Vobster quarry, N.W. corner .	D-limestone overthrust on Coal Measure shale.
8366	(2) Vobster quarry, S.W. corner .	Overthrust Millstone Grit and Carboniferous Limestone.
8367	(3) Vobster quarry . . .	Overthrust Millstone Grit and Carboniferous Limestone.
8368	(4) Vobster quarry, W. face .	Overthrust Carboniferous rocks.
8369	(7) Vallis, quarry 3 . . .	Thrust in Carboniferous Limestone.
8370	(5) Vallis, quarry 3 . . .	Thrust in Carboniferous Limestone.
8371	(6) Vallis, quarry 3 . . .	Thrust in Carboniferous Limestone.

Surrey.—*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

8372	Headley Common . . .	Current bedded ? Pliocene sand. 1928.
8373	Headley Common . . .	Current bedded ? Pliocene sand. 1928.
8374	Addington, near Croydon . .	Blackheath pebble beds. 1928.

Wales.

Anglesey.—*Photographed by the late* H. E. Spencer *and presented by* Miss Spencer. 1/1.

8375	Holyhead breakwater . .	100 tons and 200 ton concrete blocks moved by the sea. 1914.
8376	Holyhead breakwater . .	100 ton and 200 ton concrete blocks moved by the sea. 1914.

Cardigan.—*Photographed by* J. Challinor, M.A., *University College of Wales, Aberystwyth.* E.

8377	(1) Craig-y-Delyn, 1 mile S.S.W. of Borth	Cliff in rocks dipping steeply seaward. 1929.
8378	(2) Foot of Craig-y-Delyn, Borth	Wave-cut pot-hole. 1929.
8379	(3) Clarach Bay, 1 mile N. of Aberystwyth	Wave-cut platform with etching of grits. 1929.
8380	(4) N.E. of Aber-Arth . .	Earth pillars in boulder clay. 1929.

Pembrokeshire.—*Photographed by* G. M. Davies, M.Sc., 104 *Avondale Road, South Croydon.* 1/4.

8381	(30·18) E. side of Caer Bwdy Bay St. David's	Fold in Solva Beds. 1930.
8382	(30·22) Middle Mill, Solva, St. David's	Dolerite sill in Cambrian. 1930.
8383	(30·26) Ramsey Sound, looking N.N.E. from Road Uchaf, Ramsey Island	Arenig in foreground, basic intrusions in distance. 1930.
8384	(30·31) Whitesand Bay and Carn Llidi, St. David's	Gabbro of Carn Llidi, intrusive in Arenig. 1930.

Photographed by A. L. Leach, *Giltar*, 74 *Shrewsbury Lane, Shooter's Hill, Woolwich, S.E.*19. 1/1.

8385	Headland $1\frac{1}{4}$ miles N.E. of Tenby.	Reticulate weathering in Coal Measure Sandstone. 1915.

Photographed by J. Ranson, 174 *Willows Lane, Accrington.* 3×2.

8386	(1) Near Broadhaven, St. Brides Bay	Overfold in Coal Measures. 1930.
8387	(2) Near Broadhaven, St. Brides Bay	Overfold in Coal Measures. 1930.
8388	(3) Near Broadhaven, St. Brides Bay	Overfold in Coal Measures. 1930.
8389	(4) Near Broadhaven, St. Brides Bay	Overfold and thrust in Coal Measures. 1930.

Photographed by S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

8390	(05·72) Nun's Chapel, St. David's	Basal conglomerate of Cambrian. 1905.

Scotland.

Inverness.—*Photographed by the late* G. W. Palmer. 1/4.

8391	Beinn Dearg Mor and Beg from head of Loch Slapin, Skye	Shows the smooth rounded outline of the granophyre hills. 1910 or 1911.
8392	The Storr, Skye . . .	Basalt cliffs in foreground. 1910 or 1911.
8393	Dun Mor, Sanday, Canna . .	Conglomerate bands interbedded with basalts. 1910 or 1911.

Kincardine.—*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

8394	(12·41) N. of Stonehaven . .	Highly inclined Downtonian. 1912.
8395	(12·45) Cowie	Reticulate weathering of Downtonian Sandstone. 1912.

Kircudbright.—*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

8396	(28·63) Black Crag or Cairnsmore of Dee	Shows the rounded outline characteristic of many granite hills. 1928.
8397	(28·64) Near New Galloway .	Growth of water plants obliterating pond. 1928.
8398	(28·65) Near New Galloway .	Growth of water plants obliterating pond. 1928.

Ireland.

Antrim.—*Photographed by* A. R. Dwerryhouse, D.Sc., 16 *Darell Road, Caversham, Oxfordshire.* P.C.

8399	(3) Knocklayd near Ballycastle .	Wind erosion of mountain peat.
8400	(4) $2\frac{1}{2}$ miles E. of Ballintoy. .	Sill of columnar basalt overlain by lava.
8401	(1) Garron Tower . . .	Landslip of Tertiary basalt and Chalk over Lias.
8402	(2) 1 mile N. of Garron Tower .	Weathering of slipped mass of basalt.

Dublin.—*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

8403	(28·68) Portraine	Disturbed Ashgillian. 1928.

Palæozoic Rocks.—*Report of Committee*, Prof. W. W. WATTS (*Chairman*; Prof. W. G. FEARNSIDES, *Secretary*; Mr. W. S. BISAT, Dr. H. BOLTON, Prof. W. S. BOULTON, Dr. E. S. COBBOLD, Prof. A. H. COX, Mr. E. E. L. DIXON, Dr. GERTRUDE ELLES, Prof. E. J. GARWOOD, Prof. H. L. HAWKINS, Prof. V. C. ILLING, Prof. O. T. JONES, Prof. J. E. MARR, Dr. F. J. NORTH, Mr. J. PRINGLE, Dr. T. F. SIBLY, Dr. W. K. SPENCER, Dr. A. E. TRUEMAN, Dr. F. S. WALLIS) *appointed to excavate critical sections in the Palæozoic rocks of England and Wales.*

AN expedition was arranged by the Geological Department of the National Museum of Wales in June 1929 with the object of undertaking intensive collecting at certain important horizons on Ramsey Island.

Owing to the difficulty often experienced in crossing from the mainland, the party, consisting of members of the museum staff and Dr. Pringle, together with three labourers, camped on the island.

Special attention was paid to beds near the base of the Ordovician and in the upper part of the Cambrian, and a fairly extensive collection of fossils was obtained from a number of important localities that were indicated by Dr. Pringle.

One of the special objects of the visit was to rediscover the horizon from which certain earlier workers had obtained starfish remains, and after considerable search such a horizon was located at the northern end of the island and some echinodermatous material obtained. The remainder of the material consists principally of trilobites and brachiopods.

Owing to building operations at the Cardiff Museum, detailed work upon the collection has not been possible this year, but the material has been labelled in readiness for such work.

The specimens upon which asteroids or other echinoderms were present have been submitted to Dr. W. K. Spencer, who writes as follows:—'The material you sent me contained a block and counterpart (29.308 Cr. 35 and Cr. 26. Loc. 3), which showed the moulds of three specimens of a new Asterazoan. It is akin to *Uranaster*, a species of which *Uranaster ramseyensis* was the sole form known previously to these rocks. Although akin to the previously known species, it has such distinctive differences that I may have to put it into a new genus.

'Obviously you are on the track of a very old fauna, older than that known from Dyl of Bohemia. Only one Asterozoan is known earlier, and that is an undescribed genus *Cambraster*, Jaekel, from the Middle Cambrian of France.

'There were a fair number of other Echinoderm remains, and several looked to me well preserved. They look as if they would be well worth the attention of appropriate specialists.'

Great Barrier Reef.—*Final Report of Committee* (Rt. Hon. Sir M. NATHAN, *Chairman*; Hon. J. HUXHAM and Sir E. H. MACARTNEY, *Treasurers*; Prof. J. STANLEY GARDINER and Mr. F. A. POTTS, *Secretaries*; Prof. Sir EDGEWORTH DAVID, Prof. W. T. GORDON, Prof. A. C. SEWARD and Dr. H. H. THOMAS, from Section C; Mr. E. HERON ALLEN, Dr. E. J. ALLEN, Prof. J. H. ASHWORTH, Dr. G. P. BIDDER, Dr. W. T. CALMAN, Sir SIDNEY HARMER, Dr. C. M. YONGE, from Section D; Dr. R. N. RUDMOSE BROWN, Sir G. LENOX CONYNGHAM, Prof. F. DEBENHAM, Admiral DOUGLAS, Mr. A. R. HINKS, from Section E; Prof. F. E. FRITSCH, Dr. MARGERY KNIGHT, Prof. A. C. SEWARD, from Section K) *appointed to organise an expedition to investigate the Biology, Geology and Geography of the Australian Great Barrier Reef.*

AT the time of the last meeting of the British Association all the British members of the expedition had returned to England and a good deal of progress had been made into the working out of the results. It was, however, decided to delay the presentation of the final report till this year so that the complete arrangements might be recorded.

One meeting of the Committee has been held during the year.

It was decided that, so far as possible, the results of the work of the expedition should appear as a uniform series. The Royal Geographical Society were responsible for the organisation of a section of the expedition, and papers by Mr. J. A. Steers and Mr. Michael Spender were read before the Society and are to be found in the Geographical Journal, while the economic researches will be published in Australia. Apart from these, however, the series of results are being published by the British Museum (Natural History). Application was made by the Committee to the Trustees of the British Museum, who most generously agreed to undertake the full publication of the work and bear the entire expense so entailed. The reports are appearing in quarto form, and it is expected that four volumes will be produced, the first containing a preliminary account of the expedition and the physiological researches (those conducted by the 'Leaders Party'), the second the plankton and hydrographical researches (those conducted by the 'Boat Party'), the third the oecological researches and work on breeding, development and growth of Corals and other reef organisms (conducted by the 'Reef Party'). The four volumes are being published concurrently, and all the reports are appearing as separata as they are ready; a large number have now been published. The Committee wish to place on record their profound gratitude to the Trustees of the British Museum for their generous and far-seeing action.

The archives of the Committee and the collections made by the expedition will ultimately be deposited in the British Museum (Natural History) with the exception of a series of duplicates and some type specimens which are to remain in the Australian Museum in Sydney. Although the programme of work did not admit of much time being given to faunistic collecting, much assistance was received in this department of the expedition's activities from five members of the Australian Museum, who spent varying periods with the expedition, and the very full oecological data accompanying the specimens throughout give special value to the collections which have been made. The plankton collections made by the Boat Party are of the greatest value as representing the first systematic collection of plankton ever made over a period of one year in tropical seas and being accompanied by the fullest hydrographical details.

A most fitting sequel to the work of the expedition in Australia was the formation by the Queensland Government of a permanent Marine Biological Service. Mr. F. W. Moorhouse, one of the Australian members of the expedition, has been put in charge of this, and the huts, equipment and scientific library handed over by the expedition to the Queensland Government, in recognition of the indispensable aid given by the Government and people of Queensland, form the nucleus of the first permanent Marine Laboratory to be established in Australia.

The Committee do not ask for reappointment, but they resolved that a nucleus consisting of the chairman, the secretaries, the treasurer, Dr. Calman and Dr. Yonge, should be retained to deal with matters concerning the interests of marine biology in the region in which the expedition worked.

Electrical Terms and Definitions.—*Report of Committee* (Prof. Sir J. B. Henderson, *Chairman*; Prof. F. G. Baily and Prof. G. W. O. Howe (*Secretaries*); Prof. W. Cramp, Dr. W. H. Eccles, Prof. C. L. Fortescue, Prof. A. E. Kennelly, Prof. E. W. Marchant, Dr. A. Russell, Sir F. E. Smith, Prof. L. R. Wilberforce).

During the past year six members of the Committee have written and circulated theses dealing with different aspects of the questions under consideration. The difficulties and differences of opinion with which the Committee is concerned are being considered and debated in many other countries and by an International Electro-technical Commission. All these bodies are finding very great difficulties in arriving at an agreed system of definition; these difficulties are largely due to the different methods by which the subject is approached and treated by physicists and electrical engineers.

A committee, under the chairmanship of Sir Richard Glazebrook, has recently been appointed by the International Union of Physics to consider the same subject from the physicist's point of view. Up to the present the discussion has been mainly confined to those engaged in the teaching and practice of electrical engineering, as they are faced by the difficulties that arise through the use of terms to which different meanings are attached by different schools of physicists and electrical engineers.

An attempt to obtain agreement on definitions immediately discloses differences of conception and opinion of a fundamental nature, and it is very important that an effort be made to obtain agreement between physicists and electrical engineers on these fundamental points.

We therefore consider it inadvisable to issue a definitive report at the present juncture, but recommend that the Committee be reappointed and that Sir R. Glazebrook, Dr. W. E. Sumpner and Dr. D. W. Dye be added to the Committee.

Stresses in Overstrained Materials.—*Report of Committee* (Sir HENRY FOWLER, *Chairman*; Dr. J. G. DOCHERTY, *Secretary*; Prof. G. COOK, Prof. B. P. HAIGH, Mr. J. S. WILSON).

THE programme of work set out in previous interim reports has been proceeded with, and the Committee submit as their report for this year the following contributions, embodying a general survey of the phenomena of yield in mild steel under various types of stress.

1. 'An investigation of the hardness of a steel tube along certain Lüders or Piobert lines.' By Sir Henry Fowler, K.B.E. (Pubd. in *Engineering*, vol. cxxxii, p. 299.)
2. 'Change in indentation hardness of test-pieces resulting from varying amounts of deformation or plastic flow during the application of tensile stress.' By Sir Henry Fowler, K.B.E. (*Ibid.*, p. 420.)
3. 'The phenomena of tensile yield in mild steel and iron.' By James G. Docherty and F. W. Thorne. (*Ibid.*, p. 295.)
4. 'The upper and lower yield points in steel exposed to non-uniform distribution of stress.' By Prof. G. Cook, D.Sc. (*Ibid.*, p. 343.)
5. 'Plastic strain in relation to fatigue in mild steel.' By Prof. B. P. Haigh, D.Sc., and T. S. Robertson, B.Sc. (*Ibid.*, p. 389.)

(*Note.*—These papers were published in *Engineering*, and by the courtesy of the editor have been reproduced by the Association, and may be obtained—gratuitously by members of the Association entitled to the Annual Report: price to others, 1*s.* 6*d.*—on application to the British Association, Burlington House, London, W. 1.)

Sumerian Copper.—*Report of Committee* (Mr. H. J. E. PEAKE, *Chairman*; Mr. G. A. GARFITT, *Secretary*; Mr. H. J. BALFOUR, Mr. L. H. DUDLEY-BUXTON, Prof. GORDON CHILDE, Prof. C. H. DESCH, Prof. H. J. FLEURE, Prof. S. LANGDON, Mr. E. MACKAY, Sir FLINDERS PETRIE, Mr. C. LEONARD WOOLLEY) *appointed to report on the probable source of the supply of copper used by the Sumerians.*

(By Prof. C. H. DESCH, F.R.S., University of Sheffield.)

Fourth Interim Report.

DURING the past year Mr. E. S. Carey has been engaged intermittently on analytical work for the Committee, and a quantity of micrographic work has also been carried out. The largest number of specimens during the year has been received from Mohenjo-Daro. (The balance in every case represents copper.) The results are as follows:—

No.	Level in feet below Datum.	Tin per cent.	Lead per cent.	Iron per cent.	Nickel per cent.
9366	23·54	4·9	6·3	1·1	tr.
9368	22·88	—	—	tr.	tr.
9376	24·17	—	tr.	tr.	tr.
9392	23·72	13·7	—	tr.	tr.
9421	22·92	—	tr.	tr.	0·84
9442	24·34	22·1	14·9	tr.	tr.

Table continued from previous page.

No.	Level in feet below Datum.	Tin per cent.	Lead per cent.	Iron per cent.	Nickel per cent.
9446	24·24	—	tr.	tr.	0·96
9457	24·87	—	—	—	0·34
9460	24·65	+			
9472	25·12	+			
9474	25·54	—	tr.	1·08	tr.
9476	24·41	—	tr.	tr.	—
9477	24·85	tr.	tr.	1·4	tr.
9478	24·85	tr.	tr.	tr.	—
9483	24·40	+			
9497	26·21	15·0	1·37	0·76	tr.
9503	25	+			
9509	26·19	—	tr.	0·7	0·4
9514	25·72	12·3	3·0	1·4	tr.
9531	26·25	3·3	3·6	tr.	tr.
9548	26·99	11·5	tr.	tr.	tr.
9549	26·25	1·2	tr.	4·1	1·9
9555	26·12	—	tr.	2·2	0·6
9562	26·58	—	tr.	tr.	tr.
9567	26·87	26·9	tr.	traces only	—
9583	26·41	tr.	—	,,	tr.
9597	28·00	+		,,	
9617	27·78	tr.	tr.	,,	tr.
9620	26·48	—	tr.	,,	tr.
9666	29·07	+		,,	
9676	29·02	+		,,	
9682	30·52	0·6	—	,,	
9714	30·51	+		,,	
9715	30·38	tr.	tr.	,,	0·37
9722	30·48	22·2	0·86	traces only	0·75
9728	30·23	tr.	tr.	,,	0·25
9734	30·49	—	tr.	,,	tr.
9740	30·60	12·0	tr.	,,	0·23
9742	30·16	+		,,	
9744	30·16	+		,,	
9765	31·64	+		,,	
9767	31·85	+		,,	
9770	32·20	0·07	—	,,	tr.
9784	32·11	—	—	,,	—
9789	31·89	+		,,	
9808	33·03	tr.	—	,,	0·82
9825	33·40	8·3	tr.	,,	0·48
9828	32·88	+		,,	
'Piece of Copper'	36·03	+		,,	

The specimens in the second section of the Table, taken from the lower levels, were received at a later date. None of these contained more than traces of iron. A plus mark (+) indicates that the element in question was present but that the quantity of material was too small for an analysis. It will be seen that both bronze and copper specimens are found at all levels.

Three specimens from Ur from the British Museum, which were under examination for the purpose of studying the process of corrosion, were also analysed.

	Tin per cent.	Nickel per cent.	Lead per cent.	Iron per cent.
Dagger (Early) B.M.L. II . . .	20·2	0·7	nil	tr.
Axe (Late) U 11475, B.M.L. XIII .	1·64	0·21	tr.	1·0
Axe (Early) U 12483, B.M.L. XIV .	5·8	tr.	tr.	tr.

Some drillings were received from Mr. H. Frankfort, Oriental Institute of the University of Chicago. These drillings, marked K 351, were taken from a statue found at Khafaji, 20 miles north-east of Baghdad. The remains are believed to be contemporary with the royal tombs at Ur and the A cemetery at Kish, but the site is farther north than any other from which Sumerian metals have been found. This proved to contain 0·63 per cent. Tin, no Nickel, and traces only of Lead and Iron. The form of the statues was such that they must have been cast, and the quantity of Tin is sufficient to allow the making of a clean casting.

Specimens were received from the Susa Mission through M. R. de Mecquenen, of the Louvre, Paris. No. 1, Cast axe, marked 'Attapakŝon,' about the time of Hammurabi, contained Nickel 0·45 per cent., Iron 2·9 per cent., traces of Tin, Antimony and Arsenic; no Silver or Lead. No. 2, Hoe with socket made by hammering—period about Sargon I, contained Nickel 0·12 per cent., Iron 1·34 per cent., traces of Tin, Arsenic, Antimony and Silver, and no Lead. Other samples from the Susa Mission gave the following analyses :—

No.	Tin per cent.	Nickel per cent.	Iron per cent.	Lead per cent.	Antimony per cent.
1801	tr.	tr.	0·96	—	tr.
1955	tr.	0·30	1·6	—	—
1820	—	tr.	1·05	tr.	—
1944	1·63	0·35	0·88	tr.	—

A series from Thermi in Lesbos was received from Miss Winifred Lamb. These consisted of copper specimens, some of which contained Tin in small quantities, the highest being 1·65 per cent. This specimen also contained 0·42 per cent. Nickel, but none of the others had more than 0·05 per cent. of that element.

An examination has also been made of some of the Luristan bronzes from Persia, received from Mr. A. U. Pope. These will be reported on fully in the volume of Persian Art now in preparation. Both copper and bronze specimens were found, the Tin being in one instance as high as 19·7 per cent. Nickel was usually present only in traces, but one bronze contained 1·1 per cent. of that element. A silver ring contained both gold and copper, and was evidently made from the native metal.

Only a few samples of ore have been received this year. By the courtesy of the Hon. the Agent to the Governor-General and Chief Commissioner in Baluchistan, four samples of ore were obtained, two only of which proved to contain copper. No. 1 was from Koh-i-Lar at a place about six miles west from the Lar Post. This ore contained 42·76 per cent. Copper and only a trace of Nickel. Sample No. 2 came from Koh-i-Lar, about five miles southwards from Lar Post. It contained 16·80 per cent. Copper and no Nickel. The other two ores contained Lead, but no Copper.

Information obtained from the India Copper Corporation, Ltd., states that the copper mines in Singbhum were undoubtedly worked in ancient times, and large slag heaps still exist. The ore worked in recent years has contained Copper and Nickel in the approximate ratio 95 to 5. The two metals do not occur in the same mineral, but Chalcopyrite and Pyrrhotite are intimately mixed, so that in smelting Copper and Nickel would be obtained together. The ore is free from Arsenic, Antimony and Bismuth, and the quantity of precious metals present is negligible. A circular plate of cast Copper was found at the Masaboni mine, obviously of ancient origin, and had the following analysis :—

Copper	94·44 per cent.
Nickel	3·56 ,,
Iron	0·883 ,,
Sulphur	0·118 ,,
Insoluble	0·688 ,,

It will be noted that these are sulphide ores, and that the most ancient coppers and bronzes examined by us have not contained sulphur, having been obtained from the smelting of Malachite or other oxidised ore. It is probable that oxidised outcrops occurred above the deposits of sulphide ore, but have now been worked out. It is desirable that specimens of ore from other regions within reach of the Sumerians should be examined, and some clues are now being followed up.

In view of the fact that many early specimens have now been found to contain Tin in such a small proportion as to allow of clean casting, but without justifying the application of the word 'bronze,' the question of the accidental occurrence of small quantities of Tin minerals in association with the Copper minerals calls for further examination.

The Committee asks for reappointment with a grant of £100.

Excavation of Early Sites in Macedonia.—*Report of Committee* (Prof. J. L. Myres, *Chairman*; Mr. S. Casson, *Secretary*; Dr. W. L. H. Duckworth, Mr. M. Thompson).

For this year's excavations in Macedonia, a site in Western Macedonia was chosen, at the village of Armenochóri, near Flórina, some twenty kilometres south of the Greek-Jugo-Slav frontier. The narrow valley, 2,000 ft. above sea-level, is here watered by numerous tributaries running northward to the Tserna River, on one of which the site lies.

The excavations revealed a deposit of an average depth of two metres, containing two occupation levels. Both belong to the Early Macedonian Bronze Age culture, which is known, from previous excavations by the British School of Archæology in Athens in other parts of Macedonia, to have flourished about 2500–2000 B.C., and to have been the counterpart of the Early Helladic culture further South, both being probably of Anatolian origin and developing on roughly parallel lines.

At Armenochóri, the two levels represent the two phases of this culture, the earlier being imposed upon an indigenous Neolithic culture (numerous elements of which were found associated with it); the later being a development of the earlier.

In the upper level were found vases, with two high-swung ribbon-shaped handles, a form which was to have great vogue in the succeeding Period in the South, and throughout the subsequent history of Greek pottery. Its discovery in such quantity in this early context at Armenochóri is thus of unusual interest. Besides these vases, a large quantity of coarse cooking-vessels were found, some bored stone celts, small stone saws, and other stone objects, all proper to this culture, as well as a remarkable clay figurine. Except for the neolithic sherds, and one incised sherd, no obvious contacts with more northern cultures were observed.

After the conclusion of the excavation, a short excursion was made into Jugo-Slavia to the region between Bitolj and Prilep, where similar mounds had been reported. About twenty kilometres north of Bitolj, a settlement belonging to the same culture was identified.

The excavators were Mr. W. A. Heurtley, Assistant-Director of the British School of Archæology in Athens, Mr. C. A. Ralegh Radford, Mr. G. A. D. Tait, Mr. R. Jenkins and Miss S. Benton.

The Committee asks to be re-appointed with a further grant.

Prehistoric Sites in Egypt.—*Report of Committee* (Prof. J. L. Myres, *Chairman*; Mr. H. J. E. Peake, *Secretary*; Mr. H. Balfour) *appointed to co-operate with Miss Caton-Thompson in her researches in prehistoric sites in the Western Desert of Egypt.*

The subject of enquiry is the prehistory of the Oases of the Libyan Desert, which is practically unknown. The work undertaken in 1931 is in continuation of Miss Caton-Thompson's earlier researches in the Nile Valley and the Faiyum; it aims at unravelling the complicated story of the origins of Egyptian civilisation; and by combining geological and archæological investigation over a wide area will eventually form a basis for the correlation of changes of climate in Europe and Central Africa.

A concession to excavate sites in the Oasis of Kharga for three years is held by the Royal Anthropological Institute, which through its Predynastic Research Committee and with generous help from other sources has raised a sum of over £1,014 towards the cost of these explorations. Other principal contributors are the Percy Sladen Trustees, the Stewart Research Fund of Newnham College, Cambridge, the Worts Fund of Cambridge University, the Society of Antiquaries, the Royal Geographical Society, and various British and foreign museums. Miss E. W. Gardner, the geologist of the expedition, holds a Research Fellowship at Lady Margaret Hall, Oxford.

Miss Caton-Thompson had paid a short visit to Kharga in December 1928, and was therefore able to plan this season's work in advance, and without loss of time on arrival in the Oasis. To trace contacts between the Nile Valley and Kharga, the party used camel transport and traversed 125 miles of the Libyan Plateau on foot. The travertines of the Wadi Samhûd, along the route of the railway, yielded numerous fossils, to be correlated with those of the Oasis. Wind-worn palæoliths were met right across, Neolithic connexions with the Faiyum were found about 20 km. west of the Nile Valley scarp, with characteristic 'Faiyum B' implements and 55 km. further on, more 'side-blow' flakes, Badarian arrowheads, and ostrich egg chips. Roman watering stations marked the whole course of the ancient caravan route. Close to the Kharga scarp a remarkable flint chipping area covers many miles of desert ground: types ranged from Mousterian to post-palæolithic, with 'side-blow' flakes, chipped flint axes, and microliths. The source of all this flint was identified, a tabular band in the plateau limestone, dotted with stone-built shelters. The travertine of the scarp yielded well preserved plant-remains.

On the Oasis floor no evidence was found of a lake. Mousterian and neolithic implements are common. Curious hummocks of silt are due mainly to localised overflow from springs. One of these was dissected, and consists of alternating clays, loams, and pure white sands, containing Mousterian and Aterian implements, and offering an exceptional prospect of resolving the stratigraphical succession of the Stone Age industries, and contributing to the history of climate.

At the end of the season, through the generous co-operation of the Hon. Lady Bailey, D.B.E., an aerial survey was made of the whole Oasis and its surroundings; an important series of air-photographs was obtained, and a flying visit was also made to the Faiyum on the way back to Cairo.

A preliminary report of this season's work, with photographs, is published in *Man* 1931, 91.

As it is intended to resume operations next season, the Committee asks to be reappointed, with a further grant.

Kent's Cavern.—*Report of Committee* (Sir A. Keith, *Chairman*; Prof. J. L. Myres, *Secretary*; Mr. M. C. Burkitt, Dr. R. V. Favell, Mr. G. A. Garfitt, Miss D. A. E. Garrod, Prof. W. J. Sollas) *appointed to co-operate with the Torquay Antiquarian Society in investigating Kent's Cavern.*

The following report has been received from the excavators, Mr. F. Beynon and Mr. Arthur H. Ogilvie:—

'The work of excavation in Kent's Cavern was resumed in October 1930, and was carried on, with the usual holiday intervals, until May 1931. At the southern end of the trench opened up last year along the western wall of the Wolf's Cave and the Sloping Chamber a shaft has been sunk to a depth of 16 ft. below the old line of the upper or Granular Stalagmite floor, with the object of locating the Middle or Crystalline Stalagmite floor, and passing through that into the underlying stratum of Staddon Grit referred to by Pengelly as the Breccia, and discovered by him in other parts of the Cavern also. By the end of the season the following section had been exposed:

Unstratified Cave Earth	4 ft.
Impersistent layer of broken (?Crystalline) Stalagmite	0 ft. 6 in.
Unstratified, incoherent Staddon Grit	8 ft.

It will be understood that the balance of 3 ft. 6 in. required to make up the total of 16 ft. referred to above had previously been taken away by Pengelly and MacEnery.

'There does not seem to be any certainty that the impersistent layer of Stalagmite represents the true Middle or Crystalline Stalagmite floor, associated with a fauna of Cave Bear; but this is probably the case, as it occupies the same position between the Cave Earth and the Staddon Grit. This latter is here, as in some other chambers, quite loose and entirely unbrecciated, and, as is always the case when it occurs in its incoherent form, it is almost barren of fauna; whereas the true breccia contained a plentiful fauna of Cave Bear, with occasional Cave-Lion, Fox, and Deer. This distinction is worthy of notice by readers who may refer to Pengelly's Reports to the British Association, as this excavator uses the term Breccia to cover both the brecciated and the incoherent, the prolific and the barren deposits, without distinction, and turns from the one to the other without noting any change in the deposits or in the levels.

'The animal remains recovered this season numbered some 220, of which 200 came from a disturbed deposit of Cave Earth. About twenty came from the Staddon Grit, and of these all but two teeth of Cave-Bear belong to the fauna of the Cave Earth. It therefore seems possible that the impersistency of the thin layer of Stalagmite in part separating the two strata had permitted a mixture of the upper portion of the Grit with the Cave Earth, and that this had probably occurred at the time when the layer of stalagmite was broken up—very possibly by floods introducing the earliest deposits of Cave Earth.

'No human remains or artefacts have been found this season.'

The Committee has also received from Mr. H. C. Dowie, after consultation on the spot with Prof. A. S. Barnes, the suggestion that 'although we have represented at Kent's Cavern the Chelles, Le moustier, Middle and Upper Aurignac and Solutré, and also something that we may call Final Magdalenian, yet the number of flints found, over some twenty years of digging, is extraordinarily small. Finds might be abundant if we could examine the original or Palæolithic talus running down to the valley from the entrances. This work might best be started from the inside at the level of one of the Low Level Entrances, and not from the outer edge of the Tip. At La Cave in the Dordogne, the cave itself delivered very few flints, but a trench about four feet deep down the talus in front of the cave yielded 22,000 flints.'

The Committee asks to be re-appointed, with a further grant, and authority to raise funds from other sources.

Derbyshire Caves.—*Interim Report of Committee* (Mr. M. C. Burkitt, *Chairman*; Mr. G. A. Garfitt, *Secretary*; Mr. A. Leslie Armstrong, Prof. P. G. H. Boswell, Dr. R. V. Favell, Prof. H. J. Fleure, Miss D. A. E. Garrod, Dr. A. C. Haddon, Dr. J. Wilfrid Jackson, Dr. L. S. Palmer, Prof. F. G. Parsons, Mr. H. J. E. Peake) *appointed to co-operate with a Committee of the Royal Anthropological Institute in the Exploration of Caves in the Derbyshire District.*

Creswell.—Mr. A. Leslie Armstrong, F.S.A., reports that since the presentation of the last report, his work in the Pin Hole Cave has been steadily carried on and continues to yield valuable scientific results. The excavation of the large inner chamber is now in progress at a depth of 13 feet beneath the floor level prior to excavation, and the work in the passage on the eastern side of the chamber has been advanced to a distance of 20 feet from its entrance. The actual occupation of this passage by man ceased during middle Mousterian times and it appears to have been almost entirely filled by debris and the deposit partially sealed by crystalline stalagmite during the cold period which separated the middle and upper Mousterian zones. Near the entrance to the passage a number of charred and split animal bones and a typical side-scraper, of flint, were recovered from the shallow layer of cave earth lying upon the stalagmitic layer. These are believed to represent intrusions during the upper Mousterian occupation of the main chamber. Tools of bone, quartzite and flint have been recovered from the main deposit of the passage, of middle Mousterian age, and also two further examples of hyæna phalanges perforated for suspension, similar to those found in the main chamber in 1930. In addition to these, two

perforated phalanges of an unusual type have been found, the perforation having been executed longitudinally through the shaft of the phalanx from the particular surfaces; a type of amulet which appears to be previously unrecorded in a Mousterian deposit. The middle Mousterian occupation zone, both in the main chamber and the passage, has proved unusually rich in fauna, particularly in remains of Cave Lion, amongst which half a large lower jaw, with complete dentition, is noteworthy.

Other animal remains found of special interest are three skulls of cave hyæna, one of which is complete with lower jaw and full dentition, and the lower jaw of a young mammoth with full dentition on one side. Thirty-six species of birds have been identified, and a large number of skulls and jaws of small mammals collected, but not yet fully identified.

The excavation of the inner chamber and passage has confirmed the conclusions arrived at during the examination of the previous sections of the cave and has yielded important evidence relative to fluctuations of climate during the last Ice Age.

Cales Dale.—A preliminary examination has been made of a cave in Cales Dale which appears to contain an undisturbed deposit of considerable depth which may repay examination. Evidence of occupation in late Neolithic, or Bronze Age, time was obtained near the surface and also of a Pleistocene fauna in the lower levels.

The Committee desires to make application for a further grant of one hundred pounds for work during the coming year.

Archæological and Ethnological Researches in Crete.—*Report of Committee* (Prof. J. L. Myres, *Chairman*; Mr. L. Dudley Buxton, *Secretary*; Dr. W. L. H. Duckworth, Sir A. Evans, Dr. F. C. Shrubsall).

In the years 1905 and 1909 Mr. C. H. Hawes collected for this committee a series of physical measurements of ancient and modern Cretans; and a first report on these measurements was published in the Report of the Sheffield Meeting of the British Association in 1910. After this Mr. Hawes settled in the United States, taking these records with him for further study. But he has been hindered by other engagements, and has now returned the records to the custody of the secretary to the committee, at the Department of Human Anatomy at Oxford, for further examination. The committee thanks Dr. Marett, Rector of Exeter College, Oxford, who was in America last year, for undertaking the safe transport of these valuable documents to Oxford.

Distribution of Bronze Age Implements.—*Report of Committee* (Prof. J. L. Myres, *Chairman*; Mr. H. J. E. Peake, *Secretary*; Mr. A. Leslie Armstrong, Mr. H. Balfour, Prof. T. H. Bryce, Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Mr. O. G. S. Crawford, Prof. H. J. Fleure, Dr. Cyril Fox, Mr. G. A. Garfitt).

During this season, Mrs. E. Michell-Clarke has been employed to draw and measure the remaining metal objects of the Bronze Age in the British Museum. In this she has made considerable progress, but on June 30 there remained about twenty-four hoards to be done as well as a few swords. Owing to illness, Mrs. Michell-Clarke will be unable to resume her work until October, and it seems likely that six months will be required to complete the collections at the British Museum, and a sum of about £25 in excess of the balance in hand.

A few bronze implements have been discovered during the year, and some, hitherto unknown, have appeared at sales. These have been drawn on the cards by Miss L. Chitty.

The Committee wishes to be reappointed with its unexpended balance, a further grant of £25, and authority to raise funds from other sources.

Tabgah Caves, Galilee.—*Report of Committee* (Sir ARTHUR KEITH, *Chairman*; Prof. J. L. MYRES, *Secretary*; Miss D. A. E. GARROD) *appointed to complete the excavation of the prehistoric deposit in the Tabgah caves in Galilee, under the supervision of the British School of Archæology in Jerusalem.*

AFTER the discovery of the 'Galilee Skull' in the Zuttiyeh Cave at Tabgah in 1925, excavation was suspended, and the cave was enclosed by the Department of Antiquities. But in 1930 it was decided, in view of local complaints, to throw open the cave for use by shepherds as aforetime. Through the courtesy of the Department, however, an interval was conceded in which to complete the clearance of the palæolithic deposit in which the skull was found. This was done in the spring of 1931 by Mr. G. Turville Petre, a student of the British School of Archæology in Jerusalem, who had taken part in the excavation of 1925. But the remainder of the deposit was not large, and though implements were found, no further human remains were recovered. A full report will appear in the *Quarterly Statement* of the Palestine Exploration Fund.

Mr. Turville Petre subsequently took part in excavation of similar caves near Athlît, for the British School of Archæology in Jerusalem, and the Committee authorised him to apply to this work any balance which might remain after completing the necessary work at Tabgah.

Vocational Tests.—*Report of Committee* (Dr. C. S. MYERS, *Chairman*; Dr. G. H. MILES, *Secretary*; Prof. C. BURT, Mr. F. M. EARLE, Dr. L. C. WYNN JONES, Prof. T. H. PEAR, Prof. C. SPEARMAN).

BEFORE presenting a report of the work for the year 1930–1931 a short survey of that done in the preceding years is given. During the year 1925–1926 a very comprehensive survey of the work being conducted in various parts of the world was made. A questionnaire was sent out; the replies received were very carefully analysed; and the results were presented at the Oxford meeting, together with thirty-five exhibits of material in the form of tests and literature which had been received from the various agencies and organisations concerned with vocational guidance and selection. A grant was given for the purpose of analysing in the coming year the mass of material so obtained.

During the year 1926–1927 attention was concentrated mainly on a survey of intelligence tests which had been sent in by these organisations. It was found that the intelligence tests evidently only dealt with a very limited range of mental activities and that there was need for a wide range in the type of test. Further, it seemed necessary that some form of standardisation as to the instructions which are given at the beginning of each test should be attempted. The instructions given before a test evidently prepare the candidate for a certain set or attitude towards the test; and if the tests are to be comparable it is necessary that the instructions should be uniform; otherwise different mental attitudes are produced on different occasions.

During the next year, 1927–1928, attention was concentrated on the analysis of tests for vocational fitness and for this purpose further inquiries were sent out to laboratories and industrial organisations which were giving special attention to the matter of vocational tests. In all, thirty-five additional replies were received. These, together with the material previously collected, gave a wide field for further inquiries. In addition certain notable characteristics emerged, one of the most important being that the follow-up methods used varied very considerably in different firms and in different laboratories. Furthermore the criteria used as to the effectiveness or otherwise of vocational tests seemed to give great scope for further research. As regards the tests themselves, the summary showed that these were very varied in character and that many quite different types of tests were often used to test out what appeared to be fundamentally the same psychological aptitude or ability. The tests did not in general measure any unitary ability; thus it seemed essential to find out what they really did test, and this line of action naturally tended towards an analysis of the tests themselves.

It was, therefore, decided that during the year 1928–1929 it would be best to concentrate on an intensive examination of typical tests of one particular type of

activity. The type of test chosen was that used for assembly work in the hope that in the course of this analysis facts that would be useful for the work of vocational guidance would emerge, and further that the debatable relation between general intelligence, mechanical aptitude and manual dexterity might be cleared up. There appeared to be also the possibility of obtaining more information which might improve the present restricted view on motor ability.

The report for that year describes the tests carried out and the data obtained. The method of work followed was to select a rather complicated assembly operation and to find out how proficiency at certain specifically chosen tests was related to special ability at the operation. The result indicated that routine assembly operations have more in common than laboratory motor tests have in the past suggested. It was also evident that the lack of correlation usually found in motor tests may be due to the lack of reliability in the tests themselves. This indicated a need for closer analysis and more systematic classification of motor tests. The experimental work done suggested that practice and fatigue might have an effect on such motor tests. It also appeared that the ‘ abilities ’ unduly influenced the score made at these ‘ motor ’ tests of routine assembling operations.

This preliminary work seemed encouraging and during the year 1929–1930 the work was extended to a larger number of subjects. The assembly work was carefully graded in complexity and to those who were doing the assembly tests, additional tests of general ability and mechanical aptitude were applied. In all, 47 adults, 70 boys, 59 normal girls, and 22 retarded girls were tested and examined. The effects of practice were also studied. One important point to determine was the degree of reliability of these tests ; and the conclusion arrived at was that a single trial of an assembly test was highly unsatisfactory as a measure of ability. It was found, further, that in a number of trials the sum of the best, next best, etc., trials are almost as reliable as the total of all scores. It was found that the influence of random errors on reliability was far greater than that exerted by practice or fatigue during the trial. It was found that reliability depended on the number of repetitions of the operation constituting the measure rather than on the length of time required for the operation itself. Further practice had no marked influence on reliability.

The results reported in 1930 were the outcome of a partial analysis of the data obtained. A more thorough-going statistical analysis was necessary, and during 1930–1931 this has been done with useful results. Statistical analysis has shown very definitely that there exists (*a*) a general factor, (*b*) a mechanical factor associated with mechanical insight and invention, and (*c*) a motor factor associated with the manipulative side of assembly work. Interesting facts concerning the growth of practice have been obtained. The practice curve for adults has been examined. In all operations there are wide individual differences which tend to dominate as practice continues. The course of progress in practice appears to be the same for most individuals, namely, a steep initial phase which seems associated with the cognitive processes involved in learning how to manipulate material, followed by a much less steep secondary phase which is associated with ‘ motor ’ learning—that is with acquiring speed in the manipulative processes after the method of manipulation has become clear. The two phases are more distinct in the more complex assembling operations and tend to disappear in the simpler operations. There are well-marked differences in the smoothness or daily variability shown in the curve. These are especially noticeable in the more complex operations. Similar features appear in the curves of practice of schoolboys : generally the curves are more irregular, indicating less rapid and less continuous improvement than is shown in the adult curves.

The results as a whole suggest that where speed is the chief factor, the boys are of superior ability initially and show the greater improvability ; whereas where success depends on the accurate adjustment of one part to another, the adults are superior in both respects. Where the operations call for a large measure of both speed and accuracy, the boys may do better at first but the adults show the greater improvability. Furthermore, although considerable improvement is shown by most subjects during the practice period, there is a fairly close correspondence between the rank orders on the first day and those on the last.

The question of transfer has also been examined, both with ‘ adult ’ groups and with elementary schoolboys. The number of subjects in the practised group has been increased during the year. The adults now number 33, with 17 ‘ controls ’ ; the schoolboys number 38, with 32 ‘ controls.’ The practisers were divided into

groups, each of which practised a different operation. Nowhere is there any evidence of practice in one operation bringing about improvement in another. The combined results of the experiment suggest that this conclusion will hold over a wide range of operations.

The relationship between ability, improvability, variability and intelligence have also been studied from the data collected in the previous year and from experiments carried out during this year. There is a general, and in some cases a marked inverse relationship between 'initial' ability and improvability both when the latter is measured as an absolute gain, and when measured as a percentage of initial ability. A similar, though less marked, relationship holds between total ability and these two measures of improvability adopted. The relation of improvability and variability is less uniform. Improvability, both 'absolute' and 'percentage,' tends to an inverse relation with intelligence. Comparison of each of these functions with initial ability suggests that this relation is due to the handicap to further progress imposed by the greater initial ability which accompanies superior intelligence, rather than to any hindering effect of the intelligence itself.

Since the last report the mental operations involved in mechanical assembling and routine assembling have been analysed in great detail. In routine assembly work there appear to be in the main two types of activity: type I leads to a knowledge of the general spatial character of the movements to be imparted to the material; type II leads to knowledge concerning the way to bring about the requisite movements. These types have been further subdivided and analysed into their constituent mental processes. A detailed description of the whole research has been prepared for publication. It comprises 14 chapters together with an appendix of 35 tables, 65 curves and other figures.

In the last report suggestions were put forward (i) that the 'motor' factor discovered in the data may be associated with the complexity of the operation and may tend to disappear when very simple movements are concerned; and (ii) that although no transfer effect was observable when the subjects were left to their own devices, it might be possible to give general instructions or training, the effects of which would be transferable to other operations. An investigation into the first of these questions has been begun. Sixty boys constituting the two top classes of a London elementary school have been given two groups of tests (1) consisting of complex tests such as assembly and stripping containers, (2) simpler tests such as stripping screws, screwing up a turnbuckle, threading large beads, etc., and they have also been given a test of general intelligence. The correlation between the twelve tests given has been determined. Time has not yet permitted of the statistical analysis necessary to determine how far the observable differences of these figures are indicative of different factors and how far they are due to chance fluctuations. The following indications are, however, given in the cumulative coefficients. There is little, if any, correlation between the motor tests and the general intelligence tests. The coefficients range from ·09 to ·25. There is usually a definite, and in some cases, a high positive intercorrelation between the motor tests, ranging from ·07 to ·75. Thus, further evidence is provided of at least one routine factor in these tests. The average intercorrelation of those tests classed as complex is ·41, that of the simpler tests is ·32, that of the simpler and the complex is ·27. If the differences between these averages should prove of statistical significance, they would indicate the presence of a second factor (or factors) such as 'speed,' in the simpler tests, bringing about the observed closer agreement among themselves. And if, as seems more probable, the difference between ·27 and ·32 above should appear in the light of statistical criteria to be *not* significant, we must suppose that the intercorrelation of the simpler tests is brought about by the same factor as that which has already been observed in the more complex test, and that as they become simpler they become less saturated with this factor. Further work is contemplated in which still simpler tests are included and where further classification in the light of the subjective analysis which has since been completed may be employed.

Report for 1927–1928,

Together with Additional Information collected during 1930–1931.

At the last meeting of Section J a report on intelligence tests was presented. This report brought out the need for more uniformity in the method of giving instructions, and emphasised the limited range of mental activity tested by the majority of the test groups in common use. One prominent feature in the replies received was the

almost complete omission of any attempt to use objective tests for the estimate of temperament and character traits.

This year inquiries have been circulated to laboratory departments of Psychology and to individual firms in order to obtain information concerning tests for vocational fitness and to ascertain what research is being carried out in this direction.

Altogether twenty-five replies have been received from these sources; a list is appended (Appendix A). In these replies there is a conspicuous absence of tests which attempt to obtain a measure of temperament and character.

One noticeable feature of the work that is being done is the fact that a systematic follow-up of the results of the tests is being made in a considerable number of cases. The deviations from the diagnosis of the selection tests and subsequent performance in factory or workshop are frequently attributed on good evidence to the interference of temperamental factors, and this fact again emphasises the need for research in this direction.

As more systematic following-up is being carried out the need for reliable criteria of success has become very noticeable. The extent to which following-up has been done and the value of the methods of comparison that have been used varies greatly. In some cases the groups of subjects who have been tested have subsequently been under the observation of several supervisors or foremen, and have not been in sufficiently large groups to admit of a statistical evaluation of the success of the selection tests. A crude comparison with other groups of workers who have not previously been selected by tests is the only possible means of assessing the practical value of the tests. In such cases the evidence is dependent on the subjective estimate of the supervisor as to whether on the whole the performance of the workers is better than those not specially selected by tests. At the other extreme it has been possible to keep accurate records of the work of the persons selected, as shown by their progress in the training school and in the workshop. Where this has been done the evidence is certainly in favour of the efficacy of selection tests. Notable instances of this are seen in the accounts of tests in the firms of Carl Zeiss, Jena; Bergmann-Elektricitäts-Werke, Berlin; Philips' Glowlampworks, Eindhoven; The Vitkovice Mines, Steel and Iron Works Corporation, Vitkovice, Czechoslovakia; Linke-Hofmann-Werke, Breslau; A. Borsig, Berlin; Fried. Krupp, Essen; Osram, Berlin; Société des Transports en commun de la Région Parisienne, Paris; Ganz & Comp.-Danubius, Budapest; Fried. Krupp, Kiel; Verband Berliner Metall-Industrieller, Berlin; and in the follow-up of results of tests for motormen by Viteles in Philadelphia.

Various criteria are used to estimate the value of the tests. The assessment of teachers in the training school and foremen or supervisors in the workshop is perhaps the most obvious method, though it is sometimes difficult to obtain reliable and impartial ranking as to performance from those who are in close contact with the workers owing to the intrusion of personal factors which in some cases markedly and admittedly modify judgment.

Ranking a group by piece-rate earnings is not entirely satisfactory, as it seldom happens that the distribution of work is uniform, and there is often considerable inequality in the allocation of piece-rates. Variation in the supply of materials and in the tools used affects individual output and also introduces errors that cannot be satisfactorily adjusted. When records are kept for a number of years, trade fluctuations make accurate comparison of work done in one period with that done in another extremely difficult. Attempts are also being made to use statistics of labour turnover as a measure of the success of selection tests. A reduction in labour turnover is assumed as an indication of better selection, but this measure is subject to variations due to changes in general economic conditions, so that comparison between different periods is often unreliable. Where, for instance, there is a general improvement in trade, the tendency for the worker to take advantage of this may show itself in an increased labour turnover.

Records of accidents and the amount of spoiled work produced are in some cases used as a measure of the efficiency of selection, but here again the issue is often not clear. Thus the members of a group of motormen selected by the help of suitable tests will inevitably be operating on different routes where the accident liability varies considerably. In many accidents, too, it is often extremely difficult to determine which of the parties involved was responsible.

Some of the attempts that have been made to prove the value of selection tests by the usual statistical methods are, it is feared, open to grave objections. The

numbers are often small and the variables in the criteria used are not capable of estimation. It is quite likely in such cases that the subjective estimate of a supervisor summed up in a general statement that the candidates passed by the selection tests are on the whole better than those not so selected is a better proof by direct observation than the evidence of correlation coefficients obtained by the statistical treatment of data of doubtful reliability and complicated by many undeterminable variables.

The tests are very varied in character, and in many instances they closely imitate some part of the work which the candidate will subsequently have to perform. Though this type of test may give good results in practice, it does not do much towards advancing knowledge of the fundamental factors involved in any particular occupation, and cannot be applied to the more general problems of vocational guidance. An example of a group of tests of this type is given in Appendix B.

In some cases tests of a more analytic type are given. These tests are sometimes weighted or the candidate's ability may be rated according to the 'profile' obtained. An example of this type is given in Appendix C.

Though the forms of the tests varied, an analysis of those in general use shows a number of well-marked and apparently related types. Thus many of the tests require from the candidate a sequence of similar reactions, and the total time taken is a summation of the individual reactions. An example of this type is the pegging test. The initial stimulus is given in the instructions, and the candidate repeats a series of similar reactions till the test is completed.

In Appendix D some examples are given of tests which appear to measure similar activities. These types are sometimes used in widely differing occupations.

Some of the tests collected have been in use in various branches of industry for a considerable time, and their utility has in many cases been reasonably well demonstrated in careful follow-up of records.

It would be interesting to ascertain whether these tests, which apparently measure similar abilities, would, if applied to a sufficiently large group, show a significant intercorrelation.

A further analysis of these tests and the abilities which they measure might conceivably reveal some tests of high reliability, which are of a diagnostic value that would be useful in the vocational guidance of children entering industrial occupations.

It is requested that a grant of £20 be allocated towards carrying out the work of further analysis of the tests collected, the application of selected tests to a group of subjects, and the subsequent treatment of results.

APPENDIX A.

Bergmann-Elektricitäts-Werke, Berlin.
A. Borsig, Berlin-Tegel.
Bureau of Psychotechnical Research, Warsaw.
Commissariat du Peuple des Postes et des Telegraphes, Moscow.
Erste Donau-Dampfschiffahrts-Gesellschaft, Vienna.
Ganz & Co., Budapest.
Gelsenkirchener Bergwerks, Gelsenkirchen.
Handworkers Preparatory School, Oslo, Norway.
Institut für Industrielle Psychotechnik, Charlottenburg.
Fried. Krupp, Germania-Werft, Kiel.
Fried. Krupp, Essen.
Linke-Hofmann-Werke, Breslau.
Osram Kommanditges, Berlin.
Office Intercommunal pour l'Orientation Professionelle, Brussels.
Psychotechnical Department of Central Labour Office, Copenhagen.
Psychotechnisches Institut, Zurich.
Philips' Glowlamp Works, Eindhoven, Holland.
Psychotechnisches Laboratorium der Technischen Hochschule, Stuttgart.
Stork Machine Factory, Hengolo, Holland.
State Railways, Prague.
Swiss Federal Railways, Berne.
Société des Transports en commun de la Région Parisienne, Paris.
Verband Berliner Metall-Industrieller, Berlin.
Vitkovice Mines, Steel & Iron Works Corporation, Vitkovice, Czechoslovakia.
K. Zeiss, Jena.

APPENDIX B.

Examples of Tests.

Technische Hochschule, Stuttgart.

Occupation : Dressmakers.

QUALITY.	DESCRIPTION OF TEST.
Memory and appreciation of form and colour.	(i) Sheet divided into 15 squares, each containing a simple figure in colour, exposed for 30 seconds, after which candidates reproduce as many of the figures as they can remember in the corresponding squares of a second sheet. Figures reproduced in black and white, colours being written against each.
	(ii) Dress shape to be 'assembled' from wooden pieces.
Taste and creative ability.	(i) Small jointed dolls to be dressed to candidate's taste in garments cut out of muslin and pinned together.
	(ii) Original dress-border to be designed.
	(iii) Sketch of dress shown, and from box of pieces candidate must select pieces with which to trim dress and indicate how they should be used.
	(iv) Sketches of six collars and six sleeves to be paired according to suitability.
Draughtsmanship and grasp of design.	(i) Symmetrical figure, of which one side is missing, to be completed.
	(ii) Repetitive patterns to be continued.
Finger dexterity.	(i) Irregular shape to be cut out of paper with scissors.
	(ii) Sheets of thin paper to be rolled up with as little creasing as possible.
	(iii) Bootlaces to be threaded through holes in cardboard.
	(iv) Threads to be plaited.
	(v) Simple designs woven on wide-meshed canvas to be copied in different colours.
Tactual discrimination.	(i) Ten squares of cloth to be sorted according to thickness of weave.
Colour discrimination.	(i) Out of 50 knots of silk those have to be selected which exactly match shades of cloth.
	(ii) Thirty sheets of paper of different intensities of grey to be arranged in order.
Estimation of length and distance.	(i) Oblique lines to be numbered in order of length.
	(ii) A weighted string fastened at one end to be adjusted at the other end until it is exactly parallel to a fixed string.

APPENDIX B—*continued.*

Occupation: Watchmakers.

QUALITY.	DESCRIPTION OF TEST.
Manual dexterity and sense of form.	(i) Five pieces of wire, 20 mm. long and 1 mm. thick have to be bent into certain shapes, then hammered straight again.
Estimation of weight.	(i) Long box in which 15 blocks of wood are placed side by side like drawers in a chest of drawers. Blocks vary in weight between 20 and 60 gm. Handles are attached by which blocks can be raised but not pulled right out. Adjacent blocks are raised, two by two, one in each hand, and the candidate has to say in each case whether the right or left hand block is the heavier.
Steadiness of hand.	Board with 60 holes, 20 measuring 5 mm., 20 3 mm. and 20 2 mm. across. Mounted needle 1 mm. in diameter has to be placed in all holes in turn without touching sides, first in the candidate's own time, and then in time with a metronome set at 60. Contact with sides automatically recorded.
Bi-manual work.	Candidate has to file with right hand, keeping pressure steady, and file in one vertical plane, while with the left hand he moves a shutter up and down as quickly as possible. Number of movements of shutter automatically recorded, while electric contacts register if the pressure falls below or rises above a certain range, or if the file leaves the correct vertical plane.
Cleanliness.	A metal sheet with about 20 irregularly shaped openings is held in the smoke of a lamp. The surface is rubbed clean, and the candidate cleans inside the openings.
Dryness of hands.	Candidate holds an old photographic plate between his hands for two minutes, and the amount of staining is investigated.

APPENDIX C.

EXAMPLES OF TESTS.

A. BORSIG, BERLIN-TEGEL.

Occupation: Engineering apprentices.

QUALITY.	DESCRIPTION OF TEST.
Discrimination of distance.	(i) Drawing and bisecting angles by eye.
Estimation of weight.	(i) Blocks to be arranged in order of weight.
General intelligence.	(i) Sums.
	(ii) Précis.
	(iii) Groups of three associated words read out. First word of each group repeated, candidate supplies last words.

APPENDIX C—*continued.*

QUALITY.	DESCRIPTION OF TEST.
Technical intelligence.	(i) Technical diagrams shown, and candidates asked—"What will happen if so-and-so?" "What must we do to make so-and-so happen?"
	(ii) Parts of apparatus described, candidate to say how they interact.
	(iii) Superfluous lines to be erased from a diagram.
	(iv) Simple diagrams to be reproduced.
Form relations.	(i) Solid lines with irregularly shaped gaps at intervals, and pieces, from among which the candidate must select those which fill gaps.
	(ii) Three cubes are shown, piled together in various ways, and the candidate has to draw them as they would appear from various angles.
Motor ability.	(i) Bending wire into appointed shapes.
	(ii) Assembly.

APPENDIX D.

EXAMPLES OF TESTS.

1. REPEATED REACTIONS OF THE SAME KIND.

(i) Placing three dots as quickly as possible in each of a number of circles.
(Mechanical ability test: University of Minnesota.)

(ii) Plaiting threads as quickly as possible.
(Dressmakers' test: Technische Hochschule, Stuttgart.)

(iii) Threading a series of small discs, each perforated with six holes, on to a group of four mounted needles.
(Hand-workers: Siemens-Schückert.)

2. VISUAL TESTS (ACCURATE ESTIMATION OF SIZE, DISTANCE, ETC.).

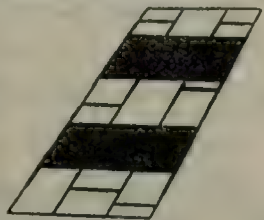
(i) Bisecting lines and angles by eye.
(Used almost universally.)

(ii) Adjusting a string fastened at one end until it appears parallel to a fixed string.
(Dressmakers: Technische Hochschule, Stuttgart.)

(iii) Placing discs
(*a*) As nearly as possible in the centre of large circles.
(Labellers in chemical works: Technische Hochschule, Darmstadt.)
(*b*) As exactly as possible on circles of the same size drawn on paper, in time with a metronome.
(Manual ability test: N.I.I.P.)

3. ESTIMATION OF VELOCITY OF MOVEMENT.

(i) Three parallel horizontal strips have black lines across them at irregular intervals. Each strip can move at various speeds either towards or away from the candidate. Certain sections of the strip are covered.



1. The three strips are set moving, and the candidate has to watch for the moments when three black lines come together to form a single line; when this happens he presses a brake which stops the apparatus momentarily.
2. The candidate has to estimate the moment at which the marks will come together, under the covered section, and use the brakes as before.

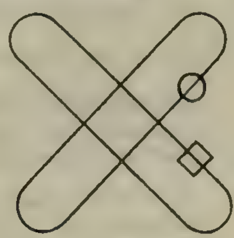
(Engine drivers: Laboratory of the University of Hamburg.)

Appendix D—*continued.*

(ii) A pointer rotates on a dial, half of which is covered. On the cover is a mark, and the candidate reacts when, in his opinion, the pointer is directly beneath the mark.

(Engine drivers, train staff, shunters and pointsmen : Dresden.)

(iii) Two moving bands intersect as shown. The circle represents a pedestrian and the square a car. Both bands are driven by an apparatus with brake and accelerator, the 'pedestrian' band being controlled by the examiner and the



other by the subject. The latter's task is to move the 'car' round as quickly as possible without colliding with the pedestrian.

(Street railways, Leipzig.)

(iv) Two pointers move with different but constant velocities along a horizontal scale, and the candidate has to estimate at what point on the scale they will meet (if they are moving towards one another) or at what point one will overtake the other (if they are moving in the same direction).

(Tram drivers, Société des Transports en commun de la Région Parisienne).

4. Divided Attention.

(i) Ten variously coloured electric lamps are arranged in a row, and each has a plug-hole, similarly coloured, corresponding to it; below is a second row of lamps of the same colours but in a different order, each having a coloured plug corresponding to it. The lamps in the top row are lit, two or three at a time; and, when this happens, the candidate has to put plugs from the bottom row into the correspondingly coloured holes in the top row; this extinguishes the top lamps and switches on the bottom ones. At the beginning of the test a number is announced, and the candidate is told that he will be required to repeat it at the end of the test. During the test twelve other numbers are announced; one of these is written on a card and placed before the candidate, and he is required to react when he hears it.

(Telephone girls: Technische Hochschule, Charlottenburg.)

(ii) A short story is placed before the candidate, who has ten minutes in which to cross out the a's, e's and n's in the text. From time to time figures are exposed—a 'warning' being given in each case by the lighting of a lamp—and at the end of ten minutes the candidate writes down as many of the figures and as much of the story as he can remember.

(Printers and Compositors: Pedagogisch-Psychologisches Institut, Munich.)

(iii) The candidate has to react to light and sound stimuli by pressing appropriate keys, and to reverse, when necessary, three sand-glasses which run out in ½, 1 and 3 minutes respectively. A clock which runs down every 2 minutes has also to be kept wound—the candidate having to judge whether the clock has stopped by the tick alone, the face being invisible.

(Chauffeurs: Amsterdam.)

5. Accuracy and Steadiness.

(i) An iron bar with a taper from 5 to 10 mm. has to be pulled up as far as possible through a 12 mm. hole without touching the sides. Contact with the sides completes a circuit which causes the bar to be attracted and held fast by an electro-magnet.

(Foundry workers: Hanover.)

(ii) The candidate is given a sheet of paper on which the terminal points of the lines of a figure are indicated by dots. A pencil is held vertically in a clamp, and the

APPENDIX D—*continued.*

candidate has to move the paper beneath the pencil so as to draw the figure. (Apprentices: Siemens-Schückert.)

(iii) A pointer has to be drawn along slits of varying size and width, if possible without touching the edges. (Apprentices: Technische Hochschule, Charlottenburg.)

6. REACTIONS UNDER DISTRACTION.

(i) Bright lights are flashed while the candidate is doing a divided attention test. Mistakes due to distraction affect his total score. (Air Force: Germany.)

(ii) Candidate manipulates the controls of a dummy cab in accordance with a code of signals. A light flashes, a loud noise is made, or the platform on which the subject is standing gives way beneath him. Signals are then given calling for rapid movement of the controls, and the subject's 'nerve' is judged by his response to these. (Tram drivers: Greater Berlin Tramway Co.)

(iii) Candidate reacts with hand or foot movements to light or sound stimuli, while seated in front of a sheet on which appear cinematograph pictures without explanation or coherence. (Tram drivers: Société des Transports en commun de la Région Parisienne.)

ADDITIONAL INFORMATION REGARDING VOCATIONAL TESTS COLLECTED DURING YEAR 1930–1931.

Letters were sent to those who had replied to the questionnaire circulated during 1927–1928 asking for information concerning the development of new tests, methods of assessing temperament and character, and any additional work that had been done concerning occupational analysis.

The replies have been few and the references are mainly to published work on these two subjects. The following is a brief summary.

Charlottenburg Technische Hochschule, Berlin.

Mainly tests for motor-car drivers. These, with other tests, are described in 'Lehrbuch der Industriellen Psychotechnik,' Berlin, 1930, Vol. 1, and further work is described in Vol. 2, which will come out in the autumn. In Vol. 1 questions of temperament and character are discussed with special reference to work done for the German State Railways and the German Army.

Stuttgart.—Work in this area is under the supervision of Dr. F. Giese, and the new developments have been in the direction of

(*a*) New tests for saleswomen and needlewomen.

(*b*) Temperament and character determination through observation of test work. Results are published in 'Beobachtung in der Arbeitsprobe'—Giese and Cordemann.

(*c*) Monographs.
Russel, 'Berufspsychologische Untersuchungen in der Edelmetallindustrie' (Engravers, silversmiths, &c.) in the Press. Will appear in the Zeitschrift für angewandte Psychologie.
Becker, 'Psychophysik der Schweissereitechnik' (involving occupation analysis of welders) Dissertation Stuttgart 1931. Will be published this year.

Barcelona Instituto de Orientacion Professional.

An account of work done is included in an article on 'L'Exploration de l'Affectivité.'

Geneva. Université de Genève. Institut J. J. Rousseau.

M. Walther has done extended work on 'Cinq Tests d'Habilité manuelle.' Madame Hélène Antipoff has published work on 'Evolution des Fonctions psyco-motrices' and tests of mechanical intelligence are in course of preparation.

Milwaukee Vocational School. U.S.A.

Trade-finding classes in seventy different vocations have been established. These shops simulate closely the materials, machines and conditions existing on the job. They give good preliminary experience to the thirteen thousand young men and women attending.

Psychological or aptitude tests are not used for prognostic work.

Temperament and character are evaluated through periodic rating by the teachers and from student council.

Occupational analyses of thirty-nine representative occupations have been made.

Posters, pictures and lectures, moving pictures and monographs are used to supplement the advice of the two hundred and seventy instructors of the school.

Michigan University.

A bibliography of occupational information has been compiled for high school and college students. Ninety occupations have been considered. These discuss the vocation with respect to possibility as a life career, and in many cases give details of the work involved in the occupation.

A study of the earnings of women in business and in the professions has also been published. The monograph analyses the present occupational status of the groups and emphasises the earnings in various occupations and types of work.

Ohio State University.

The main occupational inquiries and work in progress concern

(i) The problem of highway safety. A monograph—'Psychological Principles in Automotive Driving' has been published, and contains accounts of new tests that have been developed and some preliminary results.

(ii) Preliminary work on a social intelligence test has found this successful in differentiating college women who were prominent in extra-curricular activities. These tests are, however, still in the research stage.

New York. The Children's Welfare Foundation.—This body has not done any additional work since 1927, and has used entirely tests and information available from other sources.

Mycorrhiza in Relation to Forestry.—*Report of Committee* (Mr. F. T. Brooks, *Chairman*; Dr. M. C. Rayner, *Secretary*; Mr. W. H. Guillebaud). *Drawn up by the Secretary.*

The grant was allocated as a contribution toward the current expenses of an investigation, the purpose and scope of which were explained in a report presented last year.

In this first report the main objects of research were thus summarised :—

1. To ascertain whether the formation of normal mycorrhiza may fairly be regarded as a causative factor in the healthy growth of young trees, it being already assumed that it is an invariable concomitant of such growth.

2. To determine whether the absence of mycorrhiza associated with unsatisfactory growth is related to :—

(*a*) absence of the mycorrhiza-forming fungi appropriate to the trees ; or

(*b*) the existence of soil conditions inimical to the formation of functional mycorrhiza of a beneficial type.

3. To ascertain whether it is practicable to supply deficiencies or ameliorate existing soil conditions by experimental treatment of nursery stock or in other ways.

The present position of the work may be dealt with conveniently under these heads.

1. *Mycorrhiza as a causative factor.*

Evidence bearing on this problem has been sought in relation to three species of pine, sown or planted in the Wareham and Ringwood plantations of the Forestry Commission, viz., Scots Pine (*P. silvestris*), Maritime Pine (*P. pinaster*), and Corsican Pine (*P. laricio*) by means of humus treatments.

In the case of the first-named species, humus regarded as suitable for inoculation purposes was available in the spring of 1930. For corresponding treatments of Corsican Pine and Maritime Pine it has been considered advisable to examine mycorrhizas and collect humus containing them in habitats native to the species. For this purpose the forests of Aïtone, Valdoniello and Vizzavona in Corsica were visited in the spring of 1931. From the nature of the case, evidence derived from the use of material collected in Corsican forests is not yet available. That already obtained from field experiments of Scots Pine supports the view that the formation of normal mycorrhiza is a causative factor of great importance to the growth of young trees.

The nature of this evidence may be judged from the fact that the relative dry weights of sample seedlings on treated and untreated plots were in the ratio 2 : 1 after eight months, and the disparity now, after fifteen months' growth, is much greater. In respect to vigour of growth, root and shoot development, and colour of foliage, comparison of the growing plants provides irresistible evidence of the beneficial effects associated with induced mycorrhiza formation in seed plots of Scots Pine on the soils in question. This conclusion is confirmed by the results of pot experiments.

Corresponding experiments on other types of soil are desirable and are in progress, but results have been greatly delayed owing to the hampering conditions under which the work has been carried on.

In the case of Maritime Pine, humus treatments have likewise given positive results of a less marked kind. Using humus from a young natural stand in this country, the dry weight ratio of sample seedlings in treated and untreated plots was 3·7 : 1 at eight months, and is roughly 3 : 2 at fifteen months. The experiments have been tentative in character, and a full report of the reaction shown by seedlings of this pine to humus inoculation with material known to contain mycelium of specific root-fungi must await the results of experiments now in progress.

Treatments of Corsican Pine with humus from stands in this country have not given noteworthy results, the average dry weight ratio of treated and untreated seedlings after fifteen months being practically unity. As was anticipated, therefore, a full report on this pine must await the results of field experiments with soil known to contain the mycelium of mycorrhizal fungi appropriate to the species.

The conclusion that normal mycorrhiza formation is a factor that may become of critical importance to the growth of young trees under certain soil conditions is reinforced by observations on seedlings and young plants of Scots Pine from various stations where growth was unsatisfactory.

In general, these showed that poor root development, stunted shoot growth and yellowing of the foliage were associated with abnormalities in respect to mycorrhiza formation and parasitisation of the roots amply sufficient to account for the appearance and behaviour of the plants.

Evidence has been sought also in the case of Oak.

A preliminary survey was made of the mycorrhizal condition of the roots in young plantations of Oak in various parts of the Forest of Dean. It has been ascertained that certain types of mycorrhiza are associated with vigorous growth of the young trees, a phenomenon strikingly manifested in the case of plantations showing marked inequalities of growth in different parts. The observations are significant, although there is at present no proof of a causal connection between the two sets of phenomena.

The promising results obtained from this preliminary survey point to the necessity for more complete observations in this and other areas and more precise knowledge as to the identity of the fungi concerned in the mycorrhiza of Oak.

2 (*a*). *Presence or Absence of the appropriate Fungi.*

Mycorrhiza-forming fungi specific to Scots Pine are present in the Wareham and Ringwood areas. Observations on the hymenomycete flora of this district are at present fragmentary, and it would be rash to assume that optimum conditions in respect to the proximity of mycelium of suitable fungus species exist throughout the plantations.

The two other pines under observation present a rather different problem. Both are exotics, and in neither case is there yet available exact information as to the identity of any root fungi associated with them under natural conditions.

In Maritime Pine, mycorrhiza of a normal type is freely formed under certain ascertained conditions in the Ringwood area, although its formation is inhibited in seedlings or transplants elsewhere there and at Wareham. Experiments now in progress should throw light upon the causes of these discrepancies and indicate whether conditions in respect to this formal condition of mycorrhiza formation can be improved.

In Corsican Pine observations lead to a somewhat different conclusion. Comparative observations on roots from established stands in this country show that mycorrhiza of a normal type is formed, but suggest that disturbing conditions are not infrequently present. There is now available for the first time a standard of comparison provided by mycorrhiza collected in Corsican forests. It is possible that absence of appropriate fungus species or of a suitable grouping of such may be wholly or in part responsible for the observed abnormalities.

At Wareham no improvement in growth or increase in dry weight has yet resulted from treatments with humus from pure stands in this country, thus providing a striking contrast with the case of Maritime Pine. The reaction of seedlings and young trees of Corsican Pine to treatments with Corsican humus known to contain the mycelium of specific mycorrhiza formers should permit of final conclusions.

In view of the existence of healthy well-grown stands of Corsican Pine in this country it must be emphasised that the matter under consideration is likely to be of critical importance only in relation to the establishment of new plantations either by seed or planting on poor and unfavourable soils such as these under observation at Wareham and Ringwood.

2 (*b*). *Existence of inimical soil conditions.*

In the Wareham area and locally at Ringwood the existence of soil conditions inimical to mycorrhiza formation may be regarded as established. In the pines under consideration—notably in the two exotic species—absence of mycorrhiza is associated with an unhealthy and often moribund condition of the younger roots.

Many of the morbid characteristics of seedlings in the field are reproduced in pot cultures, and the value of the latter as supplementary to field observations emphasises the need for a suitable shelter house for growing them under more satisfactory conditions.

In the case of Scots Pine, field plots have yielded evidence that, whereas mycorrhiza formation can be freely induced experimentally, soil factors inimical to its maintenance and free functioning are present in the Wareham soil.

With regard to the nature of these factors, there is little doubt that bad aeration from several causes contributes directly and indirectly to the unhealthy condition of the roots. Improvement in this respect in pot cultures in the case of Maritime Pine alleviates the symptoms and permits a relatively free development of mycorrhiza.

This activity is not maintained, and other factors are undoubtedly concerned. In this connection great importance is attached to observations on the effects produced by supplying phosphates in various forms, alone, or in conjunction with humus treatments.

In respect to soil factors generally, field observations confirm the results of laboratory experiments with pure cultures and provide independent proof of the extreme sensitiveness of tree mycorrhiza to environmental conditions.

At Wareham there has been observed a remarkable correlation between vigorous spontaneous growth of the trees in localised areas and the appearance of the sporophores of certain fungi known to be mycorrhiza-formers. This supplies confirmatory evidence for the view held that the formation of normal and functional mycorrhiza of a type beneficial to the trees is not a resultant merely of the presence of appropriate mycelium in proximity to the roots, but is a complex phenomenon controlled by soil factors intimately affecting the physiology of both trees and fungi, and ultimately determining the behaviour of the mycelium and the reaction of the roots when attacked.

3. *Possibility of effecting improvement.*

Certain definite conclusions have been reached respecting the inimical causes operating in the Wareham plantations. It is believed that experiments on the lines of these now in progress will demonstrate conclusively that the unfavourable conditions undoubtedly present can be ameliorated by methods practicable under field conditions and not prohibitive in cost.

Consequently it should be possible to prevent the loss due to death or more or less complete stagnation of growth with irregular and sporadic emergence from this condition of check, at present observable.

Comparative observations on a number of geographical races of *P. silvestris* in respect to differences of growth and mycorrhiza-forming capacity are being continued. A full account of the work will be published in due course.

This report offers an opportunity to acknowledge once more the help and co-operation received from officers of the Forestry Commission in this country and from Mr. Forbes, late Director of Forestry in the Irish Free State.

The two grants allocated to this research committee by the British Association have facilitated the accumulation of evidence indicating that a clearer understanding of the relation of mycorrhiza formation to tree growth may be of importance in helping to solve the practical problems presented by the afforestation of uncongenial soils in Great Britain.

With their help it has been possible to lay the foundations of a comprehensive scheme of intensive research.

The grant of £40 provided by the British Association has been fully expended.

It was hoped that it would be unnecessary to ask for the reappointment of the Committee, as it was understood that the Forestry Commissioners were willing to make a grant which would enable the work to be carried on and extended. Owing, however, to the present financial position, the question of a grant will have to be reconsidered by the Commissioners, and it is very unlikely that this will be made. The Committee ask for reappointment and for the renewal of a grant of £40 to enable essential observations on existing plots to be carried out.

Transplant Experiments.—*Report of Committee* (Sir ARTHUR HILL, *Chairman*; Dr. W. B. TURRILL, *Secretary*; Prof. F. W. OLIVER, Dr. E. J. SALISBURY, Prof. A. G. TANSLEY).

A GRANT of £25 was received from the British Association on October 8, 1930. Of this sum, £22 4*s*. 3*d*. has been spent on labour and materials in a manner sanctioned by the Committee. Receipts of expenditure for this amount are enclosed herewith. The sum of £2 15*s*. 9*d*. has been held in reserve to pay for additional soil which is to be moved in during the summer or early autumn. It is requested that this be approved.

A full report of the results of the experiments obtained up to 1929 was published in the *Journal of Ecology*, xviii, 352 (1930). The Committee have recommended that a second report be published to include the results to the end of this year, 1931.

Upland Bog Waters.—*Report of Committee* (Prof. J. H. PRIESTLEY' *Chairman*; Mr. A. MALINS SMITH, *Secretary*; Dr. B. M. GRIFFITHS' Dr. E. K. RIDEAL) *appointed to carry out the Chemical Analysis o, Upland Bog Waters.*

IN last year's report it was possible to correlate the total bulk of algal growth in Miles Rough bog, Bradford, with the nitrate content of the water. The water had not then been analysed in the winter, when the algal growth is at times very vigorous. The analyses of the present year have filled this gap, having been made on November 13, 1930, and March 2, 1931. They show that water rich in nitrate enters the bog during the winter months, so rich indeed, that only a small proportion of its nitrate is used either by cryptogamic or phanerogamic vegetation (the latter being, of course, in its resting period). This is shown by the analyses :—

November 13, 1930 . .	Entry	1.79	parts N_2O_5	per	100,000
	Exit	1.62	,,	,,	,,
March 2, 1931 . . .	Entry	1.95	,,	,,	,,
	Exit	1.64	,,	,,	,,

The supply of this water, rich in nitrate, in large quantity at a time when there is little competition from phanerogamic vegetation, is probably the main reason why the amount of alga is often so large in winter, showing itself, as a rule, almost independent of temperature fluctuations.

Detailed analyses having now been obtained for the principal seasons of the year, the Committee does not seek re-appointment.

Educational Training for Overseas Life.—*Report of Committee appointed to consider the Educational Training of Boys and Girls in Secondary Schools for Overseas Life* (Sir JOHN RUSSELL, *Chairman*; Mr. C. E. BROWNE, *Secretary*; Major A. G. CHURCH, Mr. H. W. COUSINS, Mr. T. S. DYMOND, Dr. J. VARGAS EYRE, Sir R. A. GREGORY, Mr. O. H. LATTER, Miss E. H. MCLEAN, Mr. G. W. OLIVE, Miss GLADYS POTT, Mr. A. A. SOMERVILLE, Dr. G. K. SUTHERLAND, Miss MARIAN WEBB, Mrs. GORDON WILSON).

The Committee have issued five Reports, since their appointment in 1923, *on the subject of the Educational Training of Boys and Girls in Secondary Schools for Life Overseas. The present and final Report deals mainly with Overseas Careers for pupils from Secondary Schools.*

CONTENTS.

I.

INTRODUCTORY STATEMENT.

1. *The Pupils.*—The earlier work of the Committee dealt with the training of secondary school pupils who, by inclination or through force of circumstances, seemed more likely to find their fullest scope in the Empire overseas than in the crowded offices and workshops at home.[1] Some of these young people are of the adventurous type attracted by the idea of life in the open air in a new country, and anxious to emulate the success—sometimes the remarkable success—of many who had gone out before them with fewer advantages than they had. Others, while not particularly adventurous, desire at any rate to be self-supporting, and realise that there are in ordinary circumstances many more openings for honest hard-working young people overseas than at home. All these need something which a purely academic school curriculum fails to impart or to develop, viz., a power of handiness and adaptableness, ability to accomplish some definite tangible task, and, what is even more important, a speedy recognition of the task that lies before one and the best way of getting on with it. The ordinary secondary school curriculum which presupposes a university life for some and an office life for the rest does not appeal to them; it fails to draw out their inherent possibilities and gives the impression, sometimes wholly unjustified, that they are less intelligent than their more pliant fellows who pass through the school with impressive 'credits' and 'distinction.' Yet the history of the overseas lands shows that young people of this type have played a valuable part in the develop-

[1] See previous Reports of Committee on Educational Training for Overseas Life issued in 1924, 1925, 1927 and 1929.

ment of the countries to which they have gone, and their services to their fellows have often been far greater than those of the people who stayed at home.

The Committee have endeavoured to ascertain how these conditions could be improved. They have given serious attention to the possibility of developing workshop practice and other manual work related to outdoor occupations as a part of the school curriculum so as to bring out and develop any natural bias young people have in these directions and make them more fitted for overseas life through an early training. The view has been strongly held and expressed that experience in practical things, and tuition with a more practical objective, will encourage and develop qualities which would otherwise remain latent in those very pupils most likely to emigrate. It has been with a desire to give such pupils a more equal chance of developing such natural bent towards practical things that the Committee have examined the whole question with the object of finding where the difficulties exist and how they may be overcome.

The Committee have discussed in previous Reports the problem presented by these pupils, and the methods by which the difficulties have been satisfactorily overcome in a number of schools. They are here, in their final Report, concerned with the ways in which the teacher may further help the pupil who has taken advantage of the training facilities offered and has equipped himself so that he can take up life overseas.

During the past year the Dominions most favoured by migrants from this country have been in financial difficulties, and in consequence they have relaxed their efforts to attract newcomers. The Committee find also a growing disinclination on the part of some of the people in this country to be attracted. These factors have introduced complications into their inquiries, but they firmly believe that the Dominions will speedily recover, and they hope that a more enlightened attitude will soon develop here. So they have continued their work in the expectation that the present troubles are but temporary.

2. *School Curricula.*—The fact that one type of school curriculum does not suit all pupils is, of course, well recognised, as also is the further fact that many to whom it is unsuited deserve adequate attention on grounds of public interest, quite apart from any consideration of their own or their parents' rights. Recognition of the facts, however, has frequently been insufficient to compel action; in many secondary schools the greater need has been conformity to certain examination standards, and the Committee early realised that the only chance of introducing any effective alternative curriculum was to induce examining bodies to accept alternative subjects.[2] Here again difficulties arose—teachers had all been trained to teach the accepted subjects, and these alone; any new subject would require new teachers, who could not readily be found, or it would need to be undertaken by existing teachers without adequate training, so that the subject might degenerate into the so-called 'soft option.'

It was gratifying to find, however, that a number of secondary and public schools, recognising the intrinsic value of a curriculum which outsteps the limits of a scholastic career and serves to develop the sense of achievement in the normal child, had boldly given an important place to the more practical subjects, with entirely satisfactory results. Not only did benefit accrue to the pupils on whose special behalf the subjects had been introduced, but also to others for whom they were not strictly necessary. Full details of the curricula of some of these schools are given in the earlier Reports, showing exactly how the time is allotted between the practical and the written work, and what appliances are needed in order to attain the best results. Evidence shows that the school curriculum can, in practice, be widened to fit the child for the life of action and achievement, and need not be restricted to the production of the scholar or the clerk.

3. *Obstacles to Migration.*—In the course of their earlier inquiries the Committee found that there is need for some definitely organised system of bringing to the notice of parents and pupils the openings for young people occurring in the Overseas Empire, and for giving parents the information they need before deciding to what part of the Empire their sons and daughters might go. The national significance of this should be brought to the notice of education authorities and to schoolmasters and schoolmistresses in the hope that they will take steps to ensure that all their pupils have

[2] See Report for 1925, page 4.

these overseas opportunities placed before them, and that they will consider seriously such changes in the schools under their authority as may help the better to fit their pupils to take advantage of such opportunities.

They found further that little advantage was taken of the openings which till recently were both numerous and good. Various causes contribute to this undesirable result. In spite of the great efforts of the Overseas and Home Governments, and of the numerous British organisations, there is still a widespread ignorance of the conditions of life and the possibilities of life overseas. The Committee have during this year endeavoured to ascertain what steps are normally taken either by the schools or by the Dominion representatives in this country to keep the scholars informed of the possibilities. The present Report shows the results of their inquiry.

Broadly speaking, no systematic steps are taken. Some of the overseas authorities express their readiness to give lectures, demonstrations and advice, but they have gained the impression that they are not welcomed in secondary schools. As against this it is stated that the lectures and lantern slides are not suitable for secondary schools, and may indeed do more harm than good. One of the Committee's recommendations deals with this matter. Among some of those associated with schools there is a fear—which is disconcertingly widely spread—that the spirit of adventure is gone and that the boys from elementary and secondary schools are afraid to go overseas, or alternatively that their mothers are afraid; another general impression is that parents expect the education authority to find for their children good posts near home, and therefore will not consent to their sons and daughters proceeding overseas even if the posts near home do not eventuate, or prove to be only of the temporary, blind-alley type. Again, the lack of desire to proceed overseas is attributed to the blunting of the spur to incentive by the widespread social services of this country. In so far as these causes may be operative, their effect is to put upon the education authorities and those charged with the after-care of children the responsibility of seeing that the young people do not miss the chances presented by the Overseas Empire.

4. *Publicity and Information.*—The Committee have devoted considerable attention to the ways in which parents and scholars might be kept more fully informed about the advantages of overseas life. The subject is dealt with in more detail later (page 307), the activities of the different organisations in this respect being described, and some recommendations made for making the distribution of information more effective.

Important memoranda have been received from the Oversea Settlement Department on the general situation and on opportunities for boys overseas, from the Society for Oversea Settlement of British Women on the opportunities for girls overseas, from the Canadian and New Zealand Governments' Immigration Departments, from the Boy Scouts Association, which has an encouraging record of successful work, from the Director of the Imperial Institute, from several Local Education Authorities, Employment Bureaux, and other organisations interested in migration, including the Church of England Council of Empire Settlement, the Young Men's Christian Association and Local Voluntary Migration Committees. Equally valuable was the information supplied by the various Agencies for the Crown Colonies.

The Committee take this opportunity of expressing their indebtedness to all who have so readily and with considerable trouble responded to the inquiries addressed to them, and tender them their best thanks for the valuable contributions they have made to this Report.

In these communications there stands out clearly the importance of the personal touch; the direct knowledge of and faith in the country to which the young people will proceed, and faith in the young people that they will make good. It is a hopeful sign that visits are being interchanged between headmasters and headmistresses of this country and Canada, and even between school children in the two countries. This cannot fail to dispel any lingering ignorance regarding the life and the possibilities of life overseas. A number of informants have emphasised the great desirability of giving the young migrant a further period of training in the country to which he proposes to proceed. In this way he acquires easily special knowledge and experience which might otherwise be obtained only at the cost of much time and comfort.

Boys of good type able to take up a professional career with hope of success are advised to complete their training in the appropriate overseas university. They will from there step into professional vacancies more readily than from home.

In Appendices a list is given of :—

I. The organisations from which information can be obtained.
II. Sources from which financial assistance can be obtained.
III. Types of Secondary Schools and Colleges in Great Britain—a memorandum by Sir Richard Gregory.

The work of the Committee this year has been somewhat overshadowed by the economic difficulties of most of the Overseas Dominions, and this has caused them to reduce their efforts in regard to propaganda for new migrants. So far as these conditions are concerned, the Committee believe the difficulty will pass by the time the present generation of pupils, whom alone they are considering, are old enough to migrate. Even at the present time the objection of the overseas countries is, in the main, to our unemployed, and not to our fresh young people.

Moreover, no difficulties are placed in the way of young people of promise proceeding from a secondary school in Britain to a university or technical school in the Dominions overseas. For such the openings are numerous. Administrative, professional, industrial, and commercial posts are open to the graduates of such institutions, and are likely to offer greater opportunities for advancement than in Britain owing to the more rapid increase in population in the newer countries. It is well known that the Colonial Office often has difficulty in getting for service the kind of pupils they need. At most secondary schools scholarships are offered to enable pupils to enter universities or technical institutions in Britain. The Committee urge the Board of Education and school authorities to make such scholarships tenable also at British institutions overseas.[3]

5. *The Dominions and their Resources.*—Finally, it is well that all should recognise clearly the important changes that have taken place in the Dominions themselves as to their attitude towards immigration. Whereas until recently they were prepared to admit practically all types of immigrants for settlement on the land, not only from Great Britain but from Europe generally, the change in the economic conditions and the large amount of unemployment and even distress on the farm lands everywhere compel them to close their doors to the old emigrant type. They are exercising a greater vigilance over the type of settler introduced; they will no longer put up with the 'remittance' man, and the 'family disgrace.' In self-protection they must exclude all who are likely to become a charge on their resources. Naturally they do not want any but the best Great Britain can send. Boys and girls of good physique and character, with a willingness to work and with grit to overcome difficulties, are welcomed; these will always find plenty of scope for their abilities and ample reward for their labours. This aspect of the question calls for readjustment of the ordinary British idea of migration—it must no longer be thought to be a means of getting rid of the 'waster' or 'dud,' but should be considered an avenue of great opportunity, enterprise, and adventure open only to those with the best credentials. The great natural resources of these lands, particularly in Canada, the enormous potential wealth of vast areas still untouched offer in the near future opportunities unsurpassed elsewhere in the world.

6. *Summary.*—The results of the present inquiry may be summarised as follows :—

(*a*) From the statistics obtained during the last six years it has been clearly shown that some cause—which is variously said to be a want of enterprise, a lack of knowledge, or some adverse influence—has held back the youth of this country from as free a participation in the openings offered for overseas life as might reasonably have been anticipated.

(*b*) With the exception of the group of committees in the North-East of England the various Local Migration Committees have taken little, if any, interest in the secondary school boy as such, but have been mainly concerned with those from elementary schools.

(*c*) The County and Borough Education Committees make little or no attempt to direct the attention of the secondary schools in their charge to the possibilities of overseas life for many of their pupils, but simply leave such matters to the initiative of individual schools. The senior scholarships they offer are tenable at universities in Britain only, Government grants not being available otherwise.

[3] See note at end of Report (page 314).

(*d*) The schools throughout the country seem for the most part to be sadly lacking in this initiative, although many of them are circularised by one or other of the societies interested in Empire Settlement.

(*e*) Many headmasters have expressed themselves in favour of a more practical training for a large number of their boys, and believe that many of these boys would do better on the land in one of the overseas Dominions than shut up in an office in England.

(*f*) It is the absence of any organisation for keeping the schools in touch with the activities of the societies and the facilities they offer, and for maintaining a personal contact with representatives of the Dominions, that probably accounts for the general indifference of the schools, and for the small number of boys from secondary schools who take advantage of settlement schemes to go overseas.

(*g*) One of the chief obstacles usually quoted to a free movement amongst boys as they leave school towards the adoption of a career overseas lies in the opposition, or indifference, of their parents. In all probability this, too, is largely due to ignorance of the conditions under which young settlers are now started and looked after by responsible persons.

Memoranda have been included showing: (1) what careers are open for boys who have the courage and enterprise to strike out into the new life of the Empire; (2) what careers are open to girls; (3) details of the various organisations concerned in migration; (4) methods whereby

(*a*) the school, the pupils and their parents may be brought to a knowledge of the many avenues for overseas settlement, and the many facilities that are offered of assistance—financial and social—to the young migrant;

(*b*) the parent, or boy, or girl can learn how to set about the matter, where to apply for information, whom to consult, how to get the information easily and completely.

The Committee believe that a closer co-operation between the various societies and institutions interested is essential in order to bring about a better co-ordination of effort in all concerned; further, the Committee believe that the main object of the co-ordination should be a more definite and systematic effort to make known to the older pupils of our secondary schools, and to their parents, the numerous opportunities that exist in our overseas Empire for boys and girls of enterprise and character, and to draw their attention to the many societies that offer assistance in establishing the beginner in his, or her, new career.

II.

OPENINGS FOR BOYS OVERSEAS.

1. *Temporary Drawbacks.*—It is of the utmost importance to remember that, in dealing with the problem of migration, the primary consideration must be the economic condition of the country of settlement and its capacity to absorb new population. Immigration to a country in which the supply of labour exceeds the demand at the time, or in which staple products are failing to find adequate markets, is likely to lead not only to the failure and disappointment of the settler, but, by adding to existing difficulties, to engender opposition to immigration in principle. The effects of this might be felt even when economic conditions were again favourable.

At present, economic conditions in Canada and Australia and, to a lesser extent, in New Zealand and South Africa, are such as to lead the authorities to the conclusion that for the time being it would be unwise to encourage the settlement in those Dominions of the people of this country unless they have definite prospects awaiting them overseas.

This, however, in no way lessens the desirability of bringing the possibility of an overseas career before both teachers and pupils, inasmuch as the present depression in the Dominions will probably have given place to prosperity by the time the majority of the pupils have reached an age suitable for migration. It is, nevertheless, important to recognise that the present time is, generally speaking, unfavourable to successful settlement overseas, and that the reasons for this should be understood. Since also the well-being of the settler is dependent on the prosperity of the country in which he settles, and since the prosperity of the Dominions is very largely bound up with the problem of securing a market in the United

Kingdom for their products, emphasis might well be laid on the fact that the extension of the demand in this country for Empire products is one of the surest means of creating further openings for our settlers overseas.

2. *Government Assistance.*—In 1921 the Home and Overseas Governments adopted a policy of encouraging and assisting migration and settlement within the Empire, and in 1922 the Home Government passed the Empire Settlement Act. This Act enables the Government of the United Kingdom to provide not more than half the cost of settling suitable persons from this country in the Dominions, and, *inter alia*, to encourage and assist juvenile migration. In co-operation with the respective Governments, and with voluntary migration organisations, the Oversea Settlement Department has initiated schemes for the provision of free or very much reduced ocean passages, and for the training and settlement of boys on the land, and it has made arrangements whereby the welfare of the juvenile settlers is supervised until they are able to run alone. In the case of boys the aim of the Government throughout has been not only to see that they are placed in suitable agricultural employment overseas, but to give the boy of thrift and initiative a good chance of attaining independence. In pursuance of this policy special schemes (which are described below) are in force in Canada and have until recently been operating in Australia. These offer generous help to the right type of boy.

3. *Voluntary Agencies.*—The Act of 1922 resulted in a marked increase in the volume and scope of the work of voluntary societies for promoting Empire settlement, as well as extension of that work on much broader lines than hitherto. Amongst the more important of these societies may be mentioned the 1820 Memorial Settlers Association, the Church of England Council of Empire Settlement, the Big Brother Movement, the Boy Scouts Association, and the District Migration Committees. Each of these bodies has the approval and support of the Government, and all of them either have branches in the Dominions or are affiliated to kindred societies which have been set up overseas.

The work of these organisations falls into two main divisions—(1) Recruiting, (2) Placement and after-care.

Recruiting, of course, is carried out by the home branch of the organisation by means of publicity among schools, local representatives in different parts of the country, lectures, etc. All applicants are first considered by the society concerned, and if Government assistance is required towards their passage they are then submitted for approval to the representative in this country of the respective Dominion Governments.

The more important of the functions of voluntary societies are, however, placement and after-care, *i.e.* first finding suitable openings in the Dominions for the settlers recruited over here by the home organisation, and then supervising their welfare during the first—sometimes difficult—years in the new country. In the Dominions, as at home, the voluntary organisations work with the approval and, in a certain measure, under the control of the overseas Governments, and they can obviously stand in a relationship to the settler which no Government could readily fill. In short, their fundamental purpose is to supply to the settler that personal care and guidance which is so important a factor in successful settlement, from the time when he first thinks of an oversea career to the time when, having served his apprenticeship overseas, he is able to stand alone.

The majority of voluntary organisations have, of course, been more or less seriously affected by the present restriction of assisted migration, and by the fact that conditions overseas are generally unfavourable. For Australia, recruiting has had to be stopped altogether, the societies concerned confining themselves to the 'after-care' of those already settled in the Commonwealth. There is still, however, a limited flow of boys to Canada, New Zealand, and South Africa, and it is hoped that at no very distant day circumstances in all the Dominions will have improved sufficiently to justify a resumption by voluntary organisations of their full activities.

4. *Land Settlement Schemes.*—This Report is concerned only with those schemes which are suitable for secondary school boys. It is not easy, however, to draw a line of clear demarcation. Certain schemes, it is true, call for the possession of capital by the settler, and might for that reason be considered as being specially applicable to the secondary or 'public' school boy. Others, on the contrary, are open equally to boys with or without capital, but are none the less suitable for boys

from secondary schools. Particulars are, therefore, given in the following paragraphs of all schemes now in operation which it is thought would be appropriately included in this report.

(*a*) CANADA.

1. *Government Schemes.*—The British and Canadian Governments have agreed in suitable cases to make loans up to £500 to enable boys, when they have attained 21 to 25 years of age, to purchase and equip farms of their own. The loans are to be repaid within twenty years with 5 per cent. interest, and they will be made to youths (*a*) who went out to Canada under a Government assisted passage scheme ; (*b*) who were between 14 and 20 years old on arrival in Canada, and (*c*) who have proved their thriftiness by saving £100 by the time they are ready to start farming on their own account.

In connexion with this scheme the Provincial Governments of Nova Scotia, New Brunswick, Ontario, and Manitoba have established farm reception centres, to which boys go direct from the United Kingdom and from which they are placed in suitable farm employment under the regular supervision of Provincial Government representatives. In making loans, preference is given to those who have passed through the Provincial Governments' farm reception centres and have afterwards gained practical experience by working as wage earners. Boys who receive loans will be helped and advised in the choice of their farm and in the purchase of stock and equipment by Provincial Government advisers who will give all the assistance in their power to ensure the young farmers' success.

2. *Macdonald Agricultural College.*—Twenty-five boys of 17 to 22 years of age from public and secondary schools are selected each year for two years' training in agriculture at a reduced fee at Macdonald College, Ste. Anne de Bellevue (incorporated with the McGill University, Montreal). The total expenses for the 22 weeks in-College course are approximately £51 per annum. This includes tuition, board and lodging, laboratory charges, medical fees and student subscriptions. In addition, each student is required to deposit £20 to cover incidental expenses. From April 1 to October 31 in each of the two years the students do ordinary farm work on farms selected by the College authorities. For this they receive a small wage, with board and lodging. A superintendent appointed by the College is in touch with the boys during this period. For the remaining five months the students undergo theoretical and practical training in the College, to which a farm of 500 acres is attached. During the second year facilities for specialising are provided. Further particulars may be obtained from Major W. H. Hayward, The Canadian Pacific Railway Company, 62–65 Charing Cross, S.W. 1.

3. *Christ's Hospital Canadian Scholarship Scheme.*—Arrangements have been in force during the past two years between the British Government and the Governors of Christ's Hospital, Horsham, by which a certain number of Christ's Hospital boys have been awarded scholarships at the rate of £55 per annum, entitling them to two years' free tuition and maintenance at Macdonald Agricultural College. This arrangement is being renewed for 1931, when it is anticipated that eight boys will be awarded scholarships. The scholars travel to the College free of cost if under 18 years of age. On the completion of their training, which should enable them to take the Diploma in Agriculture, they will be placed in suitable agricultural employment ; or should they wish to make an independent start, they will be eligible for consideration under the scheme for the provision of Government loans towards the purchase of farms (see above under Macdonald College Scheme).

4. *Guelph Agricultural College, Ontario, Training Scheme.*—Fifty places at this College, which is affiliated to Toronto University, are available each year for youths from this country, aged 17 years and upwards, at a fee of approximately £50 per annum. The course of tuition is for either two or four years, and is designed to give youths of good education a thorough grounding in the theory and practice of Canadian farming. Students attend College from September to April, and for this period the fees cover the expenses of instruction, maintenance, books, etc. During the spring and summer, employment in Ontario is found for the students with farmers from whom they receive wages and board and lodging while gaining experience of practical farm work.

Full information can be obtained from the Ontario Government Office, 346 Strand London, W.C. 2.

5. *Church of England Council of Empire Settlement.*—This Council was formed some five years ago for the purpose of co-ordinating and extending the migration work already being done by various Church of England societies. In co-operation with the Home and Overseas Governments, the Council has also initiated and carried out schemes of its own for the placement with farmers in the Dominions of boys from the United Kingdom, with a view to their taking up farms of their own in due course. Some of these include schemes especially intended for boys of the secondary school type. Among the latter is one now in operation in Canada for assisting youths of 18 to 24 years of age, who will eventually have not less than £300 of capital, first to obtain the necessary practical knowledge of farming, and then to make an independent start. The youths selected by the Council's officials proceed either to the Council's headquarters at Toronto, or to the reception hostels maintained in Alberta and Saskatchewan by the Canadian organisation of the Church of England Council for Social Service. They are then placed with carefully selected farmers for at least 18 months' training (during which they receive a small wage plus their board and lodging), reports on their progress being sent in at regular intervals. When the youths have gained sufficient practical experience to warrant them in making an independent start, the Council, with the co-operation of the Provincial Government's agricultural service, advises them in the purchase, equipment and working of their farms. During the period of training and settlement the youths are under the direct care of responsible members of the Church of England Council.

Full information can be obtained from the Secretary, 39 Victoria Street, London, S.W. 1.

6. *The Anglo-Canadian Education Committee.*—This Committee, whose head quarters in London are Seymour House, Waterloo Place, S.W. 1, was formed with the object of creating a better understanding and a closer relationship between educationists in Great Britain and Canada. Strong committees exist in both countries. In 1930 the Committee organised a tour in Canada of sixteen headmasters of English public schools in order to visit and inspect the chief Canadian universities. As a result of this visit, which appears to have had an excellent effect, not only on the headmasters themselves but also in Canada, it is hoped that more attention will be focussed on the question of overseas careers for boys and girls, and that there will be a small but steady movement of boys from schools in Great Britain to Canadian universities, through which they will pass to their chosen occupation. It is possible that a scholarship scheme to further this movement may be started later on.

7. *The Fellowship of the Maple Leaf.*—This is essentially a Church of England organisation for the recruitment and equipment of teachers and clergy for service in the Western Provinces of Canada. It is not simply an agency for teachers but founded to promote the ideals and common citizenship of Great Britain and Canada. The workers go out either with grants if already fully trained, or with scholarships to colleges in Canada.

Full particulars can be obtained from the Secretary, Dr. P. J. Andrews, 13 Victoria Street, S.W. 1.

8. *Boy Scouts Association.*—Imperial Headquarters of the Boy Scouts Association have established a Migration Department for assisting and advising scouts and ex-scouts who wish to settle overseas. The Department supplies letters of introduction to Scout Headquarters at ports of call and in the Dominions. A special Nomination Scheme was arranged in 1927 with Headquarters in Australia, and proved a great success.

Through a generous gift of money to Imperial Headquarters Boy Scouts Association by Mr. T. H. Whitehead, and with the help of the Overseas Settlement Department, scholarships are arranged for scouts in the Agricultural Colleges in the Dominions overseas. A number of boys were placed during the year 1930, and a further party of twenty scouts who are to enter the Guelph Agricultural College sailed at the end of April, 1931. Similarly scouts have been placed at an agricultural college in Southern Rhodesia.

The scholarships in question enable a boy who is of reasonable education, of good health and physique, to obtain a two-year course in an agricultural college, all expenses being paid for the boy. The scholars for Canada enter college in the autumn and work during the spring and summer with local farmers for further practical experience and they receive wages and keep. The scholarship scheme is being developed, and there is no doubt that it will play a most important part in the future

with reference to Boy Scout Migration. Several Whitehead Scholars are now in Australia, Canada, S. Rhodesia, and reports that have been received of these are extremely gratifying.

For further particulars apply to the Boy Scouts Association, 25 Buckingham Palace Road, London, S.W. 1.

9. *Hudson Bay Company. Farming and Fur Trading.*—The Company, in association with the Canadian Pacific Railway and the Cunard Steamship Companies, has organised an Overseas Settlement Company which is prepared to find places with farmers in Canada for those ready to begin farming at once ; while for those who have had little or no farming experience the Company is operating a farm at Ridgmont in Bedfordshire known as the Brogborough Park Farm. Young men of 17 years and over are 'tested out' on the farm under a superintendent who has had nearly 20 years' experience in Canada. The course of instruction varies from a minimum of four to a maximum of ten weeks. The land and buildings cover about 250 acres.

The method of using agricultural implements, the general incidence of living and working, the character and care of live stock, hours of work, etc., conform as nearly as may be with Canadian methods and conditions, the intention being that on arrival in Canada the pupil will not find everything entirely strange, nor should the farmer with whom he may be placed find it necessary to teach him the most elementary items of daily work on a farm.

An accepted applicant is not called upon to pay anything for his tuition, board, or lodging, but he must deposit with the Company before he goes to the farm a guarantee that he intends in Canada to proceed to farm work which will be offered him by the Winnipeg office of the Company. The deposit amounts to about £30 to cover his third-class fare out to Winnipeg, and £5 which is returned to him on arrival at this town.

Opportunities occur for employment in the Fur Trading Department for public school boys between the ages of 17 and 20. Approved applicants are sent out to Canada on a five-year contract to some chosen post, in order to gain experience in the elements of the business. Accepted candidates travel to their destination free of cost, and their maintenance allowance is gradually increased from 240 dollars in the first year to 504 dollars in the fifth year with board and lodging.

Promotion is possible up to the higher administrative appointments of the Company—District Managers, Accountants, Inspectors, District Staffs, and the highest post which the service has to offer, Fur Trade Commissioner.

(*b*) AUSTRALIA.

Owing to the economic difficulties which are now being encountered in the Commonwealth all schemes for assisting the settlement there of people from this country have been suspended. Until this suspension took effect there were various schemes in force in the several States of a kind suitable for secondary school boys. For example, in Queensland and Western Australia, the Church of England Council for Empire Settlement were carrying out a scheme with the co-operation of the State Governments for the agricultural training and settlement of youths with capital of not less than £300, on the lines similar to those of the Council's Canadian project referred to above. See also under Boy Scouts Association above.

The Big Brother Movement.—In Victoria the Big Brother Movement, of which the Prince of Wales is Patron, has been doing admirable work in placing carefully chosen boys on the land, each boy being under the guardianship of a responsible Australian citizen (who was constituted his Big Brother) until he was 21 years of age. Since its inception in 1925, the Big Brother Movement has been instrumental in settling over 1,000 boys, many of them from secondary schools, in the Commonwealth.

The Queensland Farm Apprenticeship and other schemes offered excellent opportunities to boys of the right type whether of secondary or primary education. It is hoped that when the present depression in Australia has passed some at least of these schemes will be revived.

(*c*) NEW ZEALAND.

For the past three years, assisted settlement in New Zealand has been proceeding on a limited scale, but is at present in abeyance. Below is a description of a scheme promoted by the Church of England Council of Empire Settlement. It has been in

successful operation until this year (1931). It is hoped that as economic conditions improve the scheme will be resumed, as it affords excellent opportunities to secondary school boys who wish to take up farming. It must be pointed out, however, that considerable capital is needed by those who intend to start farming on their own account, and although generous advances may be obtained in approved cases from the State Advance Department of the New Zealand Government, it would be well that the settler, if he hopes eventually to own his farm, should be able to rely on some hundreds of pounds capital being available to him. There are boys without capital of their own who manage to save enough money to enable them to make a start by means of 'share-farming' and the like, but, generally speaking, some capital is desirable.

1. *Church of England Council of Empire Settlement.*—The Council arranged for the Church of England Immigration Committee in New Zealand to nominate each year a certain number of boys aged 15 to 18 years, preferably of secondary school education, for training on farms in New Zealand. The boys are placed in employment with farmers chosen by the Immigration Committee, and in the selection of farms more importance is attached to good training and a good home than to wages. The boys are under the care and guidance of the Committee, which is prepared to give help and advice to them at all times.

The boys and their parents under this scheme have to sign an undertaking that the boys are under the care and jurisdiction of the Committee for the first two years in the Dominion, and that 50 per cent. of the boy's wages are to be paid direct to the Committee to bank for him. The latter provision is an excellent incentive for further saving towards an independent start. There are also, throughout New Zealand, sub-committees who visit the boys during their training and submit reports at regular intervals to the Central Committee at Auckland. Upon the completion of their training, the boys—being then probably about 21 years of age—may continue in employment as farm workers, engage in share-farming, or, if they are in a position to do so, acquire land for themselves.

Full particulars may be obtained from the Secretary, 39 Victoria Street, London, S.W. 1.

2. *Waitaki Boys' High School, Oamaru.*—A further scheme which is of value for the purpose of introducing boys to the life of the Dominions is that of the Waitaki Boys' High School. A limited number of British boys recommended by the Public Schools Employment Bureau are accepted at this school for one or two years' agricultural training. Boarding and tuition fees are £65 per annum. The School is situated in a rich agricultural and pastoral district of the South Island, and possesses a small experimental farm of its own. Instruction in agricultural subjects, *e.g.* wool-classing, dairy science, agriculture and plot work is given by experts, and several afternoons a week are devoted to practical training on adjoining farms. When their training is completed the boys should have acquired a sound knowledge of agricultural work and conditions in South Island. The Headmaster of Waitaki School has undertaken the responsibility of placing the boys in employment with suitable farmers, keeping in touch with them, and of reporting regularly on their progress and prospects.

Full particulars can be obtained from the Secretary, Public Schools Employment Bureau, 5 Paper Buildings, Temple, E.C. 4.

(*d*) THE UNION OF SOUTH AFRICA.

1820 *Settlers Memorial Association.*—This Association was founded ten years ago to commemorate the British settlers who sailed for South Africa in 1820. Its object is to further the settlement of good British stock on the land in the Union of South Africa. Only settlers with substantial capital are accepted by the Association, which recognises that the use of native labour renders the country unsuitable except to those in a position to become land owners. The Association does not buy or sell land, but, by means of a widespread organisation throughout the Union, is able to give settlers very valuable advice and help in the selection of their land and in its equipment, etc. The Association has the approval and support of the Governments of the United Kingdom and the Union of South Africa.

Provision is also made by the Association for the agricultural settlement in South Africa of schoolboys who are over seventeen and under twenty years of age, with a minimum capital of £1,000, and who have studied at a public school or approved

secondary school. A guarantee must be furnished that the sum of £1,000 will be available for the use of the boy when he is ready to commence farming on his own account.

Boys accepted by the Association receive a rebate of 15 per cent. on their ocean passages. They are met at the port of arrival in South Africa by a representative of the Association, which thereafter assumes full responsibility for their welfare.

Each settler is required to undergo a period of three years' training—one year at the Association's Training Farm and two years with an approved farmer. Tuition is free, and during the two years with a farmer board and lodging is also free; but at the Training Farm there is a charge of £5 a month to cover living expenses. On completion of his period of training the boy is passed out as a settler, and the Association assists him to establish himself upon his property and stands behind him until he ceases to require help. Settlers who prove satisfactory during the training period may, if necessary, be granted financial assistance in the shape of loans either from the Government or from the Association, or from both.

(e) SOUTHERN RHODESIA.

Southern Rhodesia is very suitable for agriculture and ranching, but it does not offer many openings for boys. Farmers usually work their own farms, while all the unskilled labour is performed by natives. On the other hand, given that a boy is prepared (i) to pay for training and spend a year at the Matopos Agricultural College, Buluwayo, (ii) then two or three years with a farmer to gain a knowledge of local conditions and local experience, and (iii) is assured of not less than £1,500 capital at his disposal at the end of his apprenticeship, the country offers big prospects of a successful career. There were some excellent Government land settlement schemes in operation last year, but owing to the economic condition of the country they are in abeyance (1931).

(f) THE COLONIAL EMPIRE.

The Colonial Empire.—The territories[4] constituting the Empire overseas, not comprised in the self-governing Dominions or India, afford many opportunities for young men of good education. The population of these Dependencies is nearly twice that of the self-governing Dominions. Their overseas trade has trebled during the last twenty years. Government staffs have increased from 93,000 in 1909 to 220,000 in 1929; in the latter year as many as 1,078 Government appointments were filled from this country. Of these appointments, 449 were made by the Secretary of State for the Colonies to administrative, educational, medical, agricultural, scientific and numerous other posts. Other appointments were made by the Crown Agents for the Colonies to posts connected with the railways and other public works—engineers, accountants, sanitary inspectors, etc. Outside the Government services, recruits are needed by commercial firms, mining companies and the like, operating in individual Dependencies with head offices in this country, *e.g.* the 54 operating on the Gold Coast and the 22 operating in North Borneo. Many of these firms recruit young men of school-leaving age.

It should be pointed out that while, unlike the Dominions, 90 per cent. of the Colonial Empire lies within the tropics, tropical medicine and hygiene have done much to lessen the effects of the climatic conditions.

[4] The Dependencies administered by the Secretary of State for the Colonies may be classified as follows:

Tropical Africa, {*East*: Kenya, Nyasaland, Tanganyika Territory, Uganda, Somaliland, Zanzibar, and Northern Rhodesia.
West: Nigeria, the Gold Coast, Sierra Leone, and the Gambia.

Eastern Colonies and Protectorates: Ceylon, Hong Kong, the Straits Settlements, and the Malay States.

The West Indian Colonies: Jamaica, Bahamas, Barbados, Windward Islands, Leeward Islands and Trinidad, with British Guiana and British Honduras.

Western Pacific: Fiji, Gilbert and Ellice Islands, British Solomon Islands.

Mediterranean: Cyprus and Gibraltar.

Middle East: Palestine.

Various Islands: Mauritius, Seychelles, Bermuda, Falkland Islands, and St. Helena.

The vacancies in the Colonial (Government) Service for which candidates are usually selected in this country are those for junior officers in the higher grades, posts in the lower grades being usually filled by local candidates. Modern conditions require a higher standard of personal, educational and professional qualifications than formerly. The minimum educational standard is that of the school certificate; but the appointments open to boys of school-leaving age (minimum 19 years) are confined to the customs and constabulary in a few Dependencies. For all other posts, the minimum age is 21½ (23 for public works and railways), and for these a University degree or professional qualification is an advantage and in many cases essential.

For young men who have taken a University degree in one of the biological sciences there is a wide field of opportunity in the Colonial Service. In almost all the tropical Dependencies, agriculture, including stock rearing, is the principal occupation. Some 30 to 40 vacancies occur annually for agricultural officers and specialists in agricultural botany, mycology, entomology, or chemistry, in addition to 20 to 26 vacancies for veterinary and forestry officers. Many of these vacancies are filled by the holders of Colonial agricultural or veterinary scholarships. Ten of these scholarships are offered annually; the former are for a two years' course of post-graduate study, the first year in this country and the second year usually at the Imperial College of Tropical Agriculture in Trinidad. A scholarship is worth £250 a year with additional provision for travelling, books, and training fees abroad. The veterinary scholarships are similar in value. Outside the Government service there are openings for science graduates in the service of such organisations as the Empire Cotton Growing Corporation (which also offers agricultural scholarships), the Rubber Research Institute in Malaya, and the Rubber, Cocoa and Tea Schemes in Ceylon.

The following notes compiled from letters received from various agencies indicate the prospects of employment for young men in the Dependencies concerned at the present time.

The West India Committee.—Comparatively speaking, there are few openings for young Englishmen in the West Indies unless they have ample capital of, say, £3,000 and upwards to invest in fruit cultivation. Occasional vacancies occur for overseers on sugar plantations, salary about £80 a year to start with, all found, but the starting age is usually 21. Young men with agricultural posts open to them would do well to take a course at the Imperial College of Tropical Agriculture. In the professions there are not many openings, as many West Indians themselves embark on professional careers. At the present time there are no vacancies for overseers or in industries.

Malayan Information Agency.—Companies and business firms do not as a rule care to accept minors, as they are not in a position to sign binding contracts. In certain instances the Government engages probationers at 19 years of age. The Eastern banks engage at home, and train prior to leaving. When engagements are made in this country (U.K.), passages are paid in every case. In consequence of general trade depression and Government financial stringency, opportunities in general business are at present practically nil and the filling of vacancies in Government departments is restricted to absolute necessity. This Agency is always glad to give advice to any who may wish to proceed to Malaya.

The British North Borneo (Chartered) *Company.*—The European population of North Borneo, including women and children, is only about 500. Opportunities for employment are very limited, and in the present depressed state of trade the tendency is to reduce the number of Europeans employed. In normal times vacancies occur at infrequent intervals for assistants in the various rubber and tobacco estates; office assistants and accountants are also occasionally required by commercial firms. In these appointments a secondary school standard of education is generally required. The cost of passage from the United Kingdom to Borneo is usually paid. No person seeking employment should go to North Borneo unless assured of employment on arrival by some firm or company of repute. In the Government service, administrative and professional (including banking) appointments exist, but for administrative posts (cadets) the minimum age limit is 22, and professional posts are invariably filled by the appointment of qualified men with previous experience and over the age of 21.

Office of the High Commissioner for Southern Rhodesia.—While the present world depression lasts, the Government of Southern Rhodesia is not desirous of encouraging young men to go out to the colony unless they are equipped with means as well as special training for the avocation that they desire to follow.

Gold Coast (West Africa) *Government Commercial Intelligence Bureau.*—There are no openings in the Gold Coast for anyone under the age of 23 or 24. At present there are no openings of any kind, owing to the economic position.

His Majesty's East African Dependencies Trade and Information Office.—In these territories a European is always in a position of responsibility and, therefore, openings for skilled persons are not quite the same as in territories like Canada and Australia. The opportunities may be divided under three headings, (*a*) Government Service, (*b*) Commercial, (*c*) Agricultural. Practically all the commercial houses of any standing in East Africa are represented in the United Kingdom either by head offices or by agents, and engage such staffs as they require through their United Kingdom connexions. Broadly speaking, the commercial opportunities are those connected with import and export merchants, in addition to which there are the usual professions such as solicitor, doctor, dentist, surveyor, auctioneer, etc. The main opportunities for agriculture are, of course, for men with adequate capital and the requisite agricultural knowledge to start upon their own account. In addition, the bigger estates employ a number of assistant managers, accountants, engineers, etc., besides which the big motor houses or garages employ a certain number of foremen mechanics.

With regard to agriculture it is difficult to get a paid post without first obtaining local agricultural experience in East Africa. This can nearly always be arranged through this office at an approximate cost of £200 to £250 for the passage out, board, lodging and tuition for one year. Apart from the special training above referred to, training at an agricultural college,[5] or practical training on a farm in Great Britain, before going out, is of considerable assistance. There are no agricultural colleges in East Africa, the nearest being in South Africa.

No financial assistance is available in connexion with passages, except for pupils placed on farms to learn, in which circumstances they are entitled to a Settler's Certificate from the above office, which enables them to obtain a reduction of 15 per cent. They are also entitled to a second-class Railway Concession Voucher which enables them to travel from Mombasa to their destination at half fare. The employment situation in East Africa, outside the Government Service, is not at all good at the present time. These countries are mainly agricultural countries and are suffering from the worlds depression in agricultural prices. In Northern Rhodesia, round Ndola, extensive development is taking place in the copper mining area, and openings occur from time to time for men with special mining training. Such appointments as mining engineers and mining chemists are filled by the mining companies' head offices in the United Kingdom, but the bulk of the remaining employees are taken on locally.

Sudan Government London Office.—Regarding openings in the Sudan, accountants in the senior grade, with public school or university qualification and a special accountancy qualification, are considered between the ages of 23 and 32. For clerical posts, *e.g.* junior accountants, secretaries, clerks, etc., the qualifications usually specified are similar to those which are required by a good business or banking house in this country. The minimum age for such posts is usually 25, but occasionally candidates with good education and qualifications are accepted at 23 years of age. For the political service, candidates are considered between the ages of 22 and 25 if they have completed a University course and obtained a degree. For most other appointments the minimum age is usually 25, but for one or two special posts, such as veterinary inspectors, or inspectors of agriculture, the minimum age specified is 23 years. It may be taken that for all technical posts some degree or special qualification is required. All officials have to pass an examination in Arabic within two years of arrival in the Sudan, increase of pay being dependent on passing such an examination. A free passage to the Sudan is allowed to each newly appointed official, plus £E15 to cover incidental expenses during the voyage and customs duty at Port Sudan.

Government of Cyprus Trade Commissioner's Office.—Intending settlers are not advised to seek a career in Cyprus unless they have some definite employment in view.

[5] Information respecting the various Agricultural Institutions and Colleges in Great Britain can be obtained from the Ministry of Agriculture, Whitehall Place, S.W.1, the Department of Agriculture for Scotland, Edinburgh, and the Department of Agriculture for Northern Ireland, Belfast.

The minimum capital required by an intending farmer would be £3,000. Cyprus is rich in mineral deposits and the mining prospects are good; the only concerns staffed to any extent by other than Cypriots are the mines.

III. OPENINGS FOR GIRLS OVERSEAS.

There are important differences in the problem confronting the migration of girls compared with that of boys. Girls are less likely to entertain the idea of overseas life immediately on leaving school, even if the prospects are put before them while there. The openings are more aptly described as those for young women. They may be divided into groups, (1) those who are willing and able to pay their own passage, and (2) those who require State assistance to enable them to go overseas. In addition to these there is, for well-trained young women, as for young men, a considerable demand for medical nursing and educational service by the various missionary societies. These societies pay the travelling expenses of their staffs.

1. Non-State-aided Settlers.—With regard to professionally trained women, the choice of occupation is as wide as that in the United Kingdom and follows much the same lines. The remuneration offered is, as a rule, higher than that at home, but in some instances the cost of living is also on a higher scale.

(*a*) *General Conditions.*—Generally speaking, a girl must be in the Dominion to stand a good chance of obtaining a post when a vacancy in the desired occupation occurs. It will be easily understood that, as a rule, employers will not await the arrival of a candidate from overseas to fill an existing vacancy, and that though there is less competition in the newer countries than in the old it is seldom that a good post fails to attract a number of applicants. The British girl must therefore be on the spot to take her chance amongst those already in the Dominion. This involves the risk of going to the new country and relying upon private resources until such time as the fitting opportunity occurs. This may be a matter of only a few days or, on the other hand, of some weeks. The settler should be fully provided with introductions to residents in the Dominion who will be specially interested in her as a British settler and willing to advise her how to find the occupation she desires. Such introductions may be obtained from a variety of sources, such as Agents-General for the provinces or states of the Dominions, and voluntary societies, especially the Society for the Oversea Settlement of British Women, which is chiefly concerned with placing and encouraging the migration of educated women.

In the Colonial Government Service the openings for young women are practically limited to those who are qualified professionally in medicine, nursing, and education. The vacancies for lady medical officers are few, but for nurses very considerable. The appointments are pensionable in nearly all cases. In West Africa, initial salaries range up to £350 per annum with outfit allowance and free passages. Full particulars can be obtained from the Overseas Nursing Association.

There are also openings for women in the Education Departments of many of the Dependencies. Information upon these can be obtained from the Board of Education, Whitehall, S.W. 1. Postal clerks and telegraphists are appointed from among the members of the Home Service. The conditions prevailing in most of the Dependencies make it rarely practicable to appoint women to scientific posts.

(*b*) *The Society for the Oversea Settlement of British Women.*—The Society (S.O.S.B.W.) has been instrumental in placing some 2,800 women of the kind under discussion during the past ten years and is in touch with a large variety of correspondents in every part of the Oversea Dominions and in some of the Crown Colonies.

Before deciding to seek a career overseas it would be well for parents and girls to seek information from the Society or other competent authority concerning the country of destination and ascertain the conditions of local employment at the particular moment.

For instance, at the time of this Report being prepared, the economic conditions of Australia and Canada make it obviously undesirable for migrants requiring employment of any kind to take their chance overseas. On the other hand, these Dominions with their immense resources are capable of readjustment within a much shorter time than are the older European nations. It may well be that during the course of a year or so conditions in Canada will again allow the encouragement of well-educated and carefully selected girls to proceed to that Dominion. The S.O.S.B.W. is in constant touch with authorities and individual residents in each of the Dominions and, like

the Women's Branch of the Oversea Settlement Department, is in a position to give first-hand and accurate information on these and kindred points.

(c) *Teaching Posts.*—Teaching posts in *Government Schools* in Canada, Australia, New Zealand and South Africa are filled only from applicants already in the Dominions, and in Canada only from amongst those who have taken a period of training in a normal school. The length of necessary training varies in the different Provinces; it may be as long as 33 weeks or as short as 15 weeks. Occasionally an interim certificate to teach pending the final examination being taken is granted by the Education Authority. In Australia and New Zealand British-trained teachers may receive appointments provided they possess a Board of Education Certificate and have had a college training. In most of the Provinces in the Union of South Africa a locally trained candidate is given preference over a British one unless the latter has been in the Dominion for three years. This rule does not apply in Natal. In Southern Rhodesia British women holding good British qualifications may be accepted for work in Government schools. The Education Department seldom applies to England direct for trained certificated teachers; but an applicant already in the country and presenting good testimonials and certificates may occasionally be accepted. It is, however, possible for trained certificated teachers to be placed directly in positions overseas by the S.O.S.B.W.

Private Schools in all the Dominions may, if the Board of Management desire, draw their staff from British-trained teachers. The S.O.S.B.W. maintains correspondence with the headmistresses of such schools, who from time to time notify the Society of approaching vacancies on their staffs. In like manner the Society keeps a private register of qualified women who desire to hear of openings overseas. Selection is made by a panel of experienced women, who examine the qualifications, etc., of all applicants, both from amongst those already on the Society's register and those with whom contact is achieved through advertisements in educational or other papers. Employers specify the particular qualifications required for each post, and it should be borne in mind that, generally speaking, degrees in the higher branches of teaching, such as Mathematics and Latin, or Kindergarten and Froebel Diplomas, are asked for.

The salaries offered vary greatly. Those in Southern Rhodesia are, as a rule, rather higher than the teacher might expect to obtain in the United Kingdom, but the higher cost of living must be set against this. In the Dominions the salaries are, generally speaking, about the same as those in Great Britain, though in some Provinces and States the scale is lower.

(d) *Hospital Nurses.*—The Overseas Nursing Association, a society of long standing in London, acts in co-operation with the authorities of the Colonial Office, and selects nurses to fill vacancies in the hospitals of the Crown Colonies. The S.O.S.B.W. is also able to place nurses direct by maintaining contact with superintendents in charge of hospitals and nursing homes throughout the Dominions. These officials from time to time notify the Society of unfilled posts.

(e) *Nursery Governesses.*—The S.O.S.B.W. is also able to send a limited number of well-educated women to South Africa to be placed by the representatives of the Society in Cape Town as nursery governesses in private families or in other positions for which they are fitted.

(f) *Cost of Passage.*—The cost of passage must, as a rule, be borne by the individual applicant, though in some instances the school or hospital concerned will bear a portion or possibly the whole of the cost in return for an undertaking to serve a specified number of years. To others who are unable to find the necessary money the S.O.S.B.W. is able to offer help by way of loan to be repaid by instalments over a period of years.

(g) *Government Assistance.*—An agreement to which the S.O.S.B.W. is a party is in force between the Government of Great Britain and Southern Rhodesia enabling considerable assistance to be given towards the transit of young women proceeding to definite employment of a domestic kind in Southern Rhodesia. Such employment includes that of nursery governesses and trained children's nurses, and, in special instances, hospital nurses.

2. State-aided Settlers.

(a) *Assisted Passages.*—Turning to the migrant who requires State assistance, it may be said that since the passing of the Empire Settlement Act in 1922 and the resultant agreements between the British and Overseas Governments which were in

force until the end of 1930, assisted passages have been made available for girls who were able and willing to undertake domestic service for a minimum period of one year in the Dominion of their choice, and also for women, married or single, who were nominated by residents already in the Dominions.

(*b*) *System of Nominations.*—Under the system of nomination applicable to men and women, a responsible resident in one of the Dominions may apply to his (or her) Government for permission to name a person in the United Kingdom to proceed to the Dominion. The nominator has to undertake the responsibility of guaranteeing a home or employment for a definite period for the person nominated; and the Overseas Government, if satisfied that the nominator can carry out his (or her) obligations, issues, through the official representative in Great Britain, a permit which carries with it a grant of State assistance towards the expenses of transit. The exact conditions under which such nomination is allowed vary from time to time, but, generally speaking, reunion of families is encouraged by this system, and in time of commercial prosperity other categories are added to the list of those who may qualify for nomination.

New Zealand, for instance, permitted nomination for single women and widows without children under the age of 40 years. Australia and Canada have in the past allowed nomination of various kinds; at the moment, however, most of these latter facilities are in suspense owing to the economic depression in the Dominions.

(*c*) *After-care.*—The after-care or welfare of assisted migrants has been carefully provided for under the agreements entered into between the British and Dominion Governments. Domestic workers have been placed under the charge of specially appointed matrons on board ship, and arrangements have been made for the immigration officers, accompanied by women representatives of voluntary societies, to meet the ships, get into touch with all the new arrivals, and explain to them how to maintain contact with the voluntary agencies, or with individual women, in such a way that, should they at any time find themselves in difficulty, they may apply to these friends. The actual employment is found for each girl by the Immigration Officials of the Dominion concerned.

In Canada a Hostel of Reception, to which every newcomer is taken (unless she has made other arrangements), has been established in each Province. Young women may return to these hostels if they find themselves in difficulty or without employment. The same kind of arrangements have been in force in Australia and New Zealand.

In addition to such protection, the S.O.S.B.W., the Victoria League and various other voluntary agencies offer introductions to their own correspondents to women who are known to be going overseas. Thus parents and others interested in young settlers may be fully assured that the new settlers need not want for friends if the existing machinery is utilised by the intending migrant before leaving this country.

(*d*) *Training.*—During the past two or three years there have been established in various parts of Great Britain training centres in which free training of 8 to 10 weeks' duration was offered to girls who wished to take up domestic work in the Dominions but were unable to qualify without receiving training. Owing to the suspension of demand for household helps in Australia and Canada these hostels are for the moment in abeyance, but may perhaps be opened again when the flow of migration substantially increases.

IV.

PUBLICITY AND INFORMATION.

1. *Introduction.*—The problem of how to bring a knowledge of these opportunities and prospects of careers in the overseas Dominions and a knowledge of the societies and agencies offering facilities for oversea settlement to the notice of pupils and their parents has been the chief concern of the Committee this year.

A policy of encouraging migration may not be unanimously accepted as desirable by all school masters and mistresses, and to use the schools for a propaganda in the narrow sense might well be resented by the teaching profession, but information likely to be of use to a teacher in advising his or her pupils as to their future would be welcome, and should be made freely available to all.

The Committee feel that this knowledge of what life is offering, especially knowledge of what the Empire overseas is offering, should be available before school

days are over, so that the knowledge may play some part in directing the energies and maintaining the interest of the pupil in his studies while still at school.

The various ways by which teachers, parents and pupils can be brought to a knowledge of what the Empire overseas has to offer in the way of careers for boys and girls from secondary schools depend at present largely upon the unco-ordinated services of public and voluntary institutions.

2. *The Oversea Settlement Department.*—The Oversea Settlement Department, Caxton House, Tothill Street, London, is prepared to supply schools with relevant information on application, and to arrange for lectures to be given by experienced speakers with special lantern slides and films. Through the co-operation of local authorities the Department have distributed special pamphlets dealing with agricultural settlement overseas to the majority of schools—both primary and secondary—in Great Britain. They have also made it quite clear that they stand ready at all times to give any further help or advice in their power to education authorities or to teachers. They are always glad to send experienced speakers, free of charge if necessary, to give addresses to schools. Broadcast talks and dialogues on the subject have also been given under the ægis of the publicity section of this Department.

3. *Oversea Dominion Governments and Colonies.*—(*a*) *The Canadian Government Emigration Department* at Canadian House, Trafalgar Square, S.W. 1, has a well-organised scheme for sending speakers to any part of the country to give lectures, illustrated by lantern slides, on the facilities offered by the Canadian Government to prospective settlers in Canada. These speakers have hitherto gone mainly to country towns and villages, and to elementary schools, seldom to secondary schools, as they are under the impression of not being wanted there. But they would be only too pleased to go by invitation to any secondary school where projection apparatus is available. The Department has well-qualified agents residing in definitely arranged areas covering the whole of the British Isles, and these agents are fully prepared to co-operate with and to assist in any scheme for the dissemination of information concerning overseas life in Canada and its prospects.

(*b*) The *Australian Government* does not at present do anything itself in the way of propaganda, but they are ready to lend to schools films and slides, with a copy of a lecture illustrative of Australian life, and to supply any statistics required. Application should be made to Australia House.

(*c*) The *New Zealand Government* has, through its High Commissioner, distributed literature to schools and arranged for lectures in them in previous years, but owing to the depression at the present time these activities are suspended.

(*d*) The *Union of South Africa Government* has hitherto had no policy towards the encouragement of immigration. It does not itself do anything to encourage the youth of Great Britain to consider the possibility of a career in the Union, although there are signs that this attitude may change when the present world depression has abated.

(*e*) *Southern Rhodesia,* Crown House, Aldwych, W.C. 2.—The Government of Southern Rhodesia has issued a fully illustrated handbook for the use of prospective settlers. It contains a very full account of the natural resources of the country, and a description of the land settlement schemes in operation last year. At the present time no encouragement is being offered by the Government to settlers unless the immigrant has considerable capital and has already received a training for the particular type of farming he desires to pursue.

(*f*) *The Colonies.*—The Colonial Office issues a pamphlet, ' Colonial Service Recruitment, No. 1,' containing general information regarding appointments in the Dependencies administered by the Secretary of State. Detailed information upon appointments in each branch of the Service (administrative, medical, educational, legal, etc.) is afforded in Nos. 2 to 9 of the same series. No. 4 deals with agricultural, veterinary, forestry, and other scientific appointments. The pamphlets can be obtained by application in writing to the Director of Recruitment (Colonial Service), Colonial Office, 2 Richmond Terrace, Whitehall, S.W.1.

4. *The Imperial Institute.*—The Imperial Institute at South Kensington has been a potent agency by means of its Galleries, Cinema and lectures, for spreading knowledge of the overseas Empire among the schools. The Galleries have been purposely reorganised during the last four years with the object of not confining the exhibits to the raw products—animal, vegetable, and mineral—produced in each part of the

Empire, but making the Galleries a real school of Empire geography very much on the human side. Whereas prior to this reorganisation the attendance at the Galleries was about 50,000 a year, including some 5,000 children, the attendance in 1930 was over 600,000. During each of the last three years 80,000 children, in their organised classes from every type of school, have attended to learn about the scenery, activities, and products of the overseas Empire—India, Dominions, Colonies, and Protectorates. In the Cinema—provided by the Empire Marketing Board—there are four sessions each day of films from various parts of the Empire. In this way the Imperial Institute is unique, being the only place in the whole Empire in which such an effort is being made.

An information stall in the Galleries is provided for the distribution of pamphlets and booklets prepared in the Dominions giving fullest information about conditions in their countries. Lectures are also given in the Cinema by persons who have had practical experience of life in the Dominions and in the Colonies.

5. *Society for the Oversea Settlement of British Women*, Caxton House, Tothill Street, London, S.W. 1.—This Society is more concerned with young women over 18 years of age than with girls at school, but it secures and maintains contact with a large number of girls' schools through the private influence of members of the Society and through the provision of literature and of speakers to such schools. At one time the Society sent out special speakers selected by them, but since the Oversea Settlement Department has undertaken general publicity and provision of speakers for women as well as for men, speakers for girls' schools are now selected by that section of the Department in consultation with the S.O.S.B.W.

6. *Public Schools Employment Bureau*, 5 Paper Buildings, Temple, London, E.C. 4.—This organisation publishes periodically a special bulletin on the subject of Migration. The bulletin is sent to the headmaster or 'careers' master of each of the public schools in Great Britain, as well as to many others. It contains full particulars of most schemes of settlement operating in different Dominions, and also some valuable advice to the boy who thinks of going overseas. The organiser of the Bureau invites application from all who wish to participate in, or take advantage of, any of those schemes. He is prepared to supply further information at any time, and to assist any boy approved for settlement overseas in obtaining the necessary permits and introductions to the authorities of the Dominion selected.

7. *The Incorporated Association of Headmasters.*—The Employment Committee of this Association have hitherto issued no memoranda or reports on the subject of overseas employment, but circulate from time to time particulars of schemes under the Empire Settlement Act suitable for boys to schools in the counties of London, Kent, Surrey, Sussex, Essex, Herts, Middlesex, Bedford and Cambridge.

8. *The Headmistresses Association* (Employment Committee).—This Association acts through an Employment Committee under the auspices of the Ministry of Labour. Twelve headmistresses and two assistant mistresses, representative of secondary schools, serve on the London Committee. There are also several local committees of this type which meet in various parts of England and Wales, and operate on similar lines to the central one. In the last report of this Committee, published by the Ministry of Labour, no reference is made at all to the possibility of overseas careers for girls, but the headmistresses are taking considerable interest in the prospects overseas for secondary school girls, and individually do a good deal to awaken interest amongst the girls. A number of headmistresses have recently (1931) made a tour through Canada with the object of studying at first hand the conditions of employment out there. Their report has not yet been published, but it is understood that they received a very favourable impression as to the prospects awaiting girls and young women from Great Britain.[6]

9. *Church of England Council of Empire Settlement*, 39 Victoria Street, London, S.W. 1.—This society has extended its influence rapidly since its inaugural year, 1925. The Council works primarily through the clergy of some 14,000 parishes, and by their aid they have got into touch with large numbers of parents seeking openings for their sons and daughters. They have also established contact with most of the juvenile employment bureaux of the various education authorities of the counties and in some of the larger towns. The Council has thus succeeded in disseminating information on the opportunities and prospects of oversea life over a wide area. Members of the Council, particularly those with overseas experience, have visited a number of

[6] See note, page 314.

schools, and a sub-committee, especially charged in regard to propaganda, is in touch by correspondence with a large number of 'careers' masters of the larger schools. The Council also issues an Empire Settlement Handbook which conveys detailed information, and offers valuable advice to all seeking to know something of the conditions of overseas employment.

10. *Boy Scouts Association*, 25 Buckingham Palace Road, London, S.W. 1.—Within this Association there is a migration department which has definite schemes of its own for overseas settlement. Information about these schemes is conveyed to every Scout Troop Headquarters in the British Isles by some 10,000 migration wall-cards, and these are supplemented by the issue of a booklet, 'The Call of the Empire.' Leaflets supplying details of the schemes are also distributed in the same way. During the year lectures are given at all the chief centres, and the 'Scout Magazine' frequently contains illustrated articles on the subject of overseas life and its prospects. Details of Scout migration schemes have been broadcasted from Glasgow.

11. *Interchange of Teachers.*—The interchange of teachers between the schools of Great Britain and the Dominions affords another avenue through which first-hand knowledge of overseas life may reach the pupils.

The League of the Empire (see Appendix 1 (*l*), page **317**) has been instrumental in establishing a scheme accepted by the Board of Education for the interchange of teachers from elementary and secondary schools in this country with those from Dominion schools. Although the larger proportion of teachers taking advantage of the scheme come from elementary schools, there is a gradually increasing number from secondary schools. The small number from the latter is due largely to

(1) the variations in school terms in different countries ;

(2) the necessity of exchange being made between teachers possessing equivalent qualifications and attainments.

It is hoped that these difficulties will gradually disappear, and that our secondary schools will soon gain the benefit of having teachers on their staffs possessing the wider outlook and experience that such interchange affords.

12. *The Overseas Education League* (see Appendix 1 (*m*) page **317**) also has a scheme for the interchange of teachers, as well as a comprehensive organisation for promoting overseas visits of teachers and undergraduates from and to Great Britain and the Dominions.

13. *The Anglo-Canadian Education Committee*, formed as a result of a tour of sixteen headmasters of English public schools to the Canadian Universities in 1930, will, no doubt, help to turn the attention of schools in the same direction.

14. *Local Migration Committees and Juvenile Employment Bureaux.*—There are about forty-eight Local Migration Committees in Great Britain supported by voluntary contributions and founded within the last few years in counties and towns to promote schemes for training suitable boys—14 to 18 years of age—for farming careers in the oversea Dominions. Few of these, however, deal with the secondary school pupil as such. Many of them work in close association with the juvenile employment bureaux of the education authorities, and all of them in conjunction with the Oversea Settlement Department.

In most cases the education authority prefers to leave the subject entirely alone, and does not appear to favour any action that suggests even an encouragement of emigration on its part. In London, for example, an entirely passive attitude is adopted. The Education Department of the L.C.C. willingly permits accredited organisations concerned with migration to distribute suitable pamphlets regarding emigration, and allows the use of school organisation for lectures on the subject and offers facilities for boys and girls to attend them ; otherwise it does not officially take any active part in promoting or supporting special steps to assist or advise those who wish to take up a career overseas.

On the other hand, in some towns, such as Bristol and Birmingham, there is a greater interest shown by the Education Committee, and in the latter the juvenile employment officers attend at all the secondary schools in order to give advice to pupils when they leave, and co-operate with the headmaster in supplying necessary information to boys interested.

The only district in which it appears that the question of the migration of the secondary school pupil has received definite attention is in the north-east of England. There the Secretary of the Northumberland and Durham Empire Settlement Com-

mittee co-ordinates the work of twenty-five local migration committees in Northumberland, Durham, Yorkshire, and Lincolnshire.[7] He writes :

'The Migration Committees very early realised that it was highly desirable to migrate secondary school boys to the Dominions, but the number of such boys coming forward was lamentably small, compared with the number leaving school every year. In view of the grave industrial depression in this area during the past few years it has been our definite policy to endeavour to influence secondary school boys to consider the desirability of migration, and the co-operation of headmasters has been sought. In 1929, letters on the subject were addressed by the chairmen of the several committees to all headmasters of public and grammar schools throughout their area. Endeavours are made to obtain the consent of headmasters to lectures on the Dominions during or at the end of the term. Where the apparatus is available, these lectures are illustrated by cinema films in an attempt to interest boys in the Dominions from two points—(1) further education, (2) settlement on the land. In regard to (1) a number of boys from this area have, at our suggestion, proceeded to the Macdonald College, or other University in the Dominions. Others have taken up further education after settling.'

15. *Boys' Training Hostels.*—Some of the migration committees have established boys' training hostels where boys of 14½ to 18 years of age are taught the rudiments of farming, the care of animals and rough carpentry free of cost for a period of six to twelve weeks. Of these hostels, those of the Walker Hostel now transferred to Newbiggin in Northumberland, Beverley in Yorkshire, the Ham Green Farm near Bristol, Carr Hall Farm, Burscough in Lancashire, and the Cossar Boys' Training Farm near Paisley, are the best known. Although described as training hostels, their chief function is to test the suitability of boys for farming. A proportion of the boys, after a short period in the hostel, realise for themselves their unsuitability, and others are so regarded by the Warden and advised not to emigrate. Such boys are thus saved from failure in the Dominions. A few boys enter the hostels from secondary schools, but the greater bulk of those taken in are from elementary schools.

16. *Young Men's Christian Association*, Migration Department, Kingsway House, Kingsway, W.C. 2.—This world-wide organisation takes an active part in promoting migration. By means of its many branches throughout the country it has excellent opportunities for distributing information to youths who have just left school. Recruitment is carried on by talks and addresses to boys' clubs and to schools, by advertisements in the Press, and in its halls and rooms. Close touch is kept with other organisations interested in migration. Even in this year of depression (1931) it is expecting to send out 350 boys to Canada, although not more than about 5 per cent. of these come from secondary schools.

17. *The British Broadcasting Corporation.*—The B.B.C. has done yeoman service in the matter of distributing information on overseas life and its opportunities for British youths. Talks have been given by eminent men and women of wide personal experience of life in the Dominions. If they could have been heard by boys and girls in our secondary schools there would be little doubt as to the response on their part. Parents who heard them must have been impressed by the general agreement of all speakers that the Dominions offer great prospects and unlimited opportunities for the boy or girl of enterprise, sound physique and good character. Over sixty-five of these talks, arranged in consultation with the Oversea Settlement Department, have been given at intervals between 1925 and December 1930. Owing to the present state of world depression, they have been suspended as a regular feature, although occasional talks will be given when it seems suitable to the Department.

V.

THE SCHOOL.

1. *Causes adversely affecting Migration.*—In spite of the many organisations for promoting and encouraging overseas migration, there is considerable evidence that

[7] A valuable and informative report has been recently issued (1931) by the Northumberland and Durham Empire Settlement Committee. It details the many activities and excellent organisation for assisting migrants of all kinds, and presents some striking results of local interest and financial support given to the twenty-five local committees whose work the county committee co-ordinates. The arrangements outlined in the report for encouraging and assisting Empire settlement are similar to and prove the practicability of those advocated on page 314 of this Report.

the schools and parents are ignorant of the efforts made in this direction on behalf of the youths of the country.

The Committee have been impressed by the limited response that has been made during the last six or seven years to the many liberal offers of assistance, financial and social, for placing boys on the land overseas, and by the ignorance of the schools in regard to the wealth of opportunity that has been offered to the British boy during the last decade for overseas life.

It is possible that parents have not realised the value of overseas enterprise for their sons, nor the immense importance it possesses for the future of the Empire generally. It seems from the evidence of many who have worked to induce boys of a suitable type to consider the possibilities of a career overseas that the chief obstacle to the migration of boys comes from the parents. Headmasters have written in the following strain :

'A general disinclination on the part of the parents and boys to face life overseas is due partly to the urban dweller's lack of knowledge and appreciation of country life, and partly due to the prevalence of small families, the mother especially objecting to part with her only son.'

'In the majority of the cases, parents are not prepared to allow their children to emigrate before the age of 21, and they do not wish them to take up agriculture in England; consequently, on leaving school, many who would be suitable for an overseas life take up other occupations in which they lose contact with and interest in agriculture.'

'Nowadays it is difficult to persuade boys and their parents that the hope of success is often in getting away from home.'

The Secretary of the Headmasters' Employment Committee writes 'that individual boys who are told of schemes under the Empire Settlement Act at our offices are generally unwilling to embark on a farming career in the Dominions.'

A Headmistress writes : 'The chief difficulty is the parents. This opposition of parents is a vital difficulty, and unless it can be reduced, and parents' confidence in the prospects and welfare of their sons and daughters established, there will be little improvement in the numbers going overseas.'

It is pleasing to note, however, that there are signs of an awakening among the general public, particularly the thinking parent, on the question of migration. The excellent supervision and after-care of the young migrant exercised by the various voluntary societies have undoubtedly helped to bring about this change of attitude. On the schools' side individual headmasters have been sympathetic, in some cases enthusiastically so, in their desire to give their non-literary, non-mathematical pupils a training to fit them for an active outdoor life ; but it must be stated that there are many who have been, and still are, wholly indifferent, if not definitely antagonistic, to the idea of any of their pupils being encouraged to consider the possibility of an overseas career. Enlightenment is, however, gradually taking the place of ignorance. This is due in a large measure to the greater facilities for travel promoting more frequent visits, reunion of families, extended tours, interchange of teachers, and other opportunities for direct contact with countries overseas.

A sound educational training for overseas will appeal more and more to the thinking public, who will demand in place of much that is purely academic in school curricula a greater provision of those studies and practices that make for handiness, alertness, and initiative, that promote skill of hand and eye with understanding, and establish a contact with nature and science. In some quarters there is a growing tendency to adapt the curriculum to an education at once more real and practical, and therefore more suited to life overseas, but the chief obstacle to any general concession in this respect is the examination spectre, hence there is as yet no widespread adoption of this policy. That a number of public school headmasters are taking the question of migration into serious consideration is a promising sign, and their recent action may encourage the rest of the secondary schools to follow in due course.

The absence of full and reliable knowledge of conditions overseas is in all probability at the bottom of both parental and scholastic opposition. When even headmasters write : 'Fuller information on the possibilities for boys to emigrate without capital would be most acceptable,' 'Should be glad to receive information regarding agencies and organisations that exist for assisting those who desire to go

overseas,' it suggests the need for a more systematic and wider distribution of information than exists at present.

For lack of this knowledge schools are out of touch with the needs of the Empire overseas, with its opportunities for enterprise and livelihood. The schools for the most part ignore the practical boy and his need for training in things of skill, of hand and eye work, of outdoor interests; they exert no directive influence on the more practically minded of their pupils towards the possibility of open-air occupations overseas, or even for similar openings in the homeland.

2. *Means of diffusing Information.*—The schools themselves can assist in bringing a knowledge of the Dominions and of what they offer in the way of careers to boys and girls. Besides providing lectures and speakers, lantern and film demonstrations on overseas life, the following suggestions have been made to show how schools can assist:—

1. Short articles and notices about overseas life might appear from time to time in the school magazine, especially letters from Old Boys written specially for the purpose. The magazine might also contain occasionally an explanation of the schemes for assisting overseas settlement.
2. The school library might be supplied with handbooks and pamphlets issued by overseas societies and committees. They should be kept up to date, and full particulars of the various schemes operating made easily accessible. Still more important, the libraries should be well stocked with stories dealing with overseas life, and with books of travel and adventure in the Dominions.
3. Notices might be posted on the school notice board, drawing attention to the existence of these overseas prospects, and to sources of full information.
4. Broadcast talks might be given to schools dealing with specific opportunities as they occur, and as the situation opens out for improved prospects of employment overseas, talks on schemes of training and on further education possibilities in the Dominions themselves.
5. More encouragement might be given to masters and mistresses to travel in the Dominions, especially to interchange with teachers in the Dominions. Personal experience would render their subsequent teaching of overseas geography more graphic, besides enabling them to speak with better understanding of the conditions of overseas life.

3. *Visits of Dominion Speakers.*—The mere distribution of handbooks and pamphlets to the schools is of little value unless the schools are interested and willing to pass on the information. In general much of this literature is pigeon-holed and forgotten, or more often simply consigned to the waste-paper basket, so that neither pupils nor parents benefit. The Committee therefore stress the importance of personal contact between representatives of oversea organisations and the schools, the need of periodic visits to the schools by experienced speakers, experts who have themselves known the conditions of life in the Dominions. These should, as occasion offers, be invited:—

1. To give lectures illustrated by lantern or films.
2. To speak directly with boys who show an interest in the prospects.
3. To address meetings of parents at the school.

With regard to lantern slides and films, these could be made more effective by introducing a thread of human interest, such as the story of a young migrant from his departure from the home port to his final settlement in the country of his adoption overseas.

In the last case, question and debate should follow, and the whole situation, financially and socially, be discussed in order that parents may be assured, not only of the soundness of the prospects, but also of the adequacy of the arrangements provided for the welfare and after-care of the young migrant, wherever he may go in the Empire. The Committee feel convinced that it is only through the active co-operation of the headmasters and headmistresses of the schools in such a course that an improvement in the attitude of the parent can be brought about.

4. *Careers Masters.*—It is beginning to be recognised that boys and girls should be encouraged while still at school to consider their future occupation, and not allowed to drift on to the end of their school days aimless and careless in regard to the future. The only goal kept in view by the majority is the matriculation or some other similar examination success which is imagined by many to be the key that will open any door to a well-paid post. It is suggested that every school should have a 'careers'

master or mistress whose business it is to be acquainted with the labour market—professional, industrial, commercial, agricultural. Through them business houses, industries and all organisations concerned in providing openings for boys and girls would reach the most suitable candidate. It would be their business to advise pupils and their parents, and to interest them in the possibilities of various careers. They would arrange lectures and speakers on topics of vocational interest, one of which, of course, would be that of overseas life.

5. *Final Suggestions.*—The situation suggests the need of some co-ordinating agency or authority to link up the work of different overseas migration societies into one co-operating whole. Through such an agency it would be possible to organise local distributing committees whose representatives could visit systematically all the secondary schools in conveniently defined areas. The Oversea Settlement Department would be a suitable co-ordinating body, while the Local Migration Committees that already exist might become the distributing bodies. At present the latter are barely functioning owing to the depression overseas, and to the fact that they have hitherto, with one exception, dealt only with boys from elementary schools. If they can be induced to undertake a wider sphere of activity, and to concentrate on the migration of the secondary school boy and girl, to undertake on behalf of the Oversea Settlement Department the work as local representatives of the Department to the schools, they would find renewed interest in their mission and a valuable outlet for their activities.

This Committee urge the Council of the British Association to consider the possibility of bringing the matter to the notice of the Secretary of State for Dominion Affairs with a view to placing the need and value of such an organisation before the Oversea Settlement Department for consideration.

The Committee feel that no time should be lost in the promotion of a scheme such as is here outlined; they believe it is in the interests of the country, as well as of the individual, that increased publicity be given to the opportunities and facilities for sound and promising careers overseas in order thereby to secure, when the present state of depression has passed, a larger response than hitherto from boys and girls of the right type to the call of the Empire, and to secure a fuller recognition by their parents of the possibilities for successful careers awaiting their sons and daughters in the Dominions and Dependencies overseas.

The Committee also urge the Council to ask the Board of Education to make their grants towards the cost of senior scholarships, offered by local authorities, tenable at universities in the Dominions as well as at universities in Britain.

The Anglo-Canadian Committee (see page 19) have been actively engaged during the present year (1931) in urging this particular proposal upon County and Borough Education Committees. The Oversea Settlement Department has, through its representative, also strongly supported such a course.

Note 1. Since this Report was drafted the Committee have been informed that the London Education Authority has accepted the principle in a test case, as, in response to an application by the Anglo-Canadian Committee on behalf of a scholar of a well-known London school, the Authority has awarded him a Senior County Scholarship of £40 per annum for a period of three years on the understanding that this award is to be spent on his education in Canada.

The Anglo-Canadian Committee have further succeeded in obtaining for this lad a school-leaving scholarship of £50 per annum and a grant of £30 per annnm from the Thomas Wall Trustees for the same period. The total income of £120 will thus enable him to start at Queen's University, Kingston, Ontario, for his three years' course in the Faculty of Arts this September (1931).

The Committee earnestly hope that when suitable boys present themselves other Local Education Authorities will follow this lead and help boys to start their careers in this excellent manner.

Note 2. It has been further reported that, as a result of the favourable impression received by headmistresses during their visit to Canada this year (1931), a scholarship of £100 is to be offered for three years in succession, to enable a suitable girl to take a university course in the Dominion.

APPENDIX I.

List of Organisations for Overseas Settlement.

The following list of societies and institutions includes only those that definitely provide assistance and facilities for the overseas settlement of boys and girls from secondary schools. There are many other organisations providing training and financial assistance for agricultural occupations overseas, but they are essentially designed for those of a less educated type, or for adults only. Full information about these can be obtained on application from the Oversea Settlement Department.

1. Government Departments.

(a) *British Government—The Oversea Settlement Department,*
Caxton House, Tothill Street, London, S.W. (pages 7 and 18).

This Department is prepared to give full information on all schemes for overseas settlement, and to advise those who are interested how and where to obtain assistance for passage, training, or further education. It supplies, free on application, a number of handbooks which give details of schemes promoted by various voluntary societies, as well as information on all subjects relating to migration and the settlement in any of the oversea Dominions and Colonies.

(b) *Dominion Governments—Immigration Departments.*

1. *Canada.*
Canadian Building, Trafalgar Square, S.W. 1 (pages 8 and 18).

Inquiries relating to particular provinces should be addressed to the Agents-General for the Province, viz. :—

1. Quebec, 2 Cockspur Street, S.W. 1.
2. Ontario, 163–4 Strand, W.C. 2.
3. British Columbia, 1 Regent Street, W. 1.
4. Nova Scotia, 2 Cockspur Street, S.W. 1.
5. Alberta, 125 Pall Mall, S.W. 1.

2. *Australian Commonwealth—Migration and Settlement Department,*
Australia House, Strand, London, W.C. 2 (page 10).

Inquiries relating to particular States should be addressed to the Agents-General for—

1. New South Wales, Australia House, Strand, W.C. 2.
2. South Australia, Australia House, Strand, W.C. 2.
3. Tasmania, Australia House, Strand, W.C. 2.
4. Queensland, 409 Strand, W.C. 2.
5. Victoria, Melbourne Place, Strand, W.C. 2.
6. Western Australia, Savoy House, Strand, W.C. 2.

3. *New Zealand—Immigration Department,*
415 Strand, London, W.C. 2 (page 10).

4. *Union of South Africa,*
73 Strand, London, W.C. 2.

5. *Southern Rhodesia,*
Crown House, Aldwych, London, W.C. 2.

6. *Newfoundland,*
58 Victoria Street, S.W. 1.

(c) *India, including Burma,*
India Office, S.W. 1.

(d) *The Colonial Empire.*

Inquiries relating to appointments in the Colonial Government Services, whether made by the Secretary of State or by the Crown Agents for the Colonies, should be addressed to the Director of Recruitment (Colonial Services), Colonial Office, 2 Richmond Terrace, Whitehall, S.W. 1 (page 12).

Inquiries relating to non-Government employment in certain of the Dependencies should be addressed to :—

His Majesty's East African Dependencies, Trade and Information Office, 32 Cockspur Street, S.W. 1.

The Malayan Information Agency, 57 Charing Cross, S.W. 1.

The Gold Coast Commercial Intelligence Bureau, Abbey House, Victoria Street, S.W. 1.

The West India Committee, 14 Trinity Square, E.C. 3.

Office of the Trade Commissioner for Cyprus in London, Queen Anne's Chambers, Farrar Street, S.W. 1.

Inquiries relating to appointments in North Borneo and in the Sudan should be addressed respectively to :—

The British North Borneo Company, 17 St. Helen's Place, E.C. 2.

and to

The Controller, Sudan Government Office, Wellington House, Buckingham Gate, S.W. 1.

All Government Departments supply information and advice to intending settlers on all matters relating to immigration, passages, outfits, procedure, occupations, conditions of citizenship, education, etc.

2. Voluntary Societies.

(a) *Anglo-Canadian Education Committee,*
Seymour House, Waterloo Place, S.W. 1 (page 299).

Formed in 1930 for the express purpose of encouraging British boys to enter Canadian universities preparatory to taking up a career in Canada.

(b) *Boy Scouts Association,*
25 Buckingham Palace Road, London, S.W. 1 (page 299).

Assists and advises scouts and ex-scouts who wish to settle overseas, supplies letters of introduction to scout headquarters at ports of call and in the Dominions. It has operated a successful nomination scheme, and has administered scholarships provided by generous donors for agricultural training in Canada and in Rhodesia.

(c) *Church of England Council for Empire Settlement,*
39 Victoria Street, London, S.W. 1 (pages 299 and 301).

Assists in the selection of suitable settlers, and in the arrangements for their welfare during the journey from this country to their destination. It assists settlers for the outward journey and makes provision for the reception, placing and after-care at their destination. It furnishes settlers with letters of commendation to clergy and other responsible individuals in the parish in which they eventually settle.

(d) *Fellowship of the Maple Leaf,*
13 Victoria Street, London, S.W. 1 (page 299).

Promotes the appointment, in the Western Provinces of Canada, of British teachers who are members of the Church of England.

(e) *Hudson Bay Company—Migration Department for Farming and Fur Trading,*
Trafalgar Buildings, 1 Charing Cross, W.C. 2 (page 300).

(f) 1820 *Settlers Memorial Association,*
199 Piccadilly, London, W. 1.

Assists in the settlement in the Union of South Africa of British Subjects likely to become successful agriculturists and good citizens, and who have at least £1,500 capital (page 301).

(g) *Northumberland and Durham Empire Settlement Committee,*
Divisional Office, Quebec Chambers, Leeds.

An advisory and recruiting organisation for the north-east of England, co-ordinating the work of twenty-five local migration committees in the area. It provides a training hostel for testing the suitability of boys for farm work before arranging for their going overseas, and is definitely seeking the co-operation of headmasters of secondary schools in placing the opportunities of overseas careers before the boys (page 310).

Public Schools Employment Bureau, Migration Section,
5 Paper Buildings, Temple, London, E.C.

Open to all boys who have been educated at a recognised secondary school. It gives advice, and supplies information concerning overseas careers and schemes of settlement (page 309).

(j) *Society for the Oversea Settlement of British Women,*
Caxton House, Tothill Street, London, S.W. 1.

This society acts as the women's branch of the Oversea Settlement Department. It is the chief society interested in the overseas prospects for girls from secondary schools. It includes on its council representatives of other women's organisations, and co-operates with the immigration authorities and voluntary societies in the Dominions. It has representatives and hostels overseas, gives introductions, sends specially conducted parties to Canada, supplies advice and information, and in certain cases arranges loans for passages (pages 305 and 309).

(k) *Young Men's Christian Association,* Migration Department,
Kingsway House, 103 Kingsway, London, W.C. 2 (page 311).

Advises and gives information on all matters of overseas settlement. Co-operates with the Churches in Canada and in Australia, and with other overseas organisations in the placement and after-care of boys selected for farm work; undertakes the selection of suitable boys for the nominations received from the oversea Dominions; assists those selected in the arrangements for passages, financial and social, and for their reception at ports of call and destination; and generally, through its branches in all parts of the Empire, looks after their welfare until firmly established on their feet. Financial help is given in necessitous cases when possible towards their passage and outfit, and for their agricultural training.

(l) *The League of Empire,*
124 Belgrave Road, Westminster, S.W. 1 (page 310).

The League was founded in 1901 to promote co-operation between different countries and colonies of the Empire, mainly in affairs connected with Education. It initiated the scheme for the Interchange of Teachers and Inspectors throughout the Empire in 1907, but the scheme was not fully established until 1923, when the following recommendations were adopted by the Imperial Conference of that year, accepted by the Education Departments of the Dominions, and approved by the Board of Education :—

1. That interchange should be sought as far as practicable between teachers of similar qualifications and experience.
2. That leave of absence with pay be granted to teachers accepted for interchange; such leave of absence to cover the period of travel outwards and homewards.
3. That service abroad of interchanged teachers should involve no disability in respect of salary, increment, seniority, or of superannuation or other privilege.
4. That teachers so interchanged shall not enter the service of the authority to which they exchange without the permission of the authority under which they are permanently employed.
5. That all exchange of teachers shall be for the period of one year exclusive of the time spent in travelling.
6. That no teacher be accepted for interchange who is under 25 years of age or has had less than five years' experience or is over 45 years of age.

(m) *The Overseas Education League.*
29 Gloucester Terrace, W. 2 (page 310).

This League is of Canadian origin and started under the title of 'Hands across the Sea Movement.' Its main object is the organisation of visits by teachers and undergraduates to Great Britain and France, and reciprocal visits of British educationists to Canada. It has established a scheme of Interchange and Travel Study for teachers.

APPENDIX II.

Sources from which Financial Assistance can be obtained for Overseas Settlement.

During the past ten years the Home and Overseas Governments have done much to encourage and help British boys and girls to settle in the Dominions. Help has taken the form of:—

1. Free, or greatly reduced, ocean passages and railway fares;
2. Free farm training in Great Britain or in the Dominions;
3. Assured employment overseas with selected farmers;
4. Financial help and expert advice to enable suitable boys to become independent farmers.

At the present time (1931), owing to the financial depression, the amount of assistance on these lines has been severely restricted; in Australia it is entirely suspended.

I. Free or Assisted Passages.

Canada.—Free passage for boys over 14 and under 18 years of age who go to Canada under Government schemes, or under the care of approved migration organisations, provided the applicant has been accepted by the Canadian Government Immigration Authorities.

Similarly for girls over 14 and under 17 who are going to take up household employment.

Boys and girls over 18 years of age travel at a special ocean rate of £10, available for British settlers, instead of at the usual third-class rate of £18 15s.

Reduced rail fares to port of embarkation of 25 per cent. of the ordinary third-class fare from any station in Great Britain to all the above.

Australia.—At present no assisted passages to Australia are being granted.

New Zealand.—Free passages are granted to boys under 19 years of age and to girls and women under 40 years of age. Reduced fares are granted to boys over 19 years, viz., £11 10s. instead of the usual third-class fare of £37. In all cases the applicant must be of British birth, and approved by the New Zealand Authorities.

Union of South Africa and Southern Rhodesia.—No general reduced passage rates to these countries except for boys going out under the care of the 1820 Settlers Memorial Association; they are given a special reduction of 15 per cent. on the ordinary fare.

Boys and girls who wish to obtain free or reduced passages to the Dominions should apply either direct or through one of the voluntary organisations whose addresses are given on pages 316 and 317, to the Immigration Authorities of the Dominion to which they wish to go, whose approval must be obtained before an assisted passage is granted.

The Colonial Empire.—See notes on pages 302 and 308.

II. Free Farm Training in Great Britain or in the Dominions.

Great Britain.

1. Brogborough Park Farm, Ridgemont, Bedfordshire.—A short intensive course of Canadian farming methods of four to ten weeks according to circumstances. (See page 300.)

   Apply to the Hudson's Bay Company Overseas Settlement Department, Trafalgar Buildings, 1 Charing Cross, W.C. 2.
2. Newbiggin, North Seaton Hall.—A short training course of six to twelve weeks mainly for testing a boy's suitability and adaptability for farming.

   Apply to Secretary, Northumberland and Durham Empire Settlement Committee, Quebec Chambers, Leeds.
3. Ham Green Farm, Bristol.—A short course of five to twelve weeks for same purpose as above.

   Apply to Secretary, Bristol and West of England Migration Committee, Tontine Buildings, Colston Avenue, Bristol.

4. Carr Hall Farm, Burscough.
   Apply to Secretary, Migration Committee, 27 Leece Street, Liverpool.
5. Cossar Boys' Training Farm, Craigielinn, near Paisley.
   Apply to Dr. G. C. Cossar, 23 Monteith Row, Glasgow.

Canada.

1. Macdonald Agricultural College—incorporated with McGill University.
   In co-operation with the Canadian Pacific Railway Company Immigration Department, offer a two years' Diploma course of free tuition; board and residence about £51 per annum. (See page 298.)
   During the summer months farm employment is arranged, students receiving a small wage; board and lodgings free.
2. Guelph Agricultural College, Ontario; affiliated with Toronto University.
   A scheme promoted by the Ontario Government; affords a training in scientific and practical farming at a cost of £50 for tuition, board and residence. During the summer months farm employment is arranged with farmers in Ontario from whom the students receive wages, board and lodgings. (See page 298.)
   Apply for full information to Ontario Government Office, 163 Strand, W.C. 2.

South Africa.

The 1820 Settlers Memorial Association offers a free course of training to public or approved secondary school boys with a guaranteed minimum capital of £1,000. Apply to Secretary of Association, 199 Piccadilly, W. 1.

III.—Assured Employment Overseas with Selected Farmers.

1. Church of England Council of Empire Settlement, in co-operation with the Council for Social Service of the Church of England in Canada, has established hostels in Alberta and Saskatchewan for the reception of boys aged 14 to 19. Boys selected travel free from the port of embarkation in the United Kingdom to the hostel and under proper supervision. They are then placed in employment with farmers approved by the superintendent of the hostel at the standard rate of wages plus board and lodgings. The Council of Social Service helps and advises the boys and guards their interest during their first years in Canada.

2. The Vimy Ridge Scheme, promoted by the Ontario Government.—Approved boys, 15 to 19 years of age and approved by the Ontario Government, are granted free passages from their homes in the United Kingdom to the Vimy Ridge Reception Farm near Guelph, Ontario. From this centre they are placed with approved farmers, from whom they receive the current wages plus board and lodgings. The Ontario Government furnishes after-care, advice and protection for a period of at least three years after arrival. Owing to the present depression this scheme is closing down in 1932.

3. The Provincial Governments of Nova Scotia, New Brunswick, and Manitoba have established schemes similar to the Vimy Ridge Scheme, viz., free passage, reception farms and assured employment and after-care.
   Apply to—
   Ontario Government Offices, 163 Strand, W.C. 2.
   Nova Scotia Government Offices, 2 Cockspur Street, S.W. 1.
   New Brunswick Government Offices, Canadian Building, Trafalgar Square, S.W. 1.
   Manitoba Government Offices, Canadian Building, Trafalgar Square, S.W. 1.

4. The Church of England Boys Scheme.—The Church of England authorities in New Zealand arrange employment on farms for boys between the ages of 15 and 19, preferably boys from secondary schools who are selected by the Church of England Council of Empire Settlement in Great Britain. Such boys, if approved by the High Commissioner, receive free passages and travel under the care of a conductor appointed by the Council.

5. The Queensland, Western Australia, and Canada £300 Public School Scheme.—The Church of England committees in Queensland, Western Australia, and Canada can place young men who will have a minimum capital of £300 after

they have gained sufficient experience of farming. The lads must be between 18 and 24 years of age, and are placed with farmers selected by the Church Committee, where they will learn the work for such a period as the Committee consider necessary. This would be for at least $1\frac{1}{2}$ years, and the farmer will send reports to the Committee as to the young man's progress from time to time. He would receive the current rate of wages for an unskilled worker, a wage which will increase as his value as a worker increases. Half his wages will be banked for him while under 21, or longer if desired.

As soon as the young men are considered to be capable of starting on their own account the Committee will inform the parents that they are ready to make use of the capital, and will definitely advise and arrange for the settlers to lay it out to the best advantage.

IV.—Financial Help.

1. Government Loans for purchase and equipment of farms in Canada:
   Loans up to the value of £500 are obtainable from the British and Canadian Governments under certain conditions. (See page 297.)
   Full particulars can be obtained from the Oversea Settlement Department, Caxton House, Tothill Street, S.W. 1.
2. Agricultural Credits for Farmers:
   The Canadian Farm Loan Board is empowered to make loans to qualified farmers up to a maximum of
   - 50 per cent. of the value of agricultural lands, and
   - 20 per cent. of the value of the permanent insured improvements thereon, such as buildings,

   repayable by equal annual or six-monthly instalments over a period of 36 years. Particulars can be obtained from the Oversea Settlement Department.

APPENDIX III.

Types of Secondary Schools of England and Wales.

Memorandum by Sir Richard Gregory.

In previous reports of this Committee particulars have been given of some schools and courses specially adapted for training for life overseas. During the meeting of the British Association in South Africa in 1929 members of the Committee and other members of the Association were frequently asked where information could be obtained as to various types of secondary schools at home to which residents abroad might send their children. It is obviously impossible for this Committee to attempt to make a selected list of such schools where particular attention is given to practical science or outdoor training.[8] A useful purpose may be served, however, by a statement of the various types of secondary schools, with references to volumes from which details may be obtained concerning them. It is believed that the subjoined memorandum will be found of service not only abroad but also at home.

According to the definition of the Board of Education a secondary school must provide a progressive course of general education of a kind and amount suited to an age range of at least from 12 to 17 years. Within these limits are to be found a number of types of school, differing from each other in various respects but not so clearly differentiated from each other that it is always possible to be certain of the type to which any individual school should be allotted.

The first important dividing line is between those schools which receive financial assistance from the State and those which do not receive such assistance. Schools of both kinds are to be found in the list of secondary schools which are recognised by the Board of Education as efficient (List 60). This list, including both boys' and girls' schools, was first published in 1908 for the Board's own use, but schools soon began to discover the advantages to be acquired from the hall-mark which it gave. More and more of the great endowed schools took steps to be included in the list, and it now includes also a large proportion of the reputable private schools of the country. Inclusion implies no loss of autonomy, the only condition being that the school must have received a favourable report from the Board's inspectors. On the

[8] The curricula for several such schools are described in the Reports of the British Association Committee on Educational Training for Overseas Life, 1925 and 1929.

other hand, omission from the list does not imply inefficiency. For example, Harrow and Rugby are both in the list; Eton is not. The latest edition of the list (1930–31) contains the names of more than 1,550 secondary schools. It gives the name of the responsible body, the head master or mistress, the fees for tuition and boarding, the number of day pupils and of boarders, and the examinations for which pupils are prepared. There is a similar List 60 for Wales.

Although not coming strictly within the definition of a secondary school, the preparatory schools may be considered as an appendage to the secondary school system. To be recognised by the Board as efficient they must provide a general education suited to an age range between 8 and 13 years, and be schools from which pupils normally proceed to a secondary school or other similar institution.

Alongside the secondary schools, in the general acceptance of the term, are a number of schools and institutions which fill a definite place in the secondary education system of the country, but the aim of which is to prepare boys and girls for a technical, industrial or commercial career. In this class are technical day schools, junior technical, art and housewifery schools, and farm institutes.

The various types of school providing secondary education are described below, those for boys being given first.

Boys' Schools.

1. *Public Schools.*

These are generally considered as the bright particular jewel of the English educational system. With few exceptions they are boarding schools, though many of them admit day boys from the immediate neighbourhood. The traditional curriculum is largely classical, but in nearly all public schools nowadays due attention is paid to science and modern languages. The technical hall-mark of a public school is that its headmaster is a member of the Headmasters' Conference, and in addition to the nine great public schools there are some sixty others scarcely less famous scattered throughout the country, together with about forty of the older grammar schools, which maintain such close relationship with Oxford and Cambridge that their headmasters have been admitted to membership of the Conference.

The pupils at the public schools proper enter usually at the age of 12 or 13 from preparatory schools. The high fees charged practically confine the membership to children of the well-to-do classes, though some scholarships are given. The schools are governed by independent corporations and exist on high fees and rich endowments. The fees vary roughly between £100 and £200 a year for boarders and between £20 and £60 a year for day boys, though in a few cases they are higher. In the case of a number of public schools, circumstances exist which tend to give a bias in some particular direction. For example, the governing body may be connected to some religious denomination, or the school may have been founded for the benefit of the sons of members of some particular profession, such as the Army or the Church, but in many of these schools the original scheme has been widened, and pupils who do not fulfil the original conditions are now accepted. Full details on all the points mentioned may be found in the 'Public Schools' Year Book.'

2. *Grammar and Endowed Schools.*

These are endowed schools mainly of old foundation, and are to be found in most historic towns and even in some villages. Their title often indicates the period of their origin or the name of their pious founder. Many of the older schools are combined day and boarding schools, and they are distinguished from the public schools chiefly in that they acquire a local character and tradition and adapt themselves more readily to changes in educational ideals and methods. The original predominance of the classics has largely disappeared, and they now give a due place to modern languages, science and commercial subjects. While a number are still independent, the majority now receive aid from the State or the local education authorities, and there is a tendency for them to pass in consequence more and more under the control of the Board of Education and the local authority. This is particularly so in the case of the day schools, and there are probably not more than a dozen secondary day schools for boys which do not receive aid from public funds. For day boys the fees range from £10 to £30 a year, anything above £30 being exceptional. For boarders the total fees usually lie between £50 and £100 a year.

The normal age of entry is 11, but many schools have a preparatory department which boys may enter at 7 or 8. Pupils may remain to the age of 18 or 19.

Brief particulars of most of these schools can be found in the 'Schoolmasters' Year Book.'

3. *County and Municipal Secondary Schools.*

Most of these schools have been established since 1902, when the Education Act enabled local authorities to make provision for secondary education within their areas. They have been erected at the public expense, and are maintained and controlled by public bodies. A few of them are of older foundation, but owing to financial difficulties have been taken over entirely by the local authorities. Their pupils come mainly from the elementary schools at the age of 11 or 12. These schools are destined to play an increasingly important part in the secondary education of the future. They tend to model their policy and practice on those of the grammar schools, but in some areas their development is hampered by an excessive amount of control imposed upon them by the local authority. Many of them are co-educational. A few of them are entirely free, and where fees are charged they vary between £9 and £20 a year. The only boarding schools among them are a few which were originally endowed schools but have now been taken over. All schools which receive State aid must admit a certain proportion of non-fee-paying pupils, normally 25 per cent.

Their names and brief particulars of them all appear in List 60 and in the 'Schoolmasters' Year Book.'

4. *Welsh Intermediate Schools.*

These schools were set up under the Welsh Intermediate Act of 1889, and are comparable to the newer endowed schools of England. They receive both rate and State aid, and are inspected by the Welsh Department of the Board of Education. Information regarding them is given in List 60 (Wales) and in the 'Schoolmasters' Year Book.'

5. *Preparatory Schools.*

The majority of preparatory schools are under private ownership, some few being managed by corporations, but all are independent of State control and receive no money from public sources. Consequently they have to charge substantial fees. They deal only with young boys who will go on, not later than the age of 14, to other schools. In their most characteristic form they are boarding schools, although in large towns preparatory day schools also exist. The number of these schools which have a just claim to the title 'Preparatory' is estimated at over 700, and they are practically the only gateway by which boys may enter the public schools. They show a good deal of variety in the ages and number of their pupils, in their buildings and equipment, and in their fees. The education they provide is partly elementary but largely secondary, and they endeavour to supply a training in full sympathy with public school ideals for boys under the age of 13. Some few have sought inspection by the Board of Education, and appear as efficient schools in List 60. The fees vary between £100 and £200 a year.

A list of preparatory schools which prepare boys for the public schools is given in the 'Public Schools' Year Book,' but the list is not exhaustive. Fuller details of many of them are given in Paton's List of Schools and in 'Schools,' published by Messrs. Truman & Knightley.

6. *Private Schools.*

These schools number many thousands and are of all grades of efficiency. They are subject to no external control or inspection, and although some of them compare favourably with good secondary schools, many are undoubtedly inefficient, judged by modern standards. A few are on List 60.

7. *Junior Technical Schools, &c.*

While these schools are not administratively classed as secondary, they fill a definite place in the system of secondary education. They have a minimum age limit for entrance, generally 13 or 14, and they provide full-time education for two or three years which is partly general and partly a preparation for industrial or commercial employment. Similar courses are provided in Art Schools or in Junior Art Departments attached to Art Schools. In the former, pupils may enter at 14 and take full-time courses providing instruction in Drawing, Artistic Handicraft and

Design, together with literary and pedagogic subjects. In the Junior Art Departments, which provide general as well as Art Education, pupils may not remain beyond the age of 16. The pupils generally enter from the elementary schools, and the institutions are controlled by Local Education Authorities and receive both State and rate aid. Full particulars are contained in Board of Education List 111. This list contains also particulars of the six Schools of Nautical Training which provide full-time education, generally for two years, for pupils from elementary schools in preparation for employment at sea.

8. *Farm Institutes.*

The main purpose of a Farm Institute Course is to provide instruction in the scientific principles underlying sound practice. Most of the courses start in October and the full agricultural course generally covers two terms, one before Christmas and one after. In addition there are full courses in Dairying, Poultry-keeping, General Horticulture, &c., and also short courses in special subjects. The Institutes are maintained by County Councils, and the fees payable by residents in the County vary from £1 to £1 15s. 0d. a week for board, lodging and tuition. For students from other counties the fees are higher. A limited number of scholarships are awarded. For full details of the courses, see Form 732/TE and Leaflet No. 197 published by the Ministry of Agriculture.

GIRLS' SCHOOLS.

Broadly speaking, the same general types of secondary school are available for girls as for boys. The term 'public' as applied to girls' secondary schools is wider than it is in the case of boys' schools and is generally taken to mean a school which is controlled by a Governing Body as distinct from one run for private profit. There is no body corresponding to the Headmasters' Conference, and Public Secondary Schools for Girls include boarding schools and day schools controlled by independent authorities, grammar and endowed schools, and schools maintained and aided by Local Education Authorities.

1. *Public Schools.*

The girls' schools which correspond to the large boys' Public Schools are much fewer in number and of much more recent origin. The oldest of them date from the latter half of the nineteenth century and were organised on the model of the boys' Public Schools, with their house system and prefects and games. One main difference is that they take pupils from an earlier age and most of them have junior departments and kindergartens, thus making a separate class of Preparatory Schools unnecessary. The fees are little below those charged in boys' Public Schools.

Many of the independent schools are combined boarding and day schools, *e.g.* some of those controlled by the Girls' Public Day School Trust. The fees for day pupils vary between £10 and £40 a year, with a few as high as £60 ; and for boarders from £60 to £150 a year.

2. *Grammar and Endowed Schools.*

What was said about the Grammar and Endowed Schools for boys applies also to those for girls.

3. *County and Municipal Schools.*

With few exceptions, and those are where existing schools have been taken over by a local authority, these schools are day schools. Some are entirely free, and the fees in the others range from £10 to £20 a year. Pupils who live outside the area of the Local Education Authority which maintains the school are charged higher fees, in some cases £39 a year.

There are also a few Welsh Intermediate Schools for Girls.

A list of schools of all these types, with particulars of the more important of them, is given in the 'Girls' School Year Book,' the official book of reference of the Association of Headmistresses. Those which have submitted to Board of Education inspection are in List 60.

4. *Private Schools.*

Their name is legion and they are of all varieties, ranging from those which are conducted on Public School lines, many of which are in List 60, to those which apparently give no education at all. Particulars of a number of the more reputable

private schools are given in the Directory of Women Teachers and in Paton's and Truman and Knightley's lists.

5. *Junior Technical Schools, &c.*

Some of the Junior Technical Schools are for girls only and some are mixed. They give training for Dressmaking, Millinery and other industrial, commercial and domestic occupations, under the same conditions as those for boys. In addition there are about twelve Junior Housewifery Schools which provide full-time courses varying in length from one to three years. The Art Schools and Junior Art Departments are also open to girls. (See Board of Education List 111.)

6. *Farm Institute Courses.*

Most Farm Institutes provide courses for girls, especially in Dairying, Horticulture and Poultry-keeping. (See Ministry of Agriculture Form No. 732/TE.)

Co-educational Schools.

Many of the County and Municipal Schools and fewer of the Grammar and Endowed Schools are co-educational. These, almost without exception, are under headmasters. There are also a few co-educational boarding schools of the Public School type and many co-educational private schools, both day and boarding.

SUMMARY OF SECONDARY SCHOOL TYPES.

Public Schools.	Generally independent, occasionally grant earning and sometimes aided by Local Education Authorities.
Schools supported by Guilds and Companies, Educational Trusts and Religious Bodies.	Do. do.
Private Schools.	Independent.
Preparatory Schools.	Independent.
Grammar and Endowed Schools (including Welsh Intermediate Schools).	(*a*) Independent. (*b*) Semi-independent with State or Rate aid or both.
County and Municipal Secondary Schools (and Technical Schools).	Fully controlled and maintained by Local Education Authority, with State aid.

PUBLICATIONS MENTIONED IN THE TEXT.

Board of Education List 60.	H.M. Stationery Office. Price 3/6.
,, ,, ,, (Wales).	,, ,, ,, 1/6.
The Public Schools' Year Book.	The Year Book Press, Ltd., 31 Museum Street, W.C. 1. 10/6.
The Girls' School Year Book.	The Year Book Press. 7/6.
The Schoolmasters' Year Book.	The Year Book Press. 35/-.
The Directory of Women Teachers.	The Year Book Press. 25/-.
Schools.	Truman and Knightley, Ltd., 61 Conduit Street, W. 1. 2/6.
Paton's List of Schools.	J. & J. Paton, 143 Cannon Street, E.C. 4. 5/-.
Board of Education List 111.	H.M. Stationery Office. 4/-.
Form No. 732/TE and Leaflet No. 197.	Board of Agriculture and Fisheries, 10 Whitehall Place, S.W. 1. 1d.

Educational and Documentary Films.—*Report of Committee* (Sir RICHARD GREGORY, *Chairman*; Mr. J. L. HOLLAND, *Secretary*; Mr. L. BROOKS, Miss E. R. CONWAY, Mr. J. S. DOW, Mr. G. D. DUNKERLEY, Dr. B. A. KEEN, Dr. C. W. KIMMINS, Mr. R. S. LAMBERT, Mr. A. E. MUNBY, Prof. J. L. MYRES, Mr. G. W. OLIVE, Mr. G. N. POCOCK, Dr. T. SLATER PRICE, Prof. C. SPEARMAN, Dr. H. HAMSHAW THOMAS) *appointed with the following reference: Educational and Documentary Films: To enquire into the production and distribution thereof, to consider the use and effects of films on pupils of school age and older students, and to co-operate with other bodies which are studying those problems.*

SINCE the first Report of the Committee was presented at the Bristol meeting of the Association, there has been a considerable quickening of those activities which aim at the promotion of the use of cinematography as an aid to education. The Commission on Educational and Cultural Films have, amongst other things, been testing the various projectors now on the market through a special Sub-Committee on which the Committee now reporting is represented. The Report of this Sub-Committee, which may be expected shortly, should be of service to Authorities and Teachers who have to select projectors for School and Institutional use. Another of the Commission's Sub-Committees has been examining, in association with certain educational societies, such as the Geographical Association, the available films of standard size with a view to the compilation of a catalogue of those which can be considered suitable for educational purposes.

Various experiments have also been set on foot for the testing of films under school conditions of which the most important is that organised jointly by the Middlesex County and Part III Authorities and the Teachers' Associations of the area for an extended trial of sound films.

Mention should also be made of the Report of Investigations by the Birmingham Cinema Enquiry Committee under the Presidency of Sir Charles Grant Robertson, the President of the Educational Science Section of the Association for this Centenary meeting. In this Report a large amount of first-hand evidence of the effect of ordinary films upon children and adolescents has been sifted and collated. It throws vivid side lights upon the relations of education and cinematography.

The Committee believe that their first Report on certain technical questions connected with films and apparatus and their use under school conditions has also contributed to the general quickening of interest. Copies of the Report have been sent to the Local Education Authorities and, through the agency of the Educational and Cultural Films Commission, to all schools in which it is known that the cinematograph is used. Some 600 copies of the Report have been distributed in this way. A wider audience has also been reached through the re-publication of considerable extracts from the Report in educational periodicals.

Progress can also be recorded in technical matters, among which the manufacture of film is, perhaps, the most important. The ordinary film of commerce is made on a nitrate base and is unsuitable for educational use, owing to its dangerous inflammability. Sub-standard film, in this country at least, is practically always of the non-flam acetate type. In those sizes the film material lends itself to amateur handling, but in the full standard 35 mm. size it is liable to certain disadvantages in use, mainly mechanical.

For the purpose of the new Spicer-Dufay process of colour-cum-sound cinematography a film base of the acetate type is being manufactured which is claimed to be absolutely non-inflammable and to retain its suppleness and whiteness indefinitely. The Spicer-Dufay process itself as exhibited at the two conversaziones of the Royal Society this year, and described in *Nature* of May 30th, is full of educational interest and promise.

The Committee have taken as their special province the use of the film for classroom purposes. There is a growing demand for standard films and apparatus which can be used with comparatively large audiences. But the Committee believe that the educational value of cinematography will not be fully realised until sub-standard

apparatus and films—which they suggest should usually be of 16 mm. size—are available in all schools for use with single forms and classes. Until the projector is recognised as a classroom instrument it cannot be expected that educational films will be made available in sufficient variety. Moreover, the Committee wish to see the taking of films by teachers themselves for their own special purposes become a reasonable and usual practice.

In furtherance of these objects the Committee had hoped that their present Report would take the form of a catalogue of sub-standard 16 mm. films already available for teachers in this country. They find, however, that such a catalogue under present conditions can be little more than a repetition of the catalogue of the Corporation which has hitherto played the leading part in the introduction of sub-standard apparatus. They understand, however, that the other principal Corporations engaged in the cinematographic industry in this country are waiting for a lead from the educational authorities and that one at least is now producing educational sub-standard films. The work of the Film Selection Sub-Committee of the Educational and Cultural Films Commission will be of great assistance in this matter, as approved standard films can be easily and cheaply reduced to sub-standard size without serious loss of educational advantage. The Committee are, therefore, satisfied that the attempt to produce a catalogue at this time would be premature. But if they are continued in existence, as they recommend, they hope to return to this branch of the subject at a later date. In this connection it is, the Committee would suggest, much to be desired that, for the present at any rate, the principal sub-standard size recognised by the trade, namely 16 mm., should be adhered to in the preparation of new films for exclusively classroom use.

During the forthcoming winter the Committee think that they can usefully arrange for an experiment on an extended scale in the use of selected sub-standard silent films under ordinary classroom conditions in urban and rural schools. They already have a promise of assistance, if this proposal is carried out, from an important County Borough, and they anticipate no difficulty in obtaining the co-operation of a County Local Education Authority with small country schools under its control. They are satisfied that there is room for an experiment of this kind, since those so far conducted have almost invariably been with apparatus and films of standard size exhibited to grouped classes in school halls rather than in the ordinary classrooms.

In the selection of the particular apparatus to be used, the Committee hope to benefit by the work of the Projector Sub-Committee of the Educational and Cultural Films Commission, and as regards the choice of the films to be tried out, they propose that the Committee be re-constituted so as to include a sufficient proportion of teachers and others with actual educational experience.

SECTIONAL TRANSACTIONS.

(For reference to the publication elsewhere of communications entered in the following lists of transactions, see end of volume, preceding index.)

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCES.

Thursday, September 24.

PRESIDENTIAL ADDRESS by Sir J. J. THOMSON, O.M., F.R.S., on *The Growth in Opportunities for Education and Research in Physics in the past Fifty Years.* (See page 19.)

Prof. H. L. BROSE.—*The Cross-section of Gas-Molecules with respect to very slow Electrons.*

The effective cross-sections (Q) of the molecules of the saturated hydrocarbons with respect to slow electrons have been stated by Brüche to vary to an extent comparable with that which occurs in the case of argon. By using Townsend's diffusion method for determining the Q-$\sqrt{v}$-curves it has been possible to establish that the values of the Townsend temperature factor k for electrons and of their drift velocity W are widely different from the corresponding values for argon. When, however, the values of the cross-sections are calculated from the values of k and W by means of the formula $Q = \frac{Z/p}{8{\cdot}10^9 . W\sqrt{k}}$ it is found that the Q-$\sqrt{v}$-curves assume an argon-like character. Investigation thus shows that in spite of this similarity, the collisions of electrons with molecules of the methane series of gases are very different from those between electrons and argon atoms. This is borne out by the λ-$\sqrt{v}$-curves, where λ denotes the average fractional loss of energy of the electrons during collisions and v, as above, denotes the volt velocity of the electrons. The results for methane and ethane are discussed in detail.

Prof. G. P. THOMSON.—*Some recent Experiments on Electron Diffraction.*

In these experiments the diffracted cathode rays are recorded on a photographic plate ; the markings on the plate when homogeneous cathode rays are used depend on the nature of the diffracting crystal or polycrystalline aggregate, according to the laws of wave optics. The etched surface of a single crystal of metal gives a pattern of spots, which is regular if the surface is crystallographically simple. The experiment is the analogue with electron waves of a well-known optical experiment with crossed gratings. To explain it, the surface must be covered with small lumps of metal of the order 10^{-6} cm. in size. It is apparently very difficult to prepare a surface of metal of smoothness comparable to that of the cleavage face of an ionic crystal. Thus rocksalt shows quite a different type of diffraction, characterised by a few spots, and numerous lines of the kind described by Kikuchi. In both cases the results are in full agreement with the de Broglie theory of electron waves, and no abnormalities appear, such as the spectra of half orders which have been reported by some experimenters using slow electrons.

Some other results indicate the possibility of using electron diffraction to measure the size of crystal aggregates on thin surface layers. This may have interest in the study of catalysts.

Prof. J. C. McLennan, F.R.S.—*On the Moments of Atomic Nuclei.*

Spectroscopic investigations in progress have already yielded results that enable one to calculate the spin moments of the nuclei of the atoms of certain of the elements. The values thus obtained are being related now to such factors as atomic weight and atomic number. The author, with his collaborators in recent work on the fine structure of several prominent lines in the second spark spectrum of lead, PbIII, has found that these structures can be given a consistent interpretation by assuming the two values, 0 and $\frac{1}{2}$, for 'I,' the quantum number representing the spin moment of the nuclei of lead atoms. Intensity relations between the observed fine structure components indicate that $I=\frac{1}{2}$ gives the spin moment corresponding to the Pb_{207} isotope and $I=0$ that corresponding to the Pb_{206} and Pb_{208} isotopes. Hyperfine structure data derivable from a study of thallium, lead and bismuth spectral lines enable one to show the inadequacy of certain theories now before us. Constants of interaction-relationships for nuclear and electronic angular moments can be calculated, nuclear magnetic moments can be evaluated, and 'g' values (ratios of magnetic to mechanical moments) can be determined from the data accumulated. These furnish evidence which appears to show that the mechanical moments found for atomic nuclei are the resultants of angular momenta originating in the spin of the protons within atomic nuclei and in some other motion, probably orbital, of these protons.

Prof. M. N. Saha.—*The Interpretation of the Absorption Spectra of Silver Halides.* (In title.)

In this paper the view is put forward that the molecules of silver halogenides in the vapour state form ionic-compounds, and not atom-compounds, as postulated by Franck. The normal state is given by the combination Ag^+Cl^- where Ag^+ is in the $(4d)^{10}$ state. The excited state is also given by Ag^+Cl^-, but Ag^+ is now in the $(4d)^9 55$ state.

The $[U_{(r)}—r]$ curve for the neutral combination of AgCl has a hyperbolic shape, which accounts for the fact that hv_0 (where v_0 = limit of absorption frequency) is $>R$, the atomic heat of dissociation. Arguments for and against the two views are discussed, and new experiments are proposed to decide between the two views.

Dr. W. F. G. Swann.—*The Significance of Mass in Wave Mechanics.*

In the Hamiltonian Function which forms the basis of the Ψ equation, the constants m_1, m_2, &c., corresponding to the masses of the protons and electrons of an atom in the classical sense, make their appearance in a new rôle which can be seen to be the equivalent of their old rôle in classical theory only when the motion of the atom as a whole is studied in the wave mechanical sense. If, in the Hamiltonian Function, we include the scalar and vector potentials of electrodynamics as calculated from the $\Psi\overline{\Psi}$ distributions of the atom regarded as charge densities, the simple mass $m_1+m_2+\ldots$, &c., of the entity in the dynamical sense becomes increased (or decreased) by a quantity which turns out to be the electromagnetic mass of the $\Psi\overline{\Psi}$ distributions calculated according to electromagnetic principles. In this way, contrary to the ordinary electromagnetic theory of mass, the main part of the mass, the part determined by m_1, m_2, &c., has a mathematical origin different from that of the mutual mass, and is not obtainable, as in classical theory, from a mere extension of the electromagnetic principles inherent in mutual mass to the charges as a whole. The phenomena of mutual mass and of 'packing fractions' become susceptible of development without the assumption of an electron of dimensions comparable with 10^{-13} cm., with its attending difficulties. A certain latitude is open as to the way in which the values of $\Psi\overline{\Psi}$ for the positive and negative charges contribute to the Hamiltonian occurring in the Ψ equation; and, by a reasonable adjustment of these assumptions, it is possible to account for the form of the 'mass defect' curve, and for the numerical change of mass defect with atomic number, particularly in the vicinity of the higher atomic numbers.

Friday, September 25.

DISCUSSION on *Geo-physical Methods of Prospecting, with special reference to Instruments.* (Prof. A. S. EVE, C.B.E., F.R.S.; Prof. A. O. RANKINE; Prof. MINTROP, *Seismic Methods*; Mr. KARL SUNDBERG, *Electrical Methods*; Mr. A. BROUGHTON EDGE, *Spontaneous Polarisation Method*; Dr. J. H. JONES, *Portable Seismographs*; Mr. LANCASTER JONES, *Gravity Methods*; Capt. SHAW, *Magnetic Methods*.)

Mr. A. B. BROUGHTON EDGE.—*Some Recent Developments in Electrical Prospecting Instruments.*

During the past twelve months steps have been taken in this country, under the auspices of the Department of Scientific and Industrial Research and with the co-operation of British instrument manufacturers, to produce a series of reliable electrical prospecting instruments and the auxiliary equipment required for their use in the field. The experience gained in Australia during 1928–30 by the Imperial Geophysical Experimental Survey has been applied to this purpose, particularly with regard to details of design which are essential for work carried out under rigorous field conditions. Amongst the instruments already completed the following possess features of interest:—

Spontaneous Polarisation Method.—For the investigation of naturally occurring electrical phenomena, such as arise in the neighbourhood of certain types of sulphide ore-body, a portable D.C. potentiometer and non-polarising electrodes have been designed. The potentiometer has two ranges of 0–50 and 0–100 m. volts, is self-standardising, and can be used on a tripod without any special attention to levelling. The non-polarising electrodes, with which contact is made with the ground, are either of the usual copper-copper sulphate type or else are of silver immersed in a chloride or sulphate electrolyte—the salts of potassium or sodium being employed as a rule. The electrolyte is contained in a porous pot, and by preparing it in the form of a jelly the electrode may be kept in use for long periods without requiring attention. For valuable advice concerning the preparation and characteristics of silver-sodium chloride electrodes special acknowledgment is due to the Admiralty Research Department at Teddington.

Resistivity Method.—It is commonly held that for the measurement of earth resistivities it is preferable to employ alternating rather than direct current, and in view of the natural earth potentials and the electrode polarisation e.m.f.'s that have to be contended with when D.C. is used, there would seem to be good grounds for this view. In practice, however, the use of A.C. or reversed D.C. has its disadvantages, one of which is that the resistivity values derived are dependent on the frequency employed, and in some cases upon other factors which are not always appreciated. D.C. measurements are free from such objections, and can be made without difficulty if the potential electrodes employed are of the non-polarising type and if all extraneous potentials are balanced before the current circuit is closed. The potentiometer described for the spontaneous polarisation method is provided with a special auxiliary circuit for this purpose, and has been found to be fully effective for resistivity survey work.

Ratiometer Method.—Hitherto one of the principal difficulties experienced in surface potential electrical surveys, in which A.C. is used, has been to take proper account of the phase displacements which arise in the neighbourhood of concealed conductive deposits, and which are of prime importance when making a search for the actual position and extent of the latter. In order to overcome this difficulty a simple form of A.C. bridge or ratiometer has been devised which serves a variety of useful purposes. Potential and phase anomalies may be investigated along selected lines of traverse or a complete picture of the surface potential distribution may be obtained over a large area. In the latter case two distinct series of lines are followed over the ground surface and are known as *equipotential* (E.P.) and *equiquadrature* (E.Q.) lines respectively. These lines may either be traced directly over the ground with the ratiometer or they may be derived from observations made with the same instrument along a closed series of traverses. Since such a survey is so conducted that the potential spacing of the lines in each series is known, it follows that the surface ellipse at any

point is determined by the angle of intersection of the E.P. and E.Q. lines that pass through it and by the line intervals in its neighbourhood. For most practical purposes it will be possible, after a little experience, to appreciate the potential and phase anomalies and their practical significance by a simple inspection of the linear diagram that is provided by the two series of lines. It is confidently believed that this new method of representing the rather complex surface potential conditions that are met in practice will prove more satisfactory to the electrical prospector than any hitherto developed.

Dr. J. H. Jones.

Several types of seismograph have been designed for recording artificial earthquakes in connection with geophysical prospecting.

These instruments have to be robust, portable and highly sensitive, and the pendulum system must have a suitable periodic time and a convenient damping arrangement.

It is important to design the pendulum to give a maximum magnification of the ground movement at the end of the helm.

The motion of the helm is further magnified by some mechanical or electrical device.

There is a serious disadvantage common to all the mechanical methods. The coupling of the magnifying device to the pendulum introduces a constraint on the motion and reduces the periodic time of the system. A simple device for magnifying the motion of the pendulum has been used by the writer.

In this method a phosphor bronze strip, carrying a soft iron element and a small mirror, is attached to the end of the helm. The soft iron element is situated in the magnetic field of two small magnets, and is set in rotation by the motion of the pendulum.

The periodic time of the pendulum system is increased by the action of the magnetic field on the element and by simple adjustments of the magnets; both the period and magnification can be varied over wide limits.

Mr. Lancaster Jones.

The gravity gradiometer was evolved to meet the special conditions obtaining in an area, suspected of bearing iron ore, which was covered by the tide for the greater part of each day. As a very close network of stations had to be observed, an extremely portable and quick-acting gravity balance was necessary. This rapidity of action is obtained in the gradiometer by reducing the horizontal separation of the effective masses of the suspended system, and thereby its moment of inertia and period of oscillation. As a result the time necessary for a single observation is reduced by about one half, from fifty to twenty-five minutes. By a suitable rearrangement of the masses the system is made insensitive to curvature quantities, with the consequence that only three observations are necessary per station with a single-beam instrument, or two observations with a double-beam balance, as compared with the five and three observations respectively in the case of the ordinary balance. The total time necessary at each station is therefore further reduced proportionately.

Dr. Ezer Griffiths, F.R.S., and Mr. J. H. Awbery.—*The Thermophysical Properties of Refrigerants.*

For the construction of entropy-temperature charts for various fluids of interest in refrigeration it is necessary to have data concerning their physical constants over a wide range of temperature.

Under the auspices of the Engineering Committee of the Food Investigation Board we have measured the latent heat and specific volume (*i.e.* the volume per gm. of vapour, under saturation vapour pressure) of dichloroethylene and trichloroethylene, ethyl chloride and methyl chloride, sulphur dioxide, pentane and ether over a range of temperature.

Mr. J. H. Awbery and Dr. Ezer Griffiths, F.R.S.—*Humidity Measurements at Temperatures from 40° C. to 100° C.*

The subject of humidity measurement and control is rapidly assuming importance in many industries and, whilst existing tables are adequate for many purposes, they are not sufficiently extensive in range for some processes, such as timber drying.

The methods available for measuring humidity above 40°C. are the dew-point method, the wet and dry bulb hygrometer, and the gravimetric method in which the moisture is absorbed from the air and measured by weighing. Of these the dew-point method is often very troublesome in hot atmospheres, and the gravimetric method suffers from the disadvantages that the results are averages over a period of time, and are not available until some time after the observations have been taken. Thus, in most cases, the wet and dry bulb method would be the most useful, if reliable tables were available over the whole range ; in consequence, experiments have been carried out to compile a set of tables for this instrument, covering the range 40° to 100°C. dry bulb.

In these experiments, the wet and dry bulb instrument was compared with both the types mentioned above. They give absolute results, and in general excellent agreement between them was obtained, so that the true humidity at each observed value for the wet and dry bulb instrument may be taken as known accurately.

The observations have been reduced graphically to tabular form.

Sir Napier Shaw, F.R.S.—*Meteorology after the Century.*

1. The development of the physical aspect.

Reports, by a professor of natural philosophy, to the second meeting of the Association on the state of knowledge, deriding the ordinary meteorological practice, extolling the contributions of dynamics and physics and concluding, in a report to the tenth meeting, with an excellent account of meteorological optics.

The contributions of distinguished physicists, Daniell, Regnault, Mascart, and the radiologists ; Stefan's law.

A professor of mineralogy's gift of a place among the inductive sciences to mineralogy (in the history of inductive sciences) as the analytico-classificatory science—refused to meteorology because it was in fact the application of other recognised sciences to the atmosphere.

2. The development of the geographical aspect culminating in the introduction of the weather-map with forecasts and storm-warnings.

3. Sir F. Galton's endeavour, as General Secretary of the Association in 1865, to combine the physical and geographical aspects in the solution of the problem of weather. The principles of weather-sequence as set out by R. Abercromby. The dominance of sea-level pressure-distribution.

4. Sir Arthur Schuster's appeal to the seventy-first meeting, for the deliberate co-ordination of meteorological observations for definite scientific purposes, and for the specification of the probability of inferences.

5. The extension of knowledge by the exploration of the upper air in respect of pressure and motion, heat, water-vapour, light and sound.

6. The uncertainties of forecasting. The reconsideration of the principles of weather-sequence by the Norwegian school and otherwise.

7. The distribution of entropy, acting through gravity, and of kinetic energy, with the aid of the conservation of angular momentum, as the controlling factors of weather.

Mr. C. Carus-Wilson.—*Demonstration on Musical Sands.*

Musical sands of sea-beaches and those of inland aeolian accumulations. Some notable examples.

The conditions essential for the emission of musical notes from sand-grains. The production of musical sands artificially.

The effect of various types of plunger and vessel upon the intensity of the sounds emitted.

Conditions which render musical sands mute.

The rhythmic acceleration of vibrations following abrupt frictional retardation.

Monday, September 28.

Prof. P. Zeeman.—*Ionised Noble-gas Spectra and the g-sum Rule.*

The spectra of ionised neon, argon, krypton and xenon under magnetic influence have been investigated by Mr. Bakker, working under my direction in my laboratory.

A test was made of the so-called g-sum rule due to Pauli.

Prof. R. W. Wood.—*Notes on Recent Investigations in Optics.*

1. *Absorption Spectra of Solutions of Coloured Salts and Dyes in Liquid Ammonia.*—Certain coloured metallic salts are freely soluble in anhydrous liquid ammonia. The absorption bands are much narrower and blacker than in water solutions. The same is true for many aniline dyes. The broad band of continuous absorption in the ultra-violet of potassium permanganate breaks up into twelve components in ammonia solutions. In some cases the solid residue obtained by evaporation is insoluble in ammonia.

2. *Selective Temperature Emission.*—Thin rods of fused quartz are nearly non-luminous in the Bunsen flame, owing to the low value of the absorption coefficient. Heated to a higher temperature in the oxygen-coal gas flame, the emission of white light sets in suddenly, there being no 'red-hot' stage. This is due in part to the drift of the ultra-violet absorption band towards the violet, and in part to the shift of the region of the maximum of the emission curve towards the violet with rising temperature. The addition of a trace of neodymium to the quartz causes the emission of a remarkable spectrum of bright bands with almost black intervals between them.

3. Recent photographs of hyperfine structure of nine mercury lines with the 40-foot grating spectrograph, with some comments on the probable origin of a type of 'ghost' not previously discussed, which simulates fine structure.

4. Exhibition of stereo photographic models of electron motion in Stark effect.

Prof. Dr. Martin Knudsen.—*Radiometer Force.*

A long, narrow and thin band of platinum, black on one side, bright on the other, was placed in a large glass vessel containing gas at a low pressure p. The band could be heated electrically to a higher temperature T_1 (absolute) than the temperature T_0 of the vessel. If the pressure of the gas on the black side of the band be denoted by p' and on the bright side by p'', then p'-p'' is the radiometer pressure. The only reason why p' is greater than p'' must be that the coefficients of accommodation a are different on the two sides of the band.

The measurements at various pressures p have shown that the radiometer force under all circumstances is dependent on a, whence it follows that this quantity must always be considered in theories bearing upon radiometer forces if a is not properly eliminated.

From the kinetic theory of gases I have calculated the following expression, which should hold good for small values of p and T_1-T_0

$$p' - p'' = p\,\frac{T_1 - T_0}{4T_0}(a_t' - a_t'')$$

where a_t' and a_t'' are the values of a on the black and on the bright sides respectively of the band. The index t denotes that the translational energy only of the molecules is concerned. From this equation a_t'-a_t'' is found, since all other quantities have been measured.

The apparatus was also used for measurement of the heat transfer from a black platinum band and a bright one to the surrounding vessel, and from these measurements the values of a, viz. a' and a'', were determined for the two surfaces. In the values thus found both the translational and the internal energies of the molecules are concerned.

The experiments gave the following values :

for hydrogen by heat conduction . .	$a'-a''=0{,}420$
,, ,, ,, radiometer force . .	$a_t'-a_t''=0{,}415$
for helium by heat conduction . .	$a'-a''=0{,}498$
,, ,, ,, radiometer force . .	$a_t'-a_t''=0{,}512$

The measurements thus have given practically $a'-a''=a_t'-a_t''$ not only for the monatomic helium but also for the diatomic hydrogen, and it may be inferred that a for this latter gas has a value for the translational energy which does not appreciably differ from the value which a has for the internal energy.

Prof. N. Bohr.—*On Atomic Stability.*

The characteristic stability of atoms presents us, as well known, with a fundamental limitation in the applicability of the concepts on which the so-called classical physical theories rest. An analysis of this limitation will be attempted in connection with a discussion of the difficulties disclosed by the recent development of the theory of the electron, especially as regards the problems of nuclear constitution.

Discussion on *The Unit of Atomic Weight* (opened by Dr. F. W. Aston, F.R.S.).

Dr. F. W. Aston, F.R.S.

The idea conveyed by the words 'Atomic Weight' is one of the oldest and most fundamental in chemistry. It is simply the expression of the fact that elements combine with each other in fixed and definite proportions. Thanks to the outstanding accuracy of the balance among early scientific instruments of precision, some of these ratios, more correctly called 'Combining Weights,' could be fixed to about 1 per cent. over a century ago. Fifty years' work pushed the accuracy some ten times as far, and as it advanced yet further and no serious indication of variations appeared it is not surprising that atomic weights were described as natural constants and generally regarded as the weights of the atoms themselves.

Apart from mere theoretical speculation this idea was not seriously shaken till, in 1910, advances in radioactivity and positive ray analysis showed it to be on an insecure foundation. When, some ten years later, the mass-spectrograph had shown it to be quite untenable, chemists were forced to make an important decision in nomenclature. Should the terms 'Element' and 'Atomic Weight' be made to apply to the atoms themselves; in other words, should chlorine now be said to consist of two elements of atomic weights 35 and 37, or should the old meaning of the words be retained? As a member of the International Committee I was strongly in favour of the latter. This procedure was ultimately agreed to and has, I think, been justified. Since that time much painstaking research has been undertaken, on the one hand to find if the atomic weights of complex elements varied with their origin, and on the other to achieve separation of isotopes artificially. The results have proved conclusively that for all practical purposes the variation in nature is negligible, and that although partial separation can be achieved by extremely laborious operations the quantitative results are insignificant.

The present unit of atomic weight $O=16$, chosen upon chemical grounds, gives a scale upon which many of the most frequently occurring elements are sufficiently close to whole numbers for all ordinary calculations of analysis. From the purely chemical point of view the recent discovery that oxygen has isotopes is clearly of little importance. The practical substandards of atomic weight determination, chlorine, bromine and silver have long been known to be complex to a much more serious degree. As, in all probability, most chemists will be dealing for an indefinite time to come with the present mixed but constant quality of complex elements, it seems needless to alter a standard which has figured in chemical literature for so long a time.

The point of view of the physicist is an entirely different one. He is interested in the weights of the atoms themselves. Exact determination of the relative densities of gases is a step in this direction, but this involves purification, which is essentially a chemical operation. As a development of pure physics the subject is quite modern, for it is only during the last twenty years that any comparison of the weights of individual atoms has been possible. The original parabola method of Sir J. J. Thomson was, by 1912, made capable of distinguishing atoms differing by 10 per cent., and of comparing their weights with an accuracy of 1 per cent. This was sufficient to suggest, though not to prove, that neon had isotopes 20 and 22. The first mass-spectrograph (1919) had a resolving power of 1 in 130 and an accuracy of about 1 in

1,000. This instrument definitely proved the isotopic nature of neon, chlorine and many other elements and established the Whole Number Rule. The measurements with this apparatus were always referred to the line O^{16}. In the absence of any evidence to the contrary oxygen was assumed to be a simple element, so that this system of measurement made the new physical scale of the weights of atoms identical with the chemical scale of atomic weights. With but few exceptions the atomic weights deduced from mass spectra agreed well with those generally accepted, and since it was decided to retain the chemical meaning of the words, the numerous new species of atom were distinguished by the chemical symbol and their whole number weight, called the mass number.

The accuracy of 1 in 1,000 was high enough to indicate that variations from the whole number rule, expected theoretically from the nuclear packing, were present, notably in the exceptional case of hydrogen, but that higher accuracy would be necessary to measure them. In 1925 the second mass-spectrograph was built. This had a resolving power of 1 in 500, and an accuracy, in favourable cases, of 1 in 10,000. The first results obtained were described in the Bakerian Lecture of 1927. The percentage deviation of the weight of an atom from a whole number on the scale of $O^{16}=16$ was expressed in parts per 10,000 and called its packing fraction. These packing fractions when plotted against mass numbers, lie roughly on a hyperbolic curve.

During the last three years a photometric method of calculating the relative abundance of isotopes from the intensity of their lines on mass spectra has been used to determine the mean atomic weights of complex elements. In most cases these can be fixed to a few hundredths of a unit of atomic weight. The method is rapid (some 26 elements have already been done), and being in general independent of purity gives a valuable check on chemical determinations. The agreement in the majority of cases is quite satisfactory. The two most glaring discrepancies, those of Kr and Xe, have now been removed by density redeterminations. Further chemical work appears to be desirable on Se, Te, Cs and Os. In all, 62 of the non-radioactive elements have been examined for isotopes. In addition to the 62 species of atoms inferred to occur in these there have been discovered 101 by the mass-spectrograph, eight by other methods of positive ray analysis, and four by optical observations on band spectra, making a total of 175 atomic species in sixty-two elements.

In 1929 Giauque and Johnston made their surprising announcement that observations on oxygen band spectra indicated the presence of O^{17} and O^{18}. The quantitative relations to O^{16} are small, and of the order 10^{-4} and 10^{-3} respectively. Their measurement is still a matter of great difficulty. Naudé concludes that the combined effect of the isotopes makes a difference between the scale $O^{16}=16$ and the chemical scale of $1{\cdot}25\times10^{-4}$; Mecke gives the higher figure $2{\cdot}2\times10^{-4}$. In the calculations of chemical atomic weights from photometry of mass spectra I have used the former figure. The complexity of oxygen accentuates the fact that a unit founded on a chemical standard can never be really suitable for physics. We can never be quite certain that any element is absolutely simple, although it seems highly probable that this is the case with hydrogen and helium. For physical measurement it is essential to take some definite atom as our standard.

Of those proposed the most hopeful are: the Proton or the Hydrogen Atom; the Alpha Particle or the Helium Atom; the Oxygen Atom of mass number 16. Some idea of the numerical relations between scales derived from these and the present chemical scale can be obtained from the following table. The figures for the masses are purely illustrative and are based on the results of the mass-spectrograph. The mass of an electron is about 0·00054 on such scales.

	H=1	He=4	Chemical scale	$O^{16}=16$
H^1	**1·00000**	1·00723	1·00765	1·00778
He^4	3·97127	**4·00000**	4·00166	4·00216
O^{16}	15·8765	15·9914	15·9980	**16·0000**
Oxygen	15·8785	15·9933	**16·0000**	16·0020
Chlorine	35·189	35·443	35·458	35·462
Hg^{200}	198·392	199·908	199·991	200·016

It is obvious that the scale of H=1, although it would have the advantage of making all the packing fractions of the same sign, is quite inadmissible, since it throws the masses of heavy atoms right off their proper mass numbers. He=4 avoids this

defect and is the only possible standard which appears to offer the same chemical and physical scale. But this appearance can only rest on the unproved assumption that helium is a truly simple element. Helium cannot be used as a practical chemical standard for obvious reasons. Its density is not easy to fix and for mass-spectrograph purposes it is too near the low end of the scale. I question if the advantages of its adoption would offset the wholesale alteration of all accepted atomic weights.

The only physical data at present available for comparing the weights of atoms with high accuracy are band spectra and mass spectra. For the former an accuracy of 10^5 is already claimed, but as they can only be used to compare atoms of the same element with each other the general problem can only be attacked by the mass-spectrograph. I hope that in a year or two it will be possible to construct an instrument capable of an accuracy of 10^5. From experience gained in fixing the weights of over 80 atoms to an accuracy of about 10^4 it is possible to outline the general methods by which the mass scale will be laid down. First by means of the natural bracket O^{16}, $C^{12}H_4$ and the close ratio $O^{16++} : C^{12} = C^{12} : O^{16}H_2$, two independent methods, the ratio $C^{12} : O^{16}$ will be fixed with the highest certainty. The molecule CO_2 will now be compared with Hg^{198+++}, which will extend the scale by means of Hg^{++} and Hg^{+} practically as far as necessary in the upward direction. The extension downward to hydrogen may be made by the close ratios $C^{12} : O^{16++} = C^{12++} :$ He^1 and $He^4 : H_2 = H_2 : H$. The accuracy of the final value of H can be further checked by the ratio $O^{16} : C^{12} = He^4 : H_3$ and the difference between $C^{12}H_4$ and O^{16}, which is the most certain of all comparisons yet available. It is clear that for all this work the standard atom O^{16} is very much the most convenient. It has the additional advantages of lying near the geometrical mean of the series of atomic weights and at a convenient position on the packing fraction curve.

To sum up, I am in favour of retaining the present chemical scale unaltered. For the vast majority of his practical operations all the chemist requires is a list of numbers, accepted internationally, which can be guaranteed to be within 1 part in 1,000 of the true atomic weights. For the more fundamental requirements of physics the neutral atum of oxygen 16 appears to me to be the best standard. The disadvantage that the two scales differ by one or two parts in ten thousand and that this difference will be continually subject to revision I do not regard as very serious. The meaning underlying the chemical scale is so completely different from that underlying the physical one that confusion should be easily avoided by speaking of 'the atomic weight of chlorine' in the one case and 'the weight of the atom of chlorine 35' in the other.

Tuesday, September 29.

Discussion on *The Evolution of the Universe.* (Sir James Jeans, F.R.S.; Prof. E. A. Milne, M.B.E., F.R.S.; Prof. W. de Sitter; Prof. Sir A. S. Eddington, F.R.S.; Prof. R. A. Millikan; Rt. Rev. the Lord Bishop of Birmingham; Gen. the Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S.; M. l'Abbé Lemaître; Sir Oliver Lodge, F.R.S.) See Appendix.

Wednesday, September 30.

Dr. G. C. Simpson, C.B., F.R.S.—*The Second Polar Year.*

The year August 1882–August 1883 has come to be called the First Polar Year because during that year twelve nations co-operated to send into the Arctic and Antarctic regions special expeditions to make observations, chiefly in meteorology and terrestrial magnetism, according to a common plan. Great Britain joined with Canada and together they sent a party of Royal Engineers to make observations at Fort Rae on the Great Slave Lake. In all, twelve expeditions worked in the Arctic and Antarctic. The records obtained were extremely valuable and have been the foundation for practically all subsequent investigations into the physics of the atmosphere and terrestrial magnetism in polar regions.

As the fiftieth anniversary of this outstanding epoch in the study of physics approached, suggestions were made that it should be commemorated in some suitable

way, and Admiral Dominik, the President of the Deutsche Seewarte, put forward the definite proposal that the best way of commemorating the work of the First Polar Year would be to repeat it. The suggestion was taken up and approved by the International Meteorological Organisation, and they appointed an international commission to study the project. The International Union of Geodesy and Geophysics also adopted the suggestion with enthusiasm and appointed a sub-committee.

According to the plan which has received international approval, a number of stations will be occupied in polar regions, and so far as possible all those used in 1882–83 will be re-occupied. Owing to the improvement in transport most of the stations can now be reached without the employment of special ships. Therefore, instead of the large expeditions which went out in 1882, each station will be manned by a party of three or four who will reach their destination in most cases without any great difficulty. Up to the present fourteen nations have signified their intention of establishing stations in the Arctic, while it is hoped that one or two parties will proceed to the Antarctic. The scientific work will be directed to the investigation of the four great problems :

1. the lower atmosphere, in order to follow through the changes of the air in polar regions as part of the great circulation of the atmosphere ;

2. the investigation of the upper atmosphere—at present very little is known about the conditions in the stratosphere in high latitudes ;

3. the magnetic force, especially its regular changes and the conditions during magnetic storms ;

4. the frequency and distribution of the aurora, especially its height in the atmosphere. Special attention will be devoted to correlating the appearance of the aurora with the magnetic field of the earth.

The number of problems which have arisen during recent years in these four branches of knowledge is very great, and they cannot all be studied in one year. It has, therefore, been decided to concentrate on those problems which require simultaneous observations for one year over the whole polar cap. Problems which can be investigated at any one station at any convenient time are to be given a secondary place. Many new methods of investigation which were not available in 1882 will be adopted, chiefly the much greater use of self-recording instruments, the hourly observations of terrestrial magnetism being replaced by the records of magnetographs. The investigation of the upper atmosphere in polar regions gives rise to many difficult problems. In civilised countries balloons carrying instruments are sent up as high as possible. When the instruments fall they are usually found within the course of a few days and returned by the finder. The chance of recovering instruments in this way is practically nil in polar regions, therefore other methods have to be employed. Instruments which will automatically signal by radio the temperature and pressure at different heights have been developed and it is hoped will be extensively used, although they are very expensive.

Special observatories are to be established on mountain tops, when such exist near to stations established nearer sea level. Observations of temperature, humidity, wind and cloud motion from these mountain stations will give considerable information regarding the physical conditions of the upper atmosphere.

The methods developed by Prof. Carl Stormer for determining the height of the aurora through simultaneous photographs taken at the end of long base lines will be used at several stations, and it is hoped in this way that a great deal of information will be obtained regarding the physics of the aurora.

Thirty-five countries have signified their intention of taking part in the work of the Second Polar Year ; of these the following countries intend to send parties into polar regions, or establish new stations in high latitudes within their own territory : Austria, Canada, Denmark, Finland, France, Germany, Great Britain, Holland, Iceland, Japan, Norway, Russia, Sweden and the United States.

The Royal Society of London and the Royal Society of Edinburgh have established a committee to organise the effort of Great Britain. This committee provisionally suggested that Great Britain might re-establish the station at Fort Rae and an aerological station on Ben Nevis, while if funds were available a second station might be established in Canada. Government has promised to provide £10,000 which will be sufficient for establishing and maintaining the station at Fort Rae. If the remaining part of the programme is to be carried out, it will necessitate the provision of additional sums from private sources. At the moment the Committee is con-

centrating its efforts on Fort Rae. A scientific assistant from the Meteorological Office has already visited Fort Rae this summer and made preliminary arrangements for a party to be housed and fed next year. The party will consist of three scientific men, one observer and one mechanic. It will leave England in May 1932 and commence observing on August 1. Observations will continue until the end of August 1933 when the party will return to England.

A detailed plan of observations and a scheme for publishing the results have already been drawn up by the International Commission for the Polar Year 1932–33 to ensure that all stations will make comparable observations and publish them in uniform style.

Prof. DAYTON C. MILLER.—*Ether-drift Experiments in Cleveland in* 1930.

The Ether-Drift Interferometer, previously used on Mount Wilson in California from 1921 to 1926, has been remounted on the campus at Case School of Applied Science in Cleveland. Some minor improvements have been adopted, such as shock-absorbing pads on the supporting piers, to eliminate traffic vibrations, and added precautions have been taken to eliminate temperature disturbances. The methods of making and reducing observations are so devised as to remove the possibility of instrumental or terrestrial disturbances, and the observed effects seem to be cosmic in origin. As before, the interferometer has a sensitiveness represented by a light path of 214 feet, or about 130,000,000 wave-lengths of light. The numerical results are reliable to the hundredth part of a wave-length of light, corresponding to one-half of a kilometer per second of relative motion of the earth and the ether.

A series of experiments recently completed gives results wholly in accord with those previously obtained at Mount Wilson; the observed effect is such as would be produced by a relative motion of the earth and ether of about 10 kilometres per second. The direction of the indicated motion is fixed with relation to sidereal time, that is, it is towards a fixed point in space, as of a motion of the solar system towards the point having a right ascension of 17 hours, and a declination of 68 degrees north.

Ether-Drift produces an effect, as observed in the interferometer, which is proportional to the square of the ratio of the velocity of the cosmic motion of the earth and of the velocity of light; this is a 'second order' effect, and is periodic in each half revolution of the interferometer. The complete theory of this experiment, given by Hicks in 1902, shows that there is necessarily also present a *first order* effect, periodic in each full turn of the interferometer; this has *never before been taken into account*, but it is now shown that this effect is always present, and that it is in accord with the theory; and it is considered as further evidence of the validity of the present experimental results.

A discussion of the significance of these observations as compared with the recent work of other observers will be given.

It is interesting to note that the present interferometer is mounted about 300 feet from the location of the original Michelson-Morley interferometer of 1887. Contrary to a prevalent opinion, the original observations of Michelson and Morley gave a small positive effect which is almost exactly of the same magnitude as that obtained in the present experiment. Therefore the Michelson and Morley experiment fully agrees with the present results, though at the time it was differently interpreted.

Attention is called to the results of several recent important experiments in diverse fields which seem to corroborate the indicated cosmic motion of the solar system.

For table see next page.

Dr. CECILIA H. PAYNE and Dr. H. SHAPLEY.—*The Harvard Photographic Photometry.*

The paper contains a report on the first instalment of the Harvard catalogue of photographic magnitudes, which is designed to be complete to $8^{m}.25$ over the whole sky.

The photographic magnitudes are derived from plates taken with a 3-inch Ross lens, and measured with a Schilt thermoelectric micro-photometer.

The first instalment of the catalogue, from $+90°$ to $+65°$, has been completed; it contains about two thousand stars. A comparison has been made with the following catalogues that cover the same region of the sky: the Greenwich catalogues;

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	α	δ	v		
Ether Drift	17^h	68°	200	Miller	*Science* **63**, 433, 1926. *Astro. Phys. Jour.* **68**, 341, 1928.
Meridian Circle Observations .	$16\frac{1}{2}^h$	44°	—	Esclangon	*Comptes Rendus* **182**, 922, 1926.
Meridian Circle Observations .	18^h	33°	—	Courvoisier . . .	*Comptes Rendus* **182**, 923, 1926.
Lunar Occultations of Stars .	$16\frac{1}{2}^h$	45°	700	Esclangon . . .	*Comptes Rendus* **182**, 923, 1926.
Angle of Reflection . .	20^h	—	—	Esclangon . . .	*Comptes Rendus* **185**, 1593, 1927.
Earth Tides	$16\frac{3}{4}^h$	—	—	Esclangon . . .	*Comptes Rendus* **182**, 921, 1926.
Ocean Tides	$16\frac{1}{2}^h$	—	—	Esclangon . . .	*Comptes Rendus* **183**, 116, 1926.
Motions of B-type Stars . .	$20\frac{3}{4}^h$	46°	—	Plaskett	*Science* **71**, 152, 1930.
Interstellar Matter . . .	$20\frac{3}{4}^h$	46°	—	Shapley	*Nature* **122**, 482, 1928.
Star Clusters and Nebulæ .	21^h	65°	300	Stromberg . . .	*Astro. Phys. Jour.* **61**, 353, 1925.
Cosmic Radiation . . .	17^h	—	—	Kolhörster, Steinke, Büttner	*Zeit. für Physik* **50**, 808, 1928.
Cosmic Radiation . . .	17^h	—	—	Ship *Carnegie* . . .	Rept. Carnegie Inst. 1927–28, 255.
Radial Motions of Stars . .	18^h	24°	20	Campbell and Moore . .	Lick Observatory, 1926.
Proper Motions of Stars . .	18^h	27°	20	Ralph Wilson . . .	*Astron. Jour.* **36**, 1925.
Constant of Aberration . .	—	—	—	Doolittle.	
Clock Corrections . . .	—	—	—	Boss.	
Clock Corrections . . .	—	—	—	Lick Observatory.	
Star Places	—	—	—	General.	

the Yerkes Actinometry, I (Parkhurst); the Yerkes Actinometry, II (Fairley); and the Leiden magnitudes (A. de Sitter).

The visual magnitudes of 1,300 of the stars in the catalogue have been determined at Harvard in the various Harvard Photometries. With the aid of the photovisual standards recently set up in the Harvard Standard Regions (Harvard Annals, 89, No. 1) these visual magnitudes have been rendered homogeneous, and they are used to obtain the colour indices given in the catalogue. The revised visual magnitudes are very close to the Potsdam visual scale.

The spectral classes of all stars in the catalogue taken from the Henry Draper Catalogue are used in examining the dispersion of colour within one spectral class.

Miss INGE LEHMANN.—*On the Construction of Seismic Time-curves for great Distances.*

In earthquake diagrams recorded at great distances from the epicentre various phases not found at smaller distances occur. The diagrams are more complex and special methods are required for the determination of time-curves.

The data of the bulletins are available for the determination of time-curves for P and S for distances up to about 85°. With respect to the phases recorded at greater distances the bulletins do not give sufficient information. Most stations do not publish detailed readings of their seismograms, and even if this were done it would hardly yield the data required for a satisfactory determination of time-curves. The diagrams recorded at great distances are often difficult to read; some of the phases follow each other closely, and often the movement, though strong and varying in appearance, does not contain clearly marked phases; the reading of the diagrams of a single station, therefore, gives an uncertain result.

If, however, records from many stations which are not very far apart can be brought together and read by one observer a satisfactory reading can be obtained. When the records are compared with one another their characteristics become apparent and the phases which repeat themselves can be traced in all. If for an interval of distance a sufficient number of records are available, the course of the time-curves can be constructed and the phases can be identified.

For the construction of time-curves a determination of epicentre is required, and this as a rule cannot be very accurately made. But, if we use the records of a group of stations at a great distance from the epicentre, and if we are satisfied with determining the course of the time-curve and do not require the time-curve itself, no great accuracy is needed in the determination of the epicentre. For, if the epicentre adopted is shifted to a slightly different position, all the distances to the observing stations are altered by very nearly the same amount, and the course of the time-curve remains the same. Only the course of the time-curve is needed for the identification of a phase.

For five earthquakes the records of the European group of stations have been studied. The phases have been traced, the course of their time-curves determined and the phases identified. The results which have been obtained have been compared with one another and differences between the time-curves of the different earthquakes have been found. The ratio PP : P has also been considered, and has been found to differ in different cases.

The phase P′ was studied from the records of the New Zealand earthquake of 16 VI 1929.[1] It was found that, for the distances of the European stations, c. 160–170°, phases were recorded corresponding to two branches of the time-curve of P′. The time-curves of such other phases as could be traced in all the records available were also determined,[2] and compared with those constructed by B. Gutenberg.

The following earthquakes were studied for the range of distance mentioned, viz.: 22 III 1928, 4^h (80–90°)[3]; 18 VII 1928, 19^h (85–95°); 13 XI 1925, 12^h (93–100°); 24 X 1930, 20^h (95–105°).

[1] I. Lehmann: P′ as read from the records of the earthquake of June 16, 1929. *Gerl. Beitr. Geoph.* 26, 1930.

[2] I. Lehmann: Die Bedeutung der europäischen Stationsgruppe für die Bestimmung von seismischen Laufzeitkurven. *Verhandl. fünften Tagung. Balt. Geod. Komm.* Helsinki, 1931.

[3] I. Lehmann: The earthquake of 22 III 1928. *Gerl. Beitr. Geoph.* 28, 1930.

The observations of P were found to fit the tables of Jeffreys better than those of Zöppritz or of Mohorovičic.

For distances greater than 85°, $\overline{S_cP_cS}$ was found in all the records. It was a well-marked phase, and the time-curve was well determined in all cases except 22 III 1928; here it was present but not clearly marked.

S_n was well defined except in the case of 13 XI 1925.

$\overline{S_cP_cS}-P$ was found to be greater for 18 VII 1928 than for the two earthquakes for which greater distances were considered; thus the time-curves differed.

In all the records of 24 X 1930, P was very small or absent, whereas PP and SS were large and well marked phases. In the records of 18 VII 1928 and 13 XI 1925, P was clearly marked, but PP and SS were not very conspicuous. For the epicentres adopted the ranges of distance considered in the three cases overlapped. Thus the ratio of amplitudes PP : P differed greatly for distances which appeared to be the same. The epicentres could not be very accurately determined, but various considerations showed that it was almost certain that the great differences which were observed in PP : P actually occurred for the same distance.

Prof. da Costa Lobo.—*New theories of Physics, resulting from the phenomena of Radio-activity.*

DEPARTMENT OF MATHEMATICS (A*).

Thursday, September 24.

Prof. D. M. Y. Sommerville.—*Isohedral and Isogonal Generalisations of the Regular Polyhedra.*

In Max Brückner's discussion of isohedral (*gleichflächig*) and isogonal (*gleicheckig*) polyhedra (*Vielecke und Vielflache*, pp. 140 *ff.*) two faces of a polyhedron are defined to be equal when they are either directly or inversely congruent and the dihedral angles at corresponding edges are equal; two vertices are equal when the spherical polygons, which they form on unit spheres with centres at the vertices, are either directly or inversely congruent and the lengths of corresponding edges are equal. With these definitions all possible isogonal polyhedra are obtained by truncating the corners and edges of the regular n-sided prism, the octahedron and hexahedron, and the icosahedron and dodecahedron, in a way similar to that in which the semi-regular (Archimedean) bodies are obtained from the regular polyhedra; the isohedral polyhedra are the polar reciprocals of these. An isogonal polyhedron has always a circumscribed sphere, and an isohedral polyhedron has an inscribed sphere.

Among these isogonal and isohedral polyhedra there occur generalisations of the regular tetrahedron, hexahedron and octahedron, the tetrahedron (with opposite edges equal) being both isogonal and isohedral; there occur also isogonal icosahedra and isohedral dodecahedra, but not isohedral icosahedra or isogonal dodecahedra (except the regular ones).

It is shown in this paper that, if the conditions of equal corresponding dihedral angles be omitted, there are isohedral icosahedra; and, if the condition of equal corresponding edges be omitted, there are isogonal dodecahedra. These in general have neither a circumscribed nor an inscribed sphere.

Mr. H. S. M. Coxeter.—*A new Uniform Polytope in Four Dimensions.*

The cube has nine planes of symmetry: three parallel to faces, and six through pairs of opposite edges. These nine planes divide a concentric sphere into 48 spherical triangles, each of angles $\frac{1}{4}\pi$, $\frac{1}{3}\pi$, $\frac{1}{2}\pi$. We can suppose these triangles to be alternately 'white' and 'black'; each white triangle being surrounded by three black ones, and each black by three white. It is possible to select a point within a white triangle in such a way that the points similarly situated in all the 24 white triangles are the vertices of a polyhedron whose faces are regular. This polyhedron is the snub cube, one of the thirteen 'Archimedean solids'. The snub dodecahedron can be derived similarly from a different net of spherical triangles.

Analogously in four dimensions, a hyper-sphere is divided into a number of equal hyper-spherical tetrahedra by a properly chosen set of 3-spaces through its centre. In a certain special case, it is possible to select points within alternate tetrahedra so as to form the vertices of a new polytope having regular bounding solids. This is the case when each tetrahedron is a right (triangular) pyramid, with dihedral angles $\frac{1}{3}\pi$ at the basal edges and $\frac{1}{2}\pi$ at the lateral edges. The new polytope is bounded by $24 + 96$ regular tetrahedra and 24 icosahedra; and its 96 vertices, along with the 24 vertices of a regular '24-cell', make up the 120 vertices of a regular '600-cell'.

A further extension of this process leads to a four-dimensional space-filling, in which each vertex is surrounded by five '5-cells' (regular simplexes), one '16-cell' (cross polytope), and four specimens of the new polytope.

Mrs. Boole Stott has shown that the 96 vertices of the new polytope lie respectively in the 96 edges of a 24-cell (reciprocal to the above-mentioned 24-cell). Each edge of the 24-cell is divided in the ratio $\tau : 1$, where $\tau^2 = \tau + 1$. The 24 bounding icosahedra of the new polytope are inscribed in the 24 bounding octahedra of the 24-cell.

The vertices of the new space-filling divide, in the same ratio, the edges of the regular space-filling of 24-cells.

Mrs. Stott's drawings were exhibited, of two solid sections of the new polytope. One of these was coloured for comparison with her model of the corresponding section of the 600-cell.

Mr. M. H. A. Newman.—*Topology and Continuous Groups.*

Among the 'Mathematical Problems' enumerated by Hilbert in 1900 was that of showing that the analytic character of a continuous group follows from its continuity alone. For two-dimensional groups this problem was solved in 1910 by Brouwer; he showed that from the mere continuity of the transforming function it follows that the group is one of those enumerated by Lie (who had of course assumed analytic character). Since then hardly any progress has been made with the extension to n dimensions.

The recent advances in topological technique, and especially the fixed-point formula of Lefschetz, have provided the materials for a new attack on the problem, and the truth of Hilbert's conjecture can now be asserted when the group is *Abelian* and the group-space *compact*: such a continuous group is necessarily the closed translation group:

$$x'_i = x_i + a_i \quad (i = 1, 2, \ldots, n).$$

Prof. H. W. Turnbull.—*Canonical Matrices and Matrix Equations.*

The object of this communication is twofold: (1) to give examples from a wide range of subjects in which various authors have utilized matrices in solving their problems; (2) to suggest certain lines along which the future advance in matrix manipulation is likely to be made.

Examples are taken from the algebraic theory itself, besides its applications to differential equations, calculus, analytical geometry, electricity and theory of statistics.

The fundamental rôle played by functions $f(x, y)$ of two matrix arguments x, y in the study of functions of n matrices is indicated.

Friday, September 25.

Miss M. L. Cartwright.—*Integral Functions of Integral Order.*

An integral function $f(z) = f(re^{i\theta})$ is said to be of order ρ if

$$\overline{\lim_{r\to\infty}} \log \frac{\log M(r)}{\log r} = \rho,$$

where $M(r) = \max | f(re^{i\theta}) |$. Further, if $f(z)$ is of order ρ and if

$$\overline{\lim_{r\to\infty}} \frac{\log M(r)}{r^\rho} = A,$$

then $f(z)$ is said to be of order ρ and of minimum, mean, or maximum type according as $A = 0$, $A > 0$, or $A = \infty$. Suppose that $f(o) = 1$, and let $z_1 = r_1 e^{i\theta_1}$,

$z_2 = r_2 e^{i\theta_2}$, ... , where $o < r_1 \leq r_2 \leq$ . . ., be the zeros of $f(z)$, and let $n(r)$ denote the number of zeros for which $|z| \leq r$. Then if $f(z)$ is of order ρ we may write

$$f(z) = e^{c_1 z + \ldots + c_p z^p} \prod_1^\infty \left(1 - \frac{z}{z_n}\right) e^{\frac{z}{z_n} + \ldots + \frac{z}{p z_n^p}},$$

where $p = [\rho]$.

If ρ is not an integer, the type of $f(z)$ depends only on $n(r)$. In 1905, Lindelöf showed that if ρ is an integer, the type of $f(z)$ depends not only on $n(r)$ but also on

$$S(r) = c_\rho + \frac{1}{\rho} \sum_{r_n \leq r} \frac{1}{z_n^\rho}.$$

In particular, if $n(r) = O(r^\rho)$ and $S(r) = O(1)$, but one, at least, of these relations is not satisfied with o instead of O, then $f(z)$ is of mean type.

With the improved methods now available we can obtain more precise relations between $n(r)$, $S(r)$ and $|f(z)|$. For instance, if $n(r) = o(r^\rho)$ we have

$$\log f(z) \sim S(r) z^\rho$$

in a set of density 1. This is proved by splitting up the canonical product in an appropriate manner. If, on the other hand, $n(r)$ is large compared with

$$\overline{\lim} \frac{\log |f(re^{i\theta})|}{r^\rho},$$

then $S(r)$ tends to a limit. This is proved by the method which Titchmarsh used for a special function. The method involves a formula for mean values of the series $\Sigma z_n^{-\rho}$ and a Tauberian argument. The formula is very similar to Jensen's formula for $n(r)$.

Dr. E. H. Linfoot.—*Fourier Series of Almost-periodic Functions.*

A short account of the present state of the theory.

Mr. R. E. A. C. Paley.—*A Remarkable Series of Orthogonal Functions.*

We define Rademacher's functions $\varphi_n(t)$ in the following way:

$$\varphi_0(t) = +1, \ 0 \leq t < \tfrac{1}{2}; \quad \varphi_0(t) = -1, \ \tfrac{1}{2} \leq t < 1$$
$$\varphi_0(t+1) = \varphi_0(t) \quad ; \quad \varphi_n(t) = \varphi_0(2^n t), \ n = 1, 2, \ldots .$$

From Rademacher's functions we form a set of functions $\psi_n(t)$: $\psi_0(t) = 1$, and

if $$n = 2^{n_1} + 2^{n_2} + \ldots + 2^{n_\lambda}, \ n = 1, 2, \ldots ,$$

then $$\psi_n(t) = \varphi_{n_1}(t)\, \varphi_{n_2}(t) \ldots \varphi_{n_\lambda}(t).$$

These ψ-functions have been defined in another way by Walsh, and their properties have been discussed by him, and later by Kaczmarz. They compose a complete normalised set of orthogonal functions.

A function $f(t)$ of class L may be formally expanded in the interval $(0, 1)$ as a series

$$f(t) \sim \sum_0^\infty a_n \psi_n(t)$$

where $$a_n = \int_0^1 f(t) \psi_n(t) dt.$$

There is a close analogy between the behaviour of series of this form and that of ordinary Fourier series, as there is between the behaviour of series of Rademacher's

functions and that of lacunary trigonometric series. For example, we have the following theorem:

If $f(t)$ is of class L^k, where $1<k<\infty$, if $\{\lambda_0=0, \lambda_1, \lambda_2, \ldots\}$ is any sequence of positive integers for which $\lambda_{m+1}/\lambda_m > q > 1$, and if $S_p(t)$ is the δth Cesàro mean of rank p of the φ-series associated with $f(t)$, then the upper bound of $|S_{\lambda_m}^{\delta}(t)|$, for variable m, is also of class L^k, and the ratio of the integral of the kth power of this function to the integral of the kth power of $|f(t)|$ between the limits 0 and 1 lies below a number dependent only on k, q, and δ.

With $\delta=0$ this theorem describes properties of a function derived from the partial sums of the ψ-series. The function

$$\left\{\sum_{m=0}^{\infty}\left(S_{\lambda_{m+1}}(t)-S_{\lambda_m}(t)\right)^2\right\}^{\frac{1}{2}},$$

where $S_p(t)$ is written for $S_p^0(t)$, formed from the partial sums in an entirely different manner, has the same properties.

Mr. J. M. Whittaker.—*The Composition of Linear Differential Systems.*

The paper is concerned with the problem of resolving a linear differential system, consisting of a differential equation together with boundary conditions, into two or more systems of lower order. The product of two such systems is defined, and it is shown that the Green's function of the product system is the product (by composition of the second kind) of the Green's functions of the component systems. It follows that the two products of a system with the system adjoint to it are self-adjoint, and it is shown that the solution of the original system can be found in terms of the eigen-functions of these two product systems.

Mr. T. W. Chaundy.—*Partition-generating Functions.*

MacMahon, by imposing on a partition the condition of arrangement in monotonic order, was able to define many-rowed partitions (or 'plane' partitions) which were not mere obvious combinations of ordinary (*i.e.* one-rowed) partitions.

Thus

$$\begin{matrix}4 & 2 & 1 & 1\\ 3 & 2 & 1 & \\ 1 & & & \end{matrix}$$

in which both rows and columns run in non-ascending order, is a 3-rowed partition of 15. MacMahon, by his very difficult algebra of 'lattice functions', evaluated generating functions for plane partitions of various types, culminating in the remarkable formula

$$\prod_{r=1}^{\infty}(1-x^r)^{-r} \tag{1}$$

for the unrestricted plane partition.

I base my treatment of these partitions on certain recurrence-formulæ in which the monotonic principle is inherent. Thus the generating function ξ_a of ordinary partitions, in which the first part does not exceed a satisfies the recurrence-formula

$$\xi_a=\sum_{r=0}^{a}x^r\,\xi_r,$$

which leads at once to Euler's formula for ξ_a. Similarly ξ_{ab} the generating function of two-rowed partitions in which the first parts of rows 1, 2 do not exceed a, b satisfies the recurrence-formula

$$\xi_{ab}=\sum_{s=0}^{b}\sum_{r=s}^{a}x^{r+s}\,\xi_{rs}.$$

We find the solution

$$\xi_{ab}=\xi_a\,.\,\xi_b-x\xi_{a+1}\,.\,\xi_{b-1},$$

and generalise for the many-rowed partition into the form

$$\xi_{abc\ldots} = \begin{vmatrix} \xi_a & \xi_{a+1} & \xi_{a+2} & \cdots \\ x\,\xi_{b-1} & \xi_b & x^{-1}\xi_{b+1} & \cdots \\ x^{4}\xi_{c-2} & x^{2}\xi_{c-1} & \xi_c & \cdots \\ \cdot & \cdot & \cdot & \cdot \end{vmatrix}.$$

This formula extends to various other cases of specially-conditioned partitions and includes MacMahon's formula (1) as a special case.

Mr. E. M. Wright.—*Asymptotic Partition Formula.*

Hardy and Ramanujan found an asymptotic formula for $p(n)$, the number of linear partitions of n, by studying the properties of the generating function

$$f_1(x) = \prod_{l=1}^{\infty} (1 - x^l)^{-1} = 1 + \sum_{n=1}^{\infty} p(n)x^n,$$

and so finding the value of

$$\int_C \frac{f_1(x)dx}{x^{n+1}},$$

where C is the circle $|x| = 1 - \frac{1}{n}$.

This method can be extended to determine an asymptotic expansion of $p_k(n)$, the number of partitions of n into k-th powers. The generating function is

$$f_k(x) = \prod_{l=1}^{\infty} \left(1 - x^{l^k}\right)^{-1} = 1 + \sum_{n=1}^{\infty} p_k(n)x^n.$$

The transformation theory of $f_k(x)$ has to be developed by lengthy analysis. We then obtain expansions of $p_k(n)$ in a series of integral functions of $n^{1/k}$, and in a series of descending powers of $n^{1/(k+1)}$. We can also find an expansion of $q(n)$, the number of plane partitions of n, in descending powers of $n^{\frac{1}{3}}$, by means of the generating function found by MacMahon and Chaundy.

It is interesting to consider 'weighted partitions' of n into the parts $\mu_1, \mu_2, \ldots\ldots$, each weighted $\lambda_1, \lambda_2, \ldots\ldots$, so that the generating function is

$$\prod_{l=1}^{\infty} \left(1 - \lambda_l x^{\mu(l)}\right)^{-1} = 1 + \sum_{n=1}^{\infty} p_\mu(n)x^n.$$

The most interesting case appears to be that in which $\mu(l) = l$, $\lambda_l = a > 0$, and the generating function is

$$\prod_{l=1}^{\infty} \left(1 - ax^l\right)^{-1} = 1 + \sum_{n=1}^{\infty} p_a(n)x^n.$$

If $a < 1$, the asymptotic expansion of $p_a(n)$ is found as in the problems already considered. If $a > 1$, new methods are applicable. a_0 is the root of a certain transcendental equation; if $1 < a < a_0$, $p_a(n)$ has an asymptotic expansion, which may be found by an application of Cauchy's theorem. If $a \geq a_0$, the asymptotic expansion becomes convergent, and is equal to $p_a(n)$ for all positive integral values of n. In the latter case, the expansion may be found by 'elementary' methods.

Prof. A. J. McConnell.—*Applications of Differential Geometry to Dynamics.*

The configurations of a dynamical system of N degrees of freedom can be represented geometrically by points in a space of N dimensions, and consequently the

motion of a general system can be reduced to that of a particle in a hyperspace. This is especially of value in the discussion of the stability of a dynamical system, which becomes the geometrical problem of the variation of certain curves in a representative space. In particular, we can choose the space so that the dynamical trajectories are represented by geodesics and the problem of stability becomes identical with that of geodesic deviation in differential geometry.

The study of null-geodesics of a Riemannian manifold is of some importance since they are the paths of light-rays in the space-time of relativity theory. It is possible to choose a space in which the trajectories of a dynamical system are represented by its null-geodesics. Such a representation leads to a geometrical theory of the Hamilton-Jacobi equation and shows the intimate connection between the geometrical and dynamical theories of contact transformations.

Dynamical systems are of two types, holonomic and non-holonomic, and our remarks so far have been confined to the first. The attempt to find a geometrical representation of non-holonomic systems has led to a new type of differential geometry, which treats of the properties of non-holonomic manifolds. Here again we can define the geodesics of such a manifold and show that they represent the trajectories of the corresponding dynamical system.

Prof. J. B. S. Haldane.—*Some Mathematical Problems of the Biologist.*

In a population in which characters are inherited according to Mendel's laws, and which is subject to an assigned mating system and type of selection, it is required, given the population in one generation, to predict its composition in future generations. If the population be large and generations do not overlap, the parameters defining the composition of the $(n+1)$th generation can be given in terms of those of the nth. Thus, in the trivial case of self-fertilization the proportion p_n of heterozygotes is given by the equation $p_{n+1} = \frac{1}{2}p_n$, $\therefore p_n = (\frac{1}{2})^n p_0$. If all matings are between $(m-2)$th cousins p_n is the sum of m geometric series whose common ratios are the roots other than $\frac{1}{2}$, of $x^{m+1} - x^m - 2^{-m-1} = 0$. More complex problems lead to groups of as many as 22 simultaneous linear finite difference equations.

If selection occurs we have to solve non-linear difference equations of which $\Delta u_n = \dfrac{ku_n^2}{u_n+1}$ is typical. When k is small they can be solved as differential equations, and the effect of natural selection on a population whose variation is due to m genes can be represented by the motion of a point in m-dimensional space. The trajectories fall into families converging to from 1 to 2^{m-1} stable equilibria. In general the equation $\Delta u_n = k\phi(u_n)$ may be solved by expanding in ascending powers of k. The convergence of the series obtained is under investigation.

Monday, September 28.

Discussion on *Fluid Motion with reference to Aerodynamics* :—

Prof. G. I. Taylor, F.R.S.—*Stability and Turbulence in a Stream of Fluid of Variable Density.*

Stability of Waves.—The waves in a stream of fluid, the density and velocity of which vary with height above a horizontal plane, are investigated mathematically. In the case of a fluid moving with uniform shearing velocity $U=\alpha z$ parallel to the axis of x and possessing a small uniform rate of decrease in density with height expressed by $\beta = \dfrac{1}{\rho}\dfrac{\delta\rho}{\delta z}$, ρ being the density and z the height, it is found that if $\alpha^2 > 4g\beta$ no waves are possible either stable or unstable; on the other hand, if $\alpha^2 < 4g\beta$, all waves appear to be stable.

Other cases are investigated where the fluid of variable density is replaced by superposed layers of fluid each of constant density, but each less dense than the fluid below it. The velocity distribution is the same as before, namely, a uniform

shearing motion through all layers. In these cases, if $\delta\rho$ is the difference in density between each fluid and that immediately above it, and if h is the thickness of all layers except the bottom and top layers which extend to $\pm\infty$, then $\beta^1 = \delta\rho/\rho h$ is a quantity similar to β, and it is found that when there is only one intermediate layer the flow becomes unstable when $\alpha^2 > 2\,g\beta^1$, while if there are two such layers the criterion is $\alpha^2 > 2{\cdot}11\,g\beta^1$. It seems possible that with a large number the criterion might be $\alpha^2 > 4\,g\beta^1$.

Turbulence.—The stability for infinitely small disturbances has not necessarily any definite relationship with the question of whether or no turbulence can be formed in fluids with a stable distribution of density. On the other hand, disturbances of a stably stratified fluid of variable density necessarily involve an increase in gravitational potential energy. If $\rho\mu_u$ is the coefficient of virtual viscosity due to turbulence, and if μ_s is the virtual coefficient of diffusion, then the rate of supply of energy by the 'Reynolds stress' is great enough to supply all the energy necessary to maintain turbulence against gravity if $\alpha^2/g\beta > \mu_s/\mu_u$. If we accept the Reynolds-Prandtl hypothesis concerning the relationship between the transfer of momentum and of other properties in a turbulent fluid, then $\mu_s = \mu_u$ so that the criterion for the maintenance of turbulence is $\alpha^2/g\beta > 1$, a relation which was obtained by L. F. Richardson and by the present writer many years ago.

The only observations of a fluid of variable density which I have been able to find from which μ_u, μ_s, α and β can be calculated are those of Dr. J. P. Jacobsen on the mixing of salt and fresh water in Randersfjord and in the Kattegat. The figures given in Tables I and II are taken from Dr. Jacobsen's measurements. It will be observed that, as Dr. Jacobsen has pointed out, the Reynolds-Prandtl relationship $\mu_s = \mu_u$ is not found to hold when salt and fresh water mix, indeed μ_u is generally many times as great as μ_s. On the other hand, it will be seen that $\alpha^2/g\beta$ is of the same order but greater[1] than $\mu_s\mu_u$, even though both of them may be less than 0·1. In spite of the great stabilising effect of gravity the flow was evidently turbulent, for the values of μ_u are some hundreds of times as great as the viscosity due to molecular agitation.

TABLE I.—*Data for Schulz's Grund, Kattegat. C.G.S. units.*

Depth Metres.	α	β	μ_s	μ_u	μ_s/μ_u	$\alpha^2/g\beta$
2·5	-10×10^{-3}	$7{\cdot}5\times10^{-7}$	0·3	3·1	0·09	0·14
5·0	-17×10	11·2	0·4	3·1	0·13	0·24
7·5	-22×10	28·5	0·18	2·7	0·067	0·17
10·0	-24×10	60·0	0·05	2·2	0·023	0·098
12·5	-19×10	105·0	0·04	1·9	0·021	0·035
15·0	-8×10	82·5	0·2	3·8	0·05	0·008
17·5	0×10	46·0	0·74			
20·0	$+4\times10$					

TABLE II.—*Data for Randersfjord. C.G.S. Units.*

Depth Metres.	α	β	μ_s	μ_u	μ_s/μ_u	$\alpha^2/g\beta$
1	−0·033	$0{\cdot}67\times10^{-5}$	0·6	3·5	0·17	0·17
2	−0·071	$1{\cdot}35\times10^{-5}$	0·5	2·5	0·20	0·38
3	−0·062	$1{\cdot}50\times10^{-5}$	0·4	2·6	0·15	0·26

[1] Except close to a region where $\alpha = 0$.

Prof. R. V. SOUTHWELL, F.R.S., and Mr. H. B. SQUIRE.—*A Modification of Oseen's Approximation to the Equations of Motion for a Viscous Incompressible Fluid.*

The problem discussed is the motion in two dimensions, past a fixed cylindrical body, of a fluid which has negligible compressibility but finite viscosity. Its importance for Aerodynamics is evident, since a solution would render possible a theoretical prediction of 'profile drag.'

The exact hydrodynamic equations have so far proved intractable,—mainly on account of the terms representing *convection* of vorticity, which render them non-linear. Accordingly, Oseen has proposed, as an approximation, to replace the actual velocities u and v, in these convectional terms, by $\overline{\mathrm{U}}$ and $\overline{\mathrm{V}}$, the *undisturbed* velocities of the fluid. The resulting equations are linear, and have been solved in relation to a circular profile. Oseen's method of approximation is not restricted to problems in two dimensions.

This paper suggests an alternative method of approximation, leading to an equation (linear in form) which would seem to be a closer approximation than Oseen's. It can be solved exactly in relation to a point source of vorticity, and the authors are endeavouring, on the basis of that solution, to construct approximate solutions for specified profiles. Their procedure, which involves graphical and numerical methods, is being applied in the first instance to a circular profile, in order that the results may be compared with those derived from Oseen's equation.

Results are not yet available. The aim of the paper is to bring to the notice of mathematicians a modification of the governing equation for motion in two dimensions, which would seem to be a close approximation, and which may prove to be tractable by purely analytical methods.

Dr. S. GOLDSTEIN.—*The Stability of Viscous Fluid Flow between Rotating Cylinders.*

It is assumed that an incompressible viscous fluid is flowing, under the influence of a pressure gradient parallel to the axis, along a narrow annular space between two infinitely long coaxial rotating cylinders. The stability of the system is investigated mathematically by the method of small oscillations, with the assumptions that the disturbance is symmetrical about the axis and periodic along it. There is no steady disturbance possible, and for any given Reynolds number of the flow and ratio of the velocities of rotation of the inner and outer cylinders it is possible to calculate the lowest angular velocity for which instability occurs, the time-period of the critical flow, and its wave-length along the axis. Numerical results have so far been worked out for small Reynolds numbers of the flow with the outer cylinder stationary.

Prof. A. ROSENBLATT.—*The Stability of Laminary Movements.*

The problem of the stability of laminary and other movements of incompressible viscous fluids has been treated in this country by Reynolds, Kelvin, Rayleigh, and others, but not *exactly*, that is, for finite disturbances by consideration of non-linear equations. For laminary motion between two parallel walls certain disturbances which vanish exponentially at infinity and exponentially also with the time, will be described.

If $\Psi_0\,(x, y)$ is the stream function of the undisturbed motion, the stream function $\Psi\,(x, y, t)$ of the disturbed motion is of the form

$$\Psi_0\,(x, y) + \sum_{k=1}^{\infty} e^k\;\Psi_k(x, y, t).$$

In the case of exponentially vanishing perturbations between the walls $y = \pm \mathrm{H}$ we can write

$$\Psi_k\,(x, y, t) = e^{-k(\lambda x + \mu t)}\,f_k\,(y), \qquad k = 1, 2, \ldots$$

It can be shown that suitable real numbers λ, μ exist, and that within every interval of values of λ there is an interval for which the convergence of Ψ can be established by the method of dominant functions. In general, there is a

definite transcendental equation which λ and μ together must satisfy, and there are certain others which they must not satisfy. The simplest case is that of linear velocity, $\Psi_0 = -Uy^2/2H$, which includes the case of fluid initially at rest; for this particular case the actual transcendental equations will be given.

The study of periodic disturbances of finite amplitude is rendered difficult by the existence of small and zero divisors. The series for $\Psi(x, y, t)$ is real only when Ψ_k has the form

$$\sum_{h=0}^{k} \exp\left[-\left\{h\bar{\lambda}+(k-h)\lambda\right\}x-\left\{h\bar{\mu}+(k-h)\mu\right\}t\right].\ f_{k-h,\,h}(y),$$

when $\lambda, \bar{\lambda}$; $\mu, \bar{\mu}$: and $f_{k-h,\,h}(y)$, $f_{h,\,k-h}(y)$, are conjugate complex numbers.

Prof. J. R. Partington and Mr. N. L. Anfilogoff.—*Curved Pipe Stream Line Flow and Viscosity.*

Viscosity measurements by means of transpiration through a helical capillary are quite usual, but recent determinations by this method into the high temperature region have given rise to unexpected anomalies in the Sutherland Law.

It is now shown that these latter were due partly to curved pipe stream line motion at the lower temperatures and partly to a probable real breakdown in Sutherland's Law at high temperatures. Corrections by means of White's method of plotting Dean's Criterion have been found to be applicable, and the Dean formula to hold within the limits of experimental accuracy for low values of increased resistance for Argon, Air and Hydrogen Chloride.

It has further been demonstrated by means of Dean's Criterion that inconsistencies in some of the early viscosity determinations are due to curved pipe flow and hence the explanations appearing in the literature for these are probably incorrect. The low temperature determinations of the Halle School are in order, with the exception of the lowest temperature determination on Argon, the latter result being one of the range employed by Lennard Jones and by Hassé and Cook in their theories of the variation of viscosity with temperature.

General Discussion (Sir Horace Lamb, F.R.S.).

Wednesday, September 30.

Prof. A. R. Richardson.—*Recent Developments of Non-commutative Algebra.*

During the last ten years no fewer than 100 papers have been published dealing with different aspects of non-commutative algebra.

Of these, many are concerned with division algebras, that is, number systems in which given any non-zero number x, y can be found, such that $xy = 1$, the investigation of which was rendered fruitful by L. E. Dickson's definition of an infinity of such algebras by the aid of algebraic equations. Work concerning their structure has failed, however, to determine whether *all* division algebras can be so constructed from cyclic equations.

Progress in other branches of the subject has been made possible by the definition of integers in non-commutative systems. For example, in every algebra there is an equivalent of Fermats' theorem, and the theory of congruences, diophantine equations, rings and modules is being studied and applied to real integral number theory, in particular to the solution of diophantine equations. Work remains to be done on the theory of quadratic reciprocity and, notwithstanding the introduction of ideals, the problem of restoring the uniqueness of factorization is still unsolved.

The definition of determinants in non-commutative algebras has assisted the study of the theory of linear substitutions and invariants in such systems. While, however, a solution has now been found for the general linear equation, the quadratic, except in the very simplest cases, must still be regarded as unsolved. One of the difficulties presented by such equations is due to the existence of many linear and quadratic

identities, and the determination of these in large classes of algebras is a subject which remains for investigation.

Some mention must also be made of work bearing on the infinite (quantum) algebras associated with the singular linear equation $rp - pr = 1$, which has no solution in any associative finite algebra. So far, little interest has been shown in the more general problem of finding algebras in which some other given singular equation has a solution.

Finally, non-commutative algebra has also been applied to the theory of functions and of partial differential equations although, strangely enough, not by the aid of continuous groups.

Mr. T. Smith.—*Tesseral Matrices.*

A matrix u and its reciprocal U, are tesseral matrices if

$$u = \begin{pmatrix} b & d \\ a & c \end{pmatrix} \quad , \quad U = \begin{pmatrix} & c - a \\ -d & b \end{pmatrix},$$

a, b, c, d being similar matrices. Subject to the condition of rank, tesseral matrices such as

$$\begin{pmatrix} ea & eaf - A \\ a & af \end{pmatrix},$$

where A is the reciprocal of a, and e and f are symmetrical, form a group. The multiplication of matrices of this type, with a simple substitution, yields a solution of the problem of evaluating the function h

$$h\,(x, z) = f\,(x, y) + g\,(y, z),$$

where f and g are known algebraic functions of the sets of variables x, y, z, when $f + g$ is stationary for any small changes in y.

Dr. J. Wishart.—*Interpolation without Differences.*

The common interpolation formulæ involve the differences of the tabulated function, and it has hitherto been considered desirable to furnish a table with, at any rate, the even differences in order to lessen the burden to the user. C. Jordan gave in 1928 a formula of such a nature that tabular differences are of no value in facilitating its use. The general adoption of this formula would therefore much reduce the labour and expense of preparing new tables, provided it were clearly proved that the new method was an improvement, in the nature of the operations and the time involved, over the double operation of (a) calculating the differences, and (b) performing the interpolation by one of the ordinary methods. This question is discussed in the present paper.

Dr. G. Temple.—*The Relativistic Wave Equation.*

The wave equation for a free electron has been constructed by Dirac by rationalisation of the relativistic Hamiltonian equation, by Darwin and Frenkel on the analogy of Maxwell's electromagnetic equations, and by Eddington from a general transformation theory.

The equation for one electron in an electromagnetic field gives an immediate explanation of the duplexity phenomena in optical spectra in terms of the 'spin' of the electron; but, although this result is one of the great successes of Dirac's theory, there seems to be no satisfactory theoretical basis for the way in which the electromagnetic potentials are introduced.

Numerous attempts have been made to construct a theory of the direct interaction of electric changes. Gaunt's theory does not satisfy the requirements of relativity; Eddington's theory is in marked disagreement with experiment; Breit and other American physicists have tested, without success, various expressions for the interaction energy.

The only hope seems now to lie in developments of the quantum theory of electromagnetic wave fields.

THE INTEGRATION OF EMDEN'S EQUATION AND THE APPLICATION OF NUMERICAL RESULTS :—

Mr. D. H. SADLER.—*The Numerical Integration of the Equation.*

A short account is given of the computation of numerical solutions of Emden's Equation, and the precautions taken to ensure a solution correct to seven decimal places. The method used involves the calculation of differential coefficients at each point and the application of Taylor's theorem. The advantages of the application of the method to this particular problem are described. The results are compared with Emden's original solutions.

Prof. E. A. MILNE, F.R.S.—*Functions associated with Solutions.*

In the application of Emden's differential equations to problems of stellar structure certain equations occur (called 'equations of fit'), which determine the nature of configurations describable in terms of solutions of Emden's equations of different indices. These can be represented concisely in certain cases by equations

$$u(a) = \mathrm{U}(b)$$
$$v(a) = \mathrm{V}(b) \times \frac{5}{8}\ \frac{\beta_1}{\beta_2}$$

where β_1 and β_2 are stellar parameters, U and V are subsidiary functions associated with solutions of Emden's equation of index $n = \frac{3}{2}$, and u and v are subsidiary functions associated with solutions of Emden's equation of index $n = 3$. For any index n, u and v (or U and V) are connected by a first order differential equation

$$\frac{u}{v}\frac{dv}{du} = -\frac{u + v - 1}{u + nv - 3}$$

u and v being defined by

$$u = -\frac{\xi\theta^n}{\theta'},\ v = -\frac{\xi\theta'}{\theta}$$

where

$$\frac{1}{\xi^2}\frac{d}{d\xi}\left(\xi^2\frac{d\theta}{d\xi}\right) = -\theta^n$$

and $\theta \equiv \theta(\xi)$.

The object of the investigation is to determine the roots a and b. The complete solution is given for all types of solutions of the two Emden equations concerned, partly from analytical considerations, partly from numerical quadratures by N. Fairclough. When U and V are derived from an *Emden* solution of $n = \frac{3}{2}$, there are either no solution, one solution, two coincident solutions or two distinct solutions according to the type of the solution of $n = 3$ used for u and v and the values of β_1 and β_2. These results are important in the theory of stellar structure.

Mr. N. FAIRCLOUGH.—*Numerical Results for Indices 3 and $\frac{3}{2}$.*

For a given value of the index, the integrals calculated depend on one parameter. Tables are of two kinds, those in which individual integral curves are followed, and those which show the dependence on the parameter of some associated function or of some magnitude, such as the abscissa of the first zero or of the first maximum, which has not more than one value for each integral.

Mr. R. H. FOWLER, F.R.S.—*Methods of Studying Emden's Equation.*

A short account was given of certain methods which can be used with success in analysing the nature and arrangement of the solutions of equations of the type

$$\theta'' + x^{\sigma}\theta^{n} = 0$$

especially as $x \rightarrow \infty$* and as $x \rightarrow 0$. This equation is equivalent to Emden's equation when $\sigma = -4$.

* See p. 566 for references.

The idea which has enabled an analysis to be completed of the *arrangement* of the different solutions through a single starting-point (0, x) was described in more detail. The idea consists in using the one-parameter family of special solutions known as Emden's Solutions as a grid or co-ordinate system to which the behaviour of the other solutions can be referred and thereby disentangled. It is first necessary to determine when the Emden's Solutions are so arranged themselves that they have suitable properties for a co-ordinate system. The method proves successful here, but the complete results can be obtained in other ways and it does not seem likely that the new method will prove of much wider utility.

DEPARTMENT OF COSMICAL PHYSICS (A†).

Thursday, September 24.

Dr. J. Bjerknes.—*Tropopause Waves.*

The lowest part of the atmosphere—the troposphere—is characterised by a general decrease of temperature from the ground upwards with a lapse rate of about 0°6 C. per 100 m. The overlying part of the atmosphere—the stratosphere—is over the temperate zone usually characterised by an almost constant temperature and over the intertropical belt by an increasing temperature with height. The surface of separation between troposphere and stratosphere, the so-called tropopause, is on an average at 17 km. level over the equatorial belt and descends from there polewards. Over the North Pole the height of the tropopause is probably about 8–9 km.; over the South Pole its height is unknown but certainly still lower than over the North Pole. Over the temperate zone at about 50° N., from which most of the upper air observations are gathered, the tropopause slopes downwards towards the pole with an average inclination of 1 in 800.

Whenever the weather situation at the ground is changing, the tropopause is lowered beneath or raised above its average level. These changes of level of the tropopause may usually be ascribed to the passage from west to east of a series of wave-like disturbances which are here referred to as tropopause waves. The structure of the tropopause waves is shown by aid of vertical and horizontal cross-sections based on observational data. It is being shown that the orbital plane of the tropopause waves is almost horizontal, and that the change of level of the tropopause is, therefore, not due to a vertical displacement, but to a horizontal displacement of the air polewards or equatorwards. The cause of the tropopause waves is sought in the action of the polar front disturbances in the lower troposphere on the air currents of the upper troposphere. The importance of the tropopause waves for certain weather phenomena is discussed.

Sir Gilbert Walker, F.R.S.—*Stratified Clouds.*

It was shown by E. H. Weber in 1855 that in the absence of shear, polygonal patterns were set up in a vertically unstable layer of liquid and in 1920 by Idrac that a rapid shear produced longitudinal vortices. In 1929, S. Mal, at the Imperial College, obtained rectangular cells with slow shears; he also verified by observations in Berlin that (a) when strata of clouds break up they are vertically unstable, and (b) the rate of shear is minute, rapid or slow according as the cloud pattern belongs to the first, second or third type above indicated. This year A. C. Phillips and I have experimented with air drawn through a small wind channel, the motion being made visible by fumes of titanium tetrachloride; with rapid shear, instability caused longitudinal vortices with less rapid rather complex cross patterns; with slow shear, transverse vortices, and with no shear polygons whose sides were vortices.

In Mal's work the liquid always ascended in the interior of a cell and descended in the interlaces between cells; but in our experiments with air the descent was inside the cells. In actual clouds there is usually ascent within the polygons, but descent is by no means rare.

Mr. C. G. Abbot.—*Twenty-five Years' Study of Solar Radiation.*

This paper deals with the intensity of the energy of the sun's rays; its losses in the atmospheres of the sun and the earth; its inequality over the solar disk; its distribution in wave-lengths in the spectrum; the development of instruments and methods to measure these phenomena; the variability of the solar radiation; periodicities in solar variation; and the dependence of weather thereon. These wide-ranging, yet closely related researches have engrossed investigation for more than a quarter of a century. Arranged chronologically, they have yielded the following results:

1. Improved stability and sensitiveness of the recording spectrobolometer.
2. Accurate values of the dispersion of rock salt.
3. Mapping of about 1,500 lines and bands of solar and terrestrial absorption in the infra-red spectrum.
4. Development of the silver-disk, the water-flow, and the water-stir pyrheliometers, which have enjoyed wide acceptance.
5. Improvement of the fundamental process of solar constant measurement.
6. Nearly 2,000 solar-constant measurements at Washington, Bassour, Mount Wilson, and Mount Whitney from 1902 to 1920, yielding a mean value of 1·94 calories per square centimeter per minute and proving that the sun is variable.
7. Many determinations of the distribution of energy over the sun's disc for various wave-lengths.
8. Many determinations of the distribution of energy in the solar spectrum, yielding estimates of the effective temperature of the sun.
9. Development of the recording balloon pyrheliometer, and determination therewith of the solar radiation at 25,000 meters altitude.
10. Development of the pyranometer, an instrument for measuring sky radiation.
11. A new brief method of solar constant determination whereby five independent values per day are obtained with minimum atmospheric influence.
12. Daily observations of the variability of the sun at several widely separated high-altitude desert stations beginning with the year 1920.
13. Determination of the march of variation of the monthly mean values of the solar constant of radiation for the past twelve years with an accuracy sufficient for all purposes.
14. Discovery that the sun's radiation varies in five continuing regular periodicities.
15. Indications that the weather is to a considerable degree governed by solar changes and is probably predictable therefrom.
16. By-products of the research which include determinations of atmospheric transparency, precipitable water, and ozone content.

It is hoped that the apparent connection between solar variation and the weather will be verified and will lead to improved methods of long-range forecasting Smithsonian observers are going on with solar radiation studies, introducing such improvements as may help to establish a more exact record of the solar variation available for all future time.

Dr. F. J. W. Whipple.—*The Circulation of Electricity through the Atmosphere.*

Continuous records of the air-earth current, the current which flows during fine weather from the air to the ground, are now available at Kew Observatory, and provide new information with regard to the circulation of electricity through the atmosphere.

The potential of the Kennelly-Heaviside layer in the upper atmosphere is of the order 3×10^5 volts. Over the oceans the air-earth current is of about the same strength in the northern summer and the southern summer, so that it may be assumed that the mean potential of the K.H. layer is the same at all seasons. On the other hand, there is a well-marked diurnal variation, the potential gradient over the oceans being 15 per cent. below the mean at 5h. G.M.T. and 20 per cent. above the mean at 19h. G.M.T. The potential of the K.H. layer must vary in the same way.

The fluctuations in the air-earth current at a place like Kew, where there is considerable atmospheric pollution, are governed partly by the potential difference between the K.H. layer and the ground and partly by the resistance of the air. It

is estimated that in summer the resistance of a column reaching right up to the K.H. layer and with 1 cm.^{2} cross-section varies between $2{\cdot}2 \times 10^{21}$ ohms at 2 h. and $3{\cdot}3 \times 10^{21}$ ohms at 18h., whilst in winter the range is between $3{\cdot}4 \times 10^{21}$ ohms at 5h. and $8{\cdot}7 \times 10^{21}$ ohms at 17h. The resistance increases steadily during the hours in which pollution is being produced. The estimates are of the right order of magnitude, but the ratios are of more significance than the exact figures.

Potential gradient depends on the strength of the current and on the resistance of the air near the ground. The double oscillation of potential gradient in the course of the day is explained by the double oscillation in resistance. The specific resistance of the air has minima in the early morning and in the afternoon. The difference between the types of variation of total resistance of the atmosphere and of specific resistance close to the ground is explained by the fact that in the hours during which the ground is being warmed pollution diffuses to considerable heights.

The high potential of the K.H. layer is attributed to the action of thunderstorms, but the mechanism by which thunderstorms maintain the circulation of electricity is not discussed in this paper.

There is a well-known difficulty in evaluating the magnitude of the air-earth current. The vertical current in the free atmosphere is regarded as a pure conduction current, positive and negative ions moving in opposite directions. At the surface of the ground only the positive current is effective. The negative current is thought to be counterbalanced by the transport of space-charge by eddy diffusion. Such observations as are available for testing this hypothesis are considered in the paper, but the need for more observations is stressed.

Dr. H. Jeffreys, F.R.S.—*The New Seismological Tables.*

Friday, September 25.

Dr. F. Schlesinger.—*The Use of Photographs of Very Wide Angular Field in Astronomy.*

Until recently, the largest plates used to determine astronomical positions covered four square degrees. Experiments made by the writer at Allegheny and at Yale have shown that plates up to 25 square degrees could be used to advantage, and that these are particularly adapted to compiling zone catalogues and deriving in this way the proper motions of many stars, with much less labour and considerably greater accuracy than is possible with the meridian circle. Such plates must be reduced by means of comparison positions for a few of the stars, especially observed for this purpose with the meridian circle, and experience has shown that the observation of these comparison stars constitutes about one-half the total labour of the whole work. If plates considerably larger than 25 square degrees could be used, this half of the labour would be saved, and a still further, though small, increase in accuracy would be expected. Experiments have, therefore, been made at Yale with plates up to 140 square degrees. It has been found that these yield results which are slightly superior to those covering the 25 square degrees. Plates of this very large size can be reduced without the use of special meridian observations, as the positions in the Standard Catalogues already published or soon to appear (like the General Catalogue of Boss) are sufficiently numerous and sufficiently accurate to give good determinations of the plate constants, though the number of these constants is greater than with the smaller plates.

Dr. H. Spencer Jones, F.R.S.—*Nova Pictoris.*

Nova Pictoris was discovered on 1925 May 25. From that date until 1926 March 13 an extensive series of spectrograms was obtained with the four-prism spectroscope attached to the 24-inch refractor at the Cape Observatory. The spectrum from March 1926 to 1931 has been studied with the aid of objective prism spectra, obtained mainly at the Union Observatory, Johannesburg. The spectrum at discovery was a strong absorption spectrum of type cF5, many of the lines having emission borders to the red; the emission was strongest at the red end of the spectrum. Broad emission bands appeared at about the time of the first maximum. The sequence of spectral changes was generically similar to those observed in previous

novæ, but two facts render the study of Nova Pictoris of particular interest: (1) The light changes have proceeded with abnormal slowness; eleven weeks after discovery, the nova was brighter than when discovered and five years after discovery the magnitude had decreased only to $8^m{\cdot}5$, the pre-outbreak magnitude being $12^m{\cdot}75$. (2) The bands are not so broad as they usually are in novæ, being about 12A at λ 4400; the emission band spectrum is thus not complicated by extensive overlapping of bands and is comparatively simple to disentangle.

A survey is given of the main spectral changes. A feature of interest is the appearance about October 1925 of forbidden emissions associated with metastable states of the ionised atom of iron. Such emissions are prominent in the spectra of η Carinæ and of a few other stars. They all arise from transitions between even terms, ending at one of the two lowest energy levels. These emissions gradually fade away in the early part of 1926, at about the time that typical nebular and helium bands become strong. The N_1, N_2 bands are late in appearing and never become a prominent feature in the spectrum. Up to 1926 March 13 no trace of ionised helium could be seen in the spectrum, but on March 15 the λ 4686 band was one of the strongest in the spectrum. Its intensity was very variable for several months after its initial appearance. The spectrum in 1928 was of interest on account of the development of a hitherto unobserved emission, of unknown origin, at about λ 6087, which became one of the strongest lines in the spectrum. At this stage, the neutral helium emissions were weak but the He II line at λ 4686 was the strongest in the spectrum. In 1931 the λ 6087 emission had become the strongest line; Hα and an emission at λ 5724 were also prominent. The higher members of the Balmer series had decreased considerably in intensity as compared with Hα. No trace could be found of the N_1, N_2 lines. The spectrum now approximates to that of a Wolf-Rayet star.

The main phenomena are in accord with the hypothesis that the nova was in a state of rapid expansion up to the time of the first maximum and that at or about maximum, shells of gas were thrown off. By correlating the observed rate of expansion with the increase in brightness, a parallax of $0''{\cdot}0015$ is derived, corresponding to an absolute magnitude at maximum of $-7{\cdot}9$. The radius of the nova at maximum was about 380 times the radius of the Sun. The progressive shift in the displacements of the two main absorptions for several weeks after maximum can be explained on the hypothesis that the absorptions are due to the absorption by expanding shells of gas of light emitted by the star.

A summary is given of the observations at Johannesburg of the nova as a multiple star and of the surrounding nebulosity. The increase in the diameter of the latter is in good agreement with the hypothetical parallax of $0''{\cdot}0015$. Possible causes of the outbreak are considered; the hypothesis that it is due to a sudden release of energy within the star, rather than to a collision with another star or to the star entering a nebulous region, seems to accord best with the various facts which have to be taken into account.

Dr. S. A. Mitchell.—*The* 1930 *Eclipse observed from Niuafoou.*

The October, 1930, eclipse was very spectacular in all of its details. The only accessible spot from which to observe it was a tiny volcanic island, called Niuafoou or Tin-Can Island, in the Tonga group, situated nearly on the 180th meridian and at 15° south latitude. Only two scientific expeditions made observations, a small party from New Zealand and a better equipped party from the United States, the latter being under the auspices of the U.S. Naval Observatory. The weather, always uncertain in the tropics, behaved magnificently well at the crucial moment.

This was my eighth eclipse. To have the opportunity of working during totality for the grand aggregate of fifteen minutes it has been necessary for me to travel no less than 90,000 miles.

As always, my work was spectroscopic. At this eclipse I had two concave gratings, each without slit. History has shown that it is excessively difficult to secure eclipse spectra of perfect definition, especially when the spectrographs are used without slit. The reason for dispensing with the slit is to secure the heights to which the vapors extend in kilometers above the solar surface. At increased heights there is a slow diminution of temperatures and a more rapid fall in pressures so that conditions are readily reached where an atom can lose an external electron. When this happens, the atom is said to be ionised and its spectrum is radically different from that of the ordinary atom. Sir Norman Lockyer was the first to call

attention to the importance of ionised or enhanced lines. About ten years ago, Saha developed his remarkable theory of ionisation by means of which many solar mysteries have been solved.

Helium was discovered by the spectroscope at the eclipse of 1868, and the eclipse of the following year brought coronium to our ken. After more than sixty years of observation, how much do we know of this mysterious gas ? The unfortunate part has been that in all this long time we have had only about one hour's observation, or an average of one minute per year to accumulate the facts. During totality, when alone the spectrum may be obtained, a total of about fifty lines have thrust themselves into the literature. Davidson and Stratton have done eclipse astronomy a fine service by showing that most of the lines photographed during totality belong to the high chromosphere and only about fifteen to coronium. At the 1930 eclipse I had the good fortune to discover a new line of considerable strength, at wave-length 6776 in the deep red.

The combination of large dispersion and a large image of the sun obtained from the concave gratings permitted, first of all, an accurate determination of wave-lengths. Measures from eight spectra of the green line, from both limbs of the sun, give a wave-length of 5302·91, while six spectra give 6374·28 for the line discovered at the 1914 eclipse, this latter wave-length differing only 0·01 angstroms from a line in the oxygen I spectrum measured by Hopfield. The large images show that the distribution of the light around the sun differs very much in these two lines. Furthermore, the detailed structure in the coronium rings is radically different from that of the H or K or hydrogen alpha lines of the chromosphere, and also differ from the details shown in direct photographs of the sun from integrated light. In other words, the solar activity cause the coronium lines to appear near where prominences are visible, but the differences in structural detail make us believe that the green coronium line is not associated with the calcium atom which produces H and K nor with the hydrogen atom. The brightest coronium rings show absorption close to the edge of the sun, which seems to prove that coronium lines are not ' forbidden ' lines.

The discussion of the 1930 flash spectrum obtained in good focus from wavelength 3230 to 7800 will have to await a future occasion.

Mr. R. Stoneley.—*Deep Focus Earthquakes.*

It has been pointed out by Dr. Harold Jeffreys that, in accordance with a general reciprocal theorem in dynamics, the surface waves of deep-focus earthquakes should be relatively small in amplitude.

The presence of readings for L and M in earthquakes whose foci are given in the *International Seismological Summary* as 0·07, 0·08, and in one case 0·09 of the earth's radius below normal, seems *prima facie* to constitute a great difficulty in accepting the great focal depths found by Prof. H. H. Turner.

It turns out on examination of the times of transit of the listed L and M that the times correspond to the arrival of S, SS, SSS, and sometimes to G, Gutenberg's ' Early Long Wave ' (4·35 km./sec.). The examination of actual records of earthquakes of very deep focus at once confirms this : nearly all the energy is transported in the body waves, so that P, PP, PPP, S, SS, SSS, etc., are of enormous amplitude, whereas the surface waves are inconspicuous.

The reciprocal theorem argument accordingly confirms Turner's discovery that occasionally earthquakes occur with foci considerably below the normal level.

Dr. R. v. d. R. Woolley.—*Intensities of Lines in Solar and Stellar Spectra.*

Information concerning the physical state of the outer layers of stars can be obtained from the investigation of absorption lines in stellar spectra. Temperatures and pressures can be deduced from the numbers of atoms in various states of excitation and ionisation, by the well-known formulæ of Saha and of Milne. The intensity of an absorption line is a measure of the number of atoms in the lower state concerned in its formation, but there are certain difficulties in the elementary theory of Stewart and of Unsöld, which give the result that the relative number of atoms is proportional to the square of the width of the line, measured at any point (Unsöld's formula).

On the experimental side several investigators have found that the intensities of multiplets in the solar spectrum do not agree with theoretical intensities which have

been confirmed in the laboratory, and that if an attempt is made to calibrate Rowland's scale of intensities, using Unsöld's formula, the result does not agree with Adams and Russell's calibration, based on multiplet intensities. A direct check on Unsöld's formula is provided by telluric oxygen lines. These increase in width as the sun sinks from the zenith, and from them the relation between line width and number of absorbing atoms can be found. The experimental result shows a distinct deviation from Unsöld's formula.

On the theoretical side the phenomenon of interlocking has been neglected, that is to say, the necessary connection between lines having the same upper state has an effect on the intensities of these lines in absorption, which is not taken into account by the elementary theory. Further, it is assumed that when an atom absorbs a quantum in the wings of a line, it must necessarily emit a quantum of exactly the same frequency, and not, for example, sometimes emit the central frequency of the line. No laboratory evidence on this point is available. The astrophysical phenomena may throw light on this and on other questions connected with resonance radiation, which have so far proved too difficult to observe in the laboratory.

Dr. B. F. J. Schonland.—*Lightning.*

An account will be given of recent investigations upon thunderstorms in South Africa, and their bearing upon the question of lightning discharges between the cloud and the ground. Evidence will be presented which makes it difficult to accept the view that the branches in a lightning flash fork away from the positive pole of the discharge.

Mr. R. A. Watson Watt and Mr. O. F. Brown.—*Radio Research in the British Empire.*

While the participation of the British Government in radio research dates back to the experiments of Captain Jackson in the Navy and to the contemporaneous co-operation of the Post Office in Mr. Marconi's early work, it was not until the formation in 1921 of the Radio Research Board, under the Department of Scientific and Industrial Research, that a systematic programme of fundamental research was undertaken under official auspices. The present communication deals chiefly with the results obtained in the research work carried out by the Board, which has been concerned very largely with the effects of the media on the propagation of electromagnetic waves and with the nature and origin of atmospherics. It then refers to the corresponding Dominion institutions more recently established in Australia, New Zealand and Canada, and to the problems which they are attacking. Special reference is made to the necessarily world-wide scope of fundamental radio research.

Monday, September 28.

Discussion on *Magnetic Storms and Ionisation in the Upper Atmosphere.* (Prof. S. Chapman, F.R.S.; Prof. E. V. Appleton, F.R.S.; Mr. A. H. R. Goldie; Mr. W. M. H. Greaves; Prof. J. C. McLennan, F.R.S.)

Prof. E. V. Appleton, F.R.S.

The evidence of wireless transmission has shown that there are two principal regions in the upper atmosphere responsible for the reflection of wireless waves. The diurnal and seasonal variation of the ionisation in the lower one (the Kennelly-Heaviside Layer) has been studied in some detail during the past year. The maximum noon ionisation is found to be six or seven times that at midnight, while summer noon ionisation is two and a half times that at winter noon. The corresponding relations for the upper region, the ionisation content of which is greater, have not yet been determined in such detail, but it is believed that the diurnal and seasonal variations are not so large.

Observations made during periods of magnetic activity show that the principal change from normality is in the lower region, where the ionisation is usually increased. A common occurrence is for the ionisation to be increased two or three times.

The theory of Maris and Hulbert (based on the experiments of Hafstead and Tuve)

that the height of the ionised layer is very much increased during a magnetic storm does not seem acceptable. Hafstead and Tuve used 70-metre waves, which at vertical incidence would normally penetrate the lower region and be reflected by the upper region. An increase in the ionisation of the lower region would reduce the group velocity of the waves when passing up and down through that region, and thus give a fictitious increase in the height of the upper region, as measured by the echo-time method. In other words an increase in the ionisation in the lower regions produces an increase in the equivalent height of the upper region. Further evidence in favour of this alternative explanation is that during the storm which Hafstead and Tuve examined (October 18, 1928) the reflection of 70-metre waves was at times noted from the lower region, although reflection from that region was not observed on the days preceding and succeeding the magnetic storm.

Mr. A. H. R. Goldie.

The electric field in magnetic storms has been deduced from computations of the position, direction and strengths of electric currents capable of producing the magnetic displacements recorded at Lerwick, Eskdalemuir and Abinger observatories during storms; and from comparisons of these records with those of other observatories, in particular Sitka, Sodankyla, Colaba and Mauritius. The electric field so deduced appears in the northern hemisphere to have these features. Flowing northward across the latitude of about 30° N. and between the longitudes of about 11h. and 18h., there is, as on quiet days, a great belt of positive electric current. This current, however, as compared with that of quiet days, is multiplied by a factor of the order of two to five according to the intensity of the storm. Of this magnified current the part lying between 18h. and about 14h. veers round as it proceeds northward until it passes along the auroral zone, appearing in the longitudes of 18h. to 21h. as a greatly concentrated current flowing from about W.S.W.; the other part, namely that lying between about 14h. and 11h., bends in the other direction and finally flows as a concentrated current from about E.N.E. along the auroral zone between the longitudes of about 7h. to 1h.

The heights of the current centres in the auroral zone lie mostly between 200 and 350 km., but individual cases range from under 100 km. to over 500 km. Midwinter and quiet years are characterised by currents low in strength and in altitude and considerably inclined to the W.—E. direction. Other parts of the year and disturbed years generally are characterised by the opposite features. The stronger a current the greater appears to be the height of atmosphere to which it extends.

In the auroral zone between the longitudes of 21h. and 24h. or 1h. is the meeting place of the westerly and easterly electric currents. What happens here evades calculation, but at all events the amount of current that again emerges from the auroral zone and passes southward (to enter subtropical regions between the longitudes of about 19h. and 1h. as a northeasterly current) appears to be only about half of that which originally flowed northward. The region, therefore, in which visible aurora is most frequent is thus seen to be also the region of dissipation of positive electric current, that is, unless—following the Birkeland-Störmer theory—it be regarded as a region of origin of negative electric current, coming from the sun and injected at this point.

For reasons given in the paper, the author is inclined to the view that the electric current, being mainly transverse to the magnetic field, is almost wholly a flow of positive ions, and that the electric field, both on quiet and on disturbed days, is generated on the sunlit side of the globe and mainly in tropical and subtropical regions; and that the difference between quiet and disturbed days is one arising from the effect of the sun in controlling the conductivity of the higher atmosphere over the daytime region. The ionization of the atmosphere in auroral and polar regions is considered to arise mainly from the passage of electric currents generated in the sunlit regions of lower latitudes.

Calculation suggests that forces of electrical origin may play a considerable part in modifying or controlling the movement of the atmosphere at great heights, at least in the auroral zones.

Dr. G. M. B. Dobson, F.R.S.—*Recent Researches on Atmospheric Ozone.*

Proportions of the different gases in the atmosphere. Small amount of ozone. Reasons for special interest in knowing the amount of ozone. Connection with weather conditions. Distribution over the World and annual variations.

Latest photoelectric instrument for measuring the amount of ozone. Ease of measurement. Observations possible on clear or cloudy day. Object of instrument to enable daily observations to be made regularly. Programme for systematic observations over N. Europe.

Photoelectric instrument having great sensitivity allows observations to be taken either in direct sunlight or the light from the clear zenith sky until the sun is nearly setting. Method of deducing height and distribution of ozone through the atmosphere from such observations. Average height probably about 40 km. distributed between the surface and 100 km. but further observations necessary.

Effect of ozone in raising the temperature of the atmosphere at great heights. High temperature causes bending down of sound waves and gives rise to audibility of explosions at great distances. Temperatures of upper air calculated from absorption of sunlight by ozone and from observations of sound waves agree in general features.

Prof. S. Chapman, F.R.S.—*Atmospheric Absorption of Solar Radiations and some Associated Phenomena.*

The absorption of solar radiations in the upper atmosphere of the earth is discussed, taking account of the curvature of the level surface. Diagrams are given to illustrate the degree of absorption at noon in high latitudes in winter and in other latitudes during the hour before the sun has become visible at ground level. The results bear on (*a*) the annual variation of ozone in high latitudes (*b*) the daily variation of ionization and (*c*) the emission of light by excited atoms in the upper atmosphere.

Report of Committee on *Seismological Investigations* (see p. 253).

SECTION B.—CHEMISTRY.

Thursday, September 24.

Presidential Address by Brig.-Gen. Sir Harold Hartley, C.B.E., F.R.S., on *Michael Faraday and the Theory of Electrolytic Conduction* (see page 31).

Discussion on *The Influence of the Medium on the Properties of Electrolytes.* (Introduced by Sir Harold Hartley.)

Prof. P. Debye.—*The Relationship between Conductivity and Frequency in Different Solvents.*

Sir Harold Hartley.—*Evidence from Conductivity Data.*

Prof. N. J. Bjerrum.—*The Forces between Ions and Solvent Molecules and their Relation to the Solubility of Electrolytes.*

Prof. J. N. Brönsted.—*Some Aspects of the Medium Effect on the Solubility of Electrolytes.*

Prof. K. Fajans.—*Factors determining the Forces between adjacent Ions in Solution.*

Prof. J. C. Philip, F.R.S.—*A Comparative Study of the Nitriles as Solvents.*

Dr. E. A. Guggenheim.—*Ionic Equilibria.*

Dr. J. A. V. Butler.—*Behaviour of Electrolytes in Mixed Solvents.*

Prof. G. Scatchard, Dr. E. Lange, Prof. J. W. McBain, F.R.S., Prof. T. M. Lowry, F.R.S., Dr. N. V. Sidgwick, F.R.S.

Friday, September 25.

DISCUSSION on *The Chemistry of the Vitamins and Related Substances.* (Introduced by Sir FREDERICK GOWLAND HOPKINS, Pres. R.S.)

(*a*) *The Chemistry of Vitamin A and the Carotinoids.*

Prof. P. KARRER.—*The Chemical Constitution of Carotene and Related Substances.*

The carotinoids are a group of yellow pigments, which occur in plants as well as in animals.

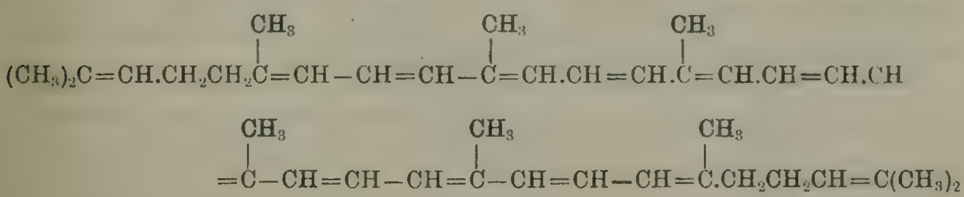
A considerable number of double linkages characterises their chemical constitution.

At present we know ten basic substances belonging to this group, and of these ten, two, *Lycopine* and *Carotene*, are hydrocarbons, having the composition $C_{40}H_{56}$.

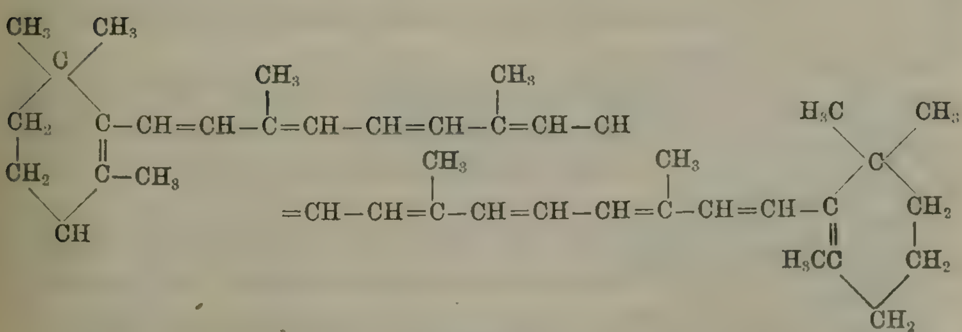
Two ways have proved particularly helpful in elucidating their constitution; catalytical hydrogenation, which gives the number of double linkages in the molecule, and degradation by oxidation, with ozone, potassium permanganate or chromic acid. Oxidation breaks up the large molecule, and from the fragments we gain an insight into the structure of the carbon chain.

Lycopene $C_{40}H_{56}$, the pigment of the tomato, has thirteen double linkages, and is therefore an entirely aliphatic compound, with an open carbon chain. The results of the oxidation indicate for Lycopene a configuration most probably like formula I.

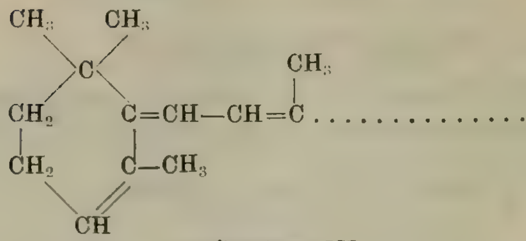
Carotene occurs in two modifications, designated as α- and β- carotene. Of the two isomeric forms the α-modification shows a high degree of optical activity; the β-carotene is optically inactive. The two modifications have a different melting point and also differ in their absorption spectra. Both carotenes have eleven double linkages and two carbon rings. The isomerism is brought about by a different arrangement of the double linkages. Formula II gives, with a high degree of probability, the constitution of the symmetrical optically inactive β-carotene. The assumption of this constitution is greatly strengthened by the results of the oxidation (6 mol. acetic acid and geronic acid). In the α-carotene it seems probable that one of the carbon rings has a configuration, as represented by formula III. It is possible to picture the two carotenes as having been derived from lycopene, by the closing of two carbon rings, at the ends of the open chain.



Lycopene I.



β—Carotene II.



α-Carotene III.

Partial hydrogenation of the carotenes leads to the di-hydrocarotenes.

The constitution of lycopene shows a similarity to that of squalen, a hydrocarbon derived from shark oil. The synthesis of squalen from farnesylbromide has established for this compound the symmetrical formula IV.

$$(CH_3)_2C{=}CH.CH_2CH_2\overset{\displaystyle CH_3}{\overset{|}{C}}{=}CHCH_2CH_2\overset{\displaystyle CH_3}{\overset{|}{C}}{=}CH.CH_2CH_2CH{=}\overset{\displaystyle CH_3}{\overset{|}{C}}.CH_2CH_2CH$$

$$={\overset{\displaystyle CH_3}{\overset{|}{C}}}\ CH_2CH_2CH{=}C(CH_3)_2$$

Squalen IV.

Vitamin A from cod-liver oil is not identical with carotene. Through saponification of the liver oil from Hippoglossus we prepared a residue which, after further purification, gives with antimony trichloride a blue colour reaction which corresponds to 10,000 C.L.O. units, which is about 130 times as strong as that given by carotene or about 10,000 times stronger than natural cod-liver oil.

Highly active vitamin A preparations give upon ozonization, just like carotene, geronic acid. The vitamin A of cod liver oil must therefore contain in its molecule the same carbon ring as carotene, and in that sense could be regarded as a derivative of carotene.

Prof. H. von Euler.—*Biochemical Experiments with Carotenes and Vitamin A.*

In close connection with Prof. Karrer's preceding paper on the chemistry of carotene and carotinoids the author gives a summary of his and Mrs. v. Euler's recent results concerning the biological and biochemical effects of the carotenes, some carotene-derivates and other growth-promoting factors.

Since it was established that two forms of carotene exist in plants and that a third isomeric carotene can be regenerated from carotene-triiodide, the growth response of these three isomers has been determined both on rats and some other animals. The increase of weight, produced by different additional daily doses of these carotenes, will be shown by curves.

The transformation of the carotene isomers into other growth-promoting factors, related to the ' vitamin A ' of the cod-liver oil has been studied. Experiments have been made to determine the distribution of the growth-promoting factors in liver and in blood-serum of different animals, examined at different ages and under different physiological circumstances.

It is the current view that only one ' vitamin A ' is present and acting in the animal body. The discovery of the heterogeneous nature of carotene seems to support the suggestion that there are several fat soluble factors of the vitamin A type.

The equilibrium between the carotene isomers, the reduction products of carotene and ' vitamin A ' will be mentioned and discussed in connection with the rôle of carotene in plant tissues.

' Vitamin A ' concentrates with an extremely high biological activity, giving a more than 100 times stronger Carr-Price reaction than carotene, have been obtained from animal oils. Some biological and physical properties of these concentrates have been measured.

It is known that vitamin D has a growth-promoting effect ; this effect is influenced by the presence of other vitamins and of several salts.

Prof. I. M. Heilbron, F.R.S., and Dr. R. A. Morton.—*Spectroscopy and Vitamin A.*

Fish and mammalian liver oils and concentrates exhibit an absorption band in the ultra-violet (maximum 328μμ), the intensity of which has been found to vary over the range 1–20,000, and which can be determined to within ± 5 per cent. With antimony trichloride the same materials give a blue solution, the depth of colour extending over the range 1–60,000 at least. The blue solutions exhibit initially two absorption bands (maxima 572–583μμ, 600–620μμ), either of which may be so much more intense than the other as to make detection of the second band difficult in extreme cases. The longer wave-length band is usually the stronger, but the shorter wave-length maximum can always be detected by applying the colour test in a modified form, $\frac{1}{15}$ of the usual quantity of reagent being used 1–1½ minutes before the rest is added. This procedure depresses the 600–620μμ band without noticeably affecting the 562–583μμ band.

In many instances the latter band predominates in the colour test. If an oil giving this result is exposed to the action of ozonised oxygen or hydrogen peroxide, a considerable increase in the intensity of the 600–620μμ band is usually seen when the oil is re-tested with antimony trichloride, but this increase is not at the expense of the 562–583μμ band.

Liver oils thus contain two chromogenic substances which behave as separate entities although the evidence does not exclude kinship. The 562–583μμ chromogen varies directly as the 328μμ absorption band, but the 600–620μμ band and the 328μμ maximum appear to arise from different substances. Comparisons of physical with biological data on a series of oils and concentrates indicate that the 328μμ band and the 562–583μμ component in the colour test are valid quantitative criteria of vitamin A potency.

The available evidence concerning the molecular weight of vitamin A and the percentage of the richest concentrates unaccounted for as inactive substances leads to a very high molecular extinction coefficient (order 10^6) at 328μμ, and a chromogenic value for the intensity of the blue colour with antimony trichloride many times greater than that of any known sterol or carotinoid.

Prof. R. Kuhn.—*Preparation of Isomeric Carotenes and their Biological Effects.*

Three different methods for the preparation of α-carotene have been worked out which, regardless of the source of the material, lead to products having identical absorption spectrum and optical activity. The ratio between α- and β-carotene varies significantly in green plants. The pigment of palm-oil contains relatively the largest amount of α-carotene. On the other hand, the carotene in ovaries is optically inactive.

The effect of the purest preparations of the carotene components on the growth of rats will be described, especially the effect of preparations which have been purified by partial oxidation and by other chemical methods. Finally, a report will be made on the physical and biological examination of the liver oils obtained after feeding rats with α- and β-carotene.

Dr. T. Moore.—*Steps in the Concentration of Vitamin A.*

Although the high vitamin A content of mammalian livers has long been realised, most serious attempts at the isolation of the vitamin have been based on the use of cod-liver oil, or some similar fish-liver oil, as the starting material. Concentrates prepared from cod liver oil are not usually effective in rat tests at doses lower than 0·01 mg., and give values of 200–400 blue units per mg. in the antimony trichloride reaction. (A typical cod liver oil before treatment may be taken as giving about 1·0 B.U. per mg.) Almost all natural mammalian liver oils are richer sources of the vitamin than cod-liver oil, although this superiority may be set off against the much smaller amount of oil present per unit of liver. The mammal offers an additional advantage in being amenable to artificial feeding before treatment of the liver, by which means the vitamin A content may be raised to very high levels.

Experiments have been carried out upon several groups of rats which have received in their diets either carotene (as 15 per cent. of red palm oil) or preformed vitamin A

(as 15 per cent. of cod-liver oil). The capacity of the animal for absorbing reserves of vitamin A has been found to be extremely high, the limit reached being about 100,000 B.U. for the entire liver, or about a century's supply assuming utilisation at a rate equal to the minimal curative dose. The individual liver oils sometimes gave colour values as high as 600 B.U. per mg. After saponification and the removal of crystalline matter concentrates were obtained having values of 2,000 B.U. per mg. in the case of the rats fed with red palm oil (biologically active at 0·001 mg.), or 1,200 B.U. per mg. in the rats fed with cod-liver oil. A point of interest lies in the constancy of the final values obtained, which did not seem to be affected by the individual values of the various samples before saponification, even when these were widely divergent. Some hope may therefore be entertained that the vitamin at least presents a considerable constituent of these preparations. Experiments on a larger scale, using pigs, have now been started.

The above results shed some further light on the changes which must occur when carotene is converted to vitamin A by the animal. Since carotene has a colour value of only about 200 B.U. per mg. it must be implied that conversion is accompanied by at least a tenfold increase in colour value. On the other hand, it does not seem necessary to assume that the spectroscopically different colour units given by carotene and the vitamin must bear the same relation to biological activity. As far as could be judged from a limited number of tests the best concentrates were not more than 2–3 times as active as crystalline carotene.

Prof. J. C. Drummond.

Prof. I. M. Heilbron, F.R.S.

Afternoon.

(b) *The Chemistry of Vitamin B and Related Problems.*

Prof. B. C. P. Jansen.

The method by which Dr. Donath and I isolated the anti-neuritic B_1 vitamin was the following:—

One hundred kg. of rice-polishings were extracted by water; the dissolved vitamin is adsorbed by 3 kg. of acid clay; from this it is eluted by baryta. This solution is acidified and successively treated by silver nitrate and baryta, by phosphotungstic-acid and, in alcoholic solution by an alcoholic solution of platinum-chloride. After decomposing the platinum-precipitate the solution in absolute alcohol is fractionately precipitated by acetone; after several fractionations we got 30 milligrams of pure B_1-vitamin-hydrochloride; 100 kg. of rice-polishings contain about $1\frac{1}{2}$ gram of B_1 vitamin; while our procedure gives after several months' work only 30 milligrams, so I tried to improve the method. Phosphotungstic-acid was substituted by the more specific silico-tungstic-acid. As Peters first showed, fractionating with silico-tungstic-acid gives a much better output. The decomposed platinum-salt may be further purified by gold-chloride (Drummond). Seydell and van Veen remove many impurities by benzoylation.

Prof. R. A. Peters.—*The Vitamin B Complex.*

This summary is an account of work by Messrs. H. Barnes, J. R. O'Brien, C. W. Carter, R. B. Fisher, N. Gavrilescu, H. W. Kinnersley and Dr. V. Reader.

To solve the chemistry of the vitamin B complex, we must know how many compounds can be isolated from it, which exhibit independent physiological actions. At this stage it seems premature to consider whether such factors have quite different chemical structures or whether the independent activities are due to different chemical groupings attached to the same nucleus. Estimation of B_1 may be influenced by the presence of other factors; in the rat by B_4 (and to a less extent, B_2), and in the pigeon by B_5 (and perhaps B_3). B_1 is concerned with lactic acid metabolism, and the lowered brain oxidation in its absence appears to be restored by addition of B_1 in vitro, providing thereby hope of an *in vitro* test. Yeast B_1 concentrates curing pigeons and rats

in doses of approximately 12γ/diem (γ=1/1,000 mg.) have been obtained, the corresponding value for Jansen's crystals from rice polishings being 8γ. The methods used are aqueous extract, purification with lead acetate, baryta and mercuric sulphate, followed by absorption upon charcoal at pH 7·0. By successive treatment with dilute HCl (X extracts) and alcoholic HCl (Y extracts), it is found that X contains mostly B_1, whereas Y, B_1, B_4 and B_5. Both are free from B_2. Alcohol fractionation combined with new methods of using phosphotungstic acid rapidly gives material of 50γ/ diem, and the constancy of precipitation of B_1 with phosphotungstate at *p*H 5·0–7·0 shows that this is a property of B_1. B_1 is soluble in absolute alcohol as hydrochloride (so also B_4 and B_5). It is insoluble in lipoid solvents. Much impurity may mask these solubilities. It is resistant to acid hydrolysis, withstanding even boiling with strong nitric acid; it resists oxidising and reducing agents, also treatment with nitrous acid, but is inactivated by alkali. The facts suggest a tertiary base which is also supported by the *p*H at which pure B_1 is absorbed upon charcoal, *p*H 9·0 (Phelps). The reaction given by amounts of B_1 of two- to four-day doses with diazotised sulphanilic acid (Pauly reaction) is yellow with a trace of pink; the pink colour is enhanced, however, by choice of a suitable alkalinity for the coupling.

Vitamin B_4, often confused with B_1 in curative rat tests, is apt to accompany B_1 closely, being present in a sample of Jansen's crystals recently tested. Difference from B_1 is established by (*a*) separation by adsorption upon charcoal at acid *p*H prior to adsorption of B_1, and (*b*) the wide divergence between the ratio B_1/B_4 in different X and Y concentrates. Following the tryptophane closely, it is precipitated by mercuric sulphate and disappears upon prolonged treatment with dilute acid. Vit. B_5 tends to follow B_1 in phosphotungstic fractionation, and is probably basic. It seems more stable to alkali than B_1. By the use of the pure 24 phosphotungstic acid it is hoped to obtain sharp separation of B_1, B_4 and B_5. Vitamin B_3 is not extracted from wheat germ by warm 97 per cent. alcohol. When combined, it withstands heat, and also acid hydrolysis, but rapidly disappears when liberated from combination. This constitutes a difference from other factors.

Prof. J. C. Drummond.

(*c*) *The Chemistry of Vitamin D and Related Sterols.*

Mr. R. B. Bourdillon and Dr. R. K. Callow.—*Crystalline Preparations of Vitamin D.*

Attempts to isolate a crystalline vitamin D from the resinous mixtures obtained by the ultra-violet irradiation of ergosterol have now been partially successful in three different countries. In England a group of workers at the National Institute for Medical Research have obtained a crystalline product of m.p.124° C and $\alpha\frac{20}{5461}+260°$ (in alcohol), by vacuum distillation of irradiation products of ergosterol, in a still designed to secure fractional condensation of the vapours. This product appears to form esters, and to be an isomeride of ergosterol. In Germany Prof. Windaus has removed some of the irradiation products of ergosterol by condensation with maleic anhydride, and has obtained from the residues a crystalline product somewhat similar to the English one, but showing an appreciably lower specific rotation. In Holland Drs. Reerink and van Wijk have obtained a crystalline product by irradiating ergosterol under very carefully controlled conditions. The physical properties of this product have not yet (July, 1931) been fully described, but it appears to be more easily oxidised, and to have lower detro-rotation than the other two crystalline products.

It is suggested that all three products may be mixtures in different proportions of two nearly isomorphic antirachitic compounds, one showing a high dextrorotation and considerable stability, and the other less stable and with laevorotation or low dextrorotation.

Prof. A. Windaus (not communicated in person).

The physical and chemical properties of ergosterol will be considered, with especial reference to its behaviour on hydrogenation, dehydrogenation and oxidation. Mention

will also be made of the action of maleic anhydride upon ergosterol and the effect of removal of water from the ergosterol molecule. These subjects will be discussed from the point of view of the constitution of this sterol.

The formation of the isomers of ergosterol under various conditions will be dealt with. These include the action of hydrogen chloride upon ergosterol, the treatment of ergosterol with finely-divided nickel, and the effect of heating the sterol with sodium ethoxide at 200°. Isomerisation of ergosterol can also be brought about by the action of ultra-violet light, whereby a number of compounds are formed ; of these at least one possesses outstanding importance in virtue of its being the antirachitic vitamin.

Consideration will be given to the process of irradiation and the compounds thereby produced. Their physical and chemical properties will be compared with ergosterol, and an interpretation of the photochemical reaction whereby vitamin D and the products arising from over-irradiation are formed will be attempted.

Dr. E. H. Reerink and Dr. A. van Wijk.—*Isolation of a Crystalline Antirachitic Reaction-Product from Irradiated Ergosterol.*

It is possible to obtain a crystalline reaction-product from irradiated ergosterol in the following way : ergosterol, dissolved in pure peroxyde-free ether, is irradiated, under complete absence of oxygen and under good conditions of stirring, with ultra-violet light, from which wave-lengths below 284 $m\mu$ are excluded. The irradiation is stopped when about 25–30 per cent. of the ergosterol have been transformed. The unchanged ergosterol is eliminated by recrystallisation from ether and alcohol, while residual traces may be precipitated with digitonin. All operations have to be performed in vacuo. After evaporation of the last solvent the reaction-product is obtained as a white crystalline mass.

It has been proved that this isolated reaction-product is not stable in solution in vacuo ; accordingly, its physical properties vary somewhat with the exact condition of the procedure of the isolation (*e.g.* the duration and the temperature), and do not tally exactly with those calculated from the mixture of ergosterol and reaction-product immediately after irradiation. The antirachitic activity, however, has not perceptibly changed. The melting-point is generally 115°–117°, and the substance very rapidly oxidises in presence of oxygen. The possible relation of this substance to the crystalline preparations recently obtained in the National Institute for Medical Research, and by Windaus, is discussed.

Prof. I. M. Heilbron, F.R.S., Dr. F. S. Spring, Dr. D. G. Wilkinson, and Mr. J. C. E. Simpson.

A series of ergosteryl ethers has been prepared and is described. The absorption spectrum of methyl ergosteryl ether is identical with that of ergosterol itself, but on irradiation it fails to produce an antirachitic product.

Conclusion of Discussion by Prof. R. Robinson, F.R.S.

Monday, September 28.

Symposium on *The British Fuel Problem*, by Sir David Milne-Watson (*Coal*), Sir John Cadman (*Oil*), and Mr. H. T. Tizard, C.B., F.R.S. (*Future possibilities*). Sir James C. Irvine, F.R.S., and Prof. W. A. Bone, F.R.S.

Tuesday, September 29.

Discussion on *The Structure of Simple Molecules*. (Introduced by Prof. P. Debye.)

Prof. P. DEBYE.—*Interferometric Measurements of Atomic Distances in Free Molecules.*

Interferometric measurements of atomic distances in single free molecules are possible because even a random orientation of the molecules, as it occurs in gases, cannot completely obliterate the intensity fluctuations in the angular distribution of the scattered radiation, which would have to be called self-evident in the case of a single molecule of definite orientation.

Now in many cases this special interference effect has been found experimentally with X-rays and also with cathode rays. In the very beginning it was considered sufficient to register the angles for which intensity maxima or minima occurred and to derive the atomic distances from the comparison of the experimental result with a theoretical intensity curve which had been calculated assuming the atoms to act as scattering points. Soon it became evident that as the electrons of every atom are distributed over a region with dimensions comparable with the wave-length of the primary radiation, the interference effect of the scattered wavelets coming from different points of every single atom had to be taken in account. This was done by the introduction of an atom form factor, as it is used in discussing the intensity of the X-ray reflection by crystals. From an experimental point of view this presupposes the observation of the total intensity curve for the angular distribution. It is certainly not sufficient to examine simply the photographic film obtained, which by a well-known physiological effect gives the impression of maxima and minima even if only slight fluctuations in the blackening exist. In comparing the experimental intensity curve with the theoretical curve obtained after the correction for the electron distribution in the atom, *e.g.* the distance Cl—Cl in the CCl_4 molecule could be determined to 2,99 Å with an error of ± 1 per cent. But even so the correspondence between the experimental and the theoretical curve is not perfect. In general the experiment gives—

(*a*) More radiation for large angles than is theoretically calculated ; and

(*b*) The difference in intensity between succeeding maxima and minima is less than predicted.

During the last year these differences have been considered and explained.

(*a*) The scattering of X-rays by an atom does not only give coherent radiation of the same wave-length, but also a certain amount of incoherent radiation with changed wave-length (Compton-Effect). The incoherent radiations from different atoms of the molecule cannot interfere. Whereas the coherent radiation sets in for the angle zero between the primary and the secondary ray with an intensity proportional to the *square of the number* of electrons contained in the atom and falls off with increasing angle, the incoherent radiation starts with the intensity zero, it increases with increasing angle, and ultimately can reach an intensity proportional only to *the number* of electrons. A few months ago the calculation of the angular distribution of the incoherent radiation was only possible starting with the knowledge of the density distribution for every single electron. Shortly, however, Heisenberg succeeded in giving a general and simple formula for this radiation, which, as in the case of the atom form factor for the coherent radiation, enables us to derive its angular distribution from one single curve valid for all atoms.

(*b*) So far the atoms in the molecule had been considered as rigidly connected to each other. We know, however, *e.g.* from the evidence of specific heat measurements, that vibrations exist, and therefore the distances between two atoms are only constant in the average. It is possible to calculate the effect of the vibration on the angular distribution of the scattered intensity, and it is seen that it lessens the difference in intensity between maxima and minima. In this way the experimental intensity curve also gives evidence of the average amplitude of the atomic vibrations, and leads *e.g.* for CCl_4 to an amplitude of approximately 0·2 Å, that is 6 per cent. of the distance Cl—Cl. Moreover, we know, *e.g.* from experiments on the Raman-Effect of CCl_4, the frequency of the vibrations. It can be shown, by comparing their energy quantum with the classical value for their energy at the temperature for which the experiments are carried out, that a considerable part of the vibration has to be considered as due to zero-point energy, which even at the absolute zero-point of the temperature would still exist.

It may be said now that the scattering curves can be explained in detail. Results for special molecules like CO_2, CS_2 to illustrate the effect of the differences in electronic density in different atoms ; CCl_4, $CHCl_3$, CH_2Cl_2, CH_3Cl to illustrate the distortion in

the molecules by substitution of different atoms ; $C_2H_4Cl_2$ to determine experimentally how much of the so-called 'free rotation' around a single C—C bond exists, &c., will be considered with the help of diagrams for the experimental scattering curves.

Prof. J. E. LENNARD-JONES.—*Evidence from Molecular Spectra.*

Mr. R. H. FOWLER, F.R.S.—*Quantum Mechanical Theory of Valency.*

Prof. V. HENRI.—*Predissociation Structure of some Special Molecules.*

Prof. W. HEISENBERG, Prof. MAX BORN, Prof. C. G. DARWIN, F.R.S., Prof. GOUDE SMITH, Dr. N. V. SIDGWICK, F.R.S., Prof. W. L. BRAGG, F.R.S., Prof. T. M. LOWRY, F.R.S.

Mr. C. N. HINSHELWOOD, F.R.S.

Exhibits during the Meeting :—

(*a*) *The Chemistry of Thallium.* (Dr. R. C. MENZIES.)

(*b*) *Some Recent Investigations of the Chemistry of Gold.* (Prof. C. S. GIBSON, O.B.E., F.R.S.)

(*c*) *The Chemistry of Rhenium.* (Prof. H. V. A. BRISCOE.)

(*d*) *Some Recent Investigations in* (i) *The Menthone Series,* (ii) *The Hydrobenzoin Series.* (Prof. JOHN READ, Mr. R. A. STOREY, Mr. W. J. GRUBB, and Dr. ISHBEL G. M. CAMPBELL.)

(*e*) Optical behaviour of ethyl cinnamate and formation of sodium *o*-nitrophenoxide from *o*-nitrophenol and glass (Prof. E. J. HARTUNG).

(*f*) Automatic continuous extraction of hard and soft water by means of standard soap solution (Dr. H. S. HATFIELD).

(*g*) At the Department of Chemical Technology, Imperial College. Special exhibits and experimental work in connection with the following researches. (Prof. W. A. BONE, D.Sc., F.R.S.)

(i) *Gaseous Combustion at High Pressures,* including superpressure bomb in which H_2-air and CO-air mixtures will be exploded at initial pressures of 1,000 atmospheres.

(ii) *Catalytic Reactions at High Pressures,* including the production of CH_3OH from $CO+2H_2$ mixtures and by the direct oxidation of methane.

(iii) *The Photographic Analysis of Explosion Flames.*

(iv) *The Chemical Constitution of Coal.*

(v) *Blast Furnace Reactions.*

(vi) *Combustion in Electrical Discharges : the Electrical Condition of Surfaces during Catalytic Combustion.*

(vii) *Problems in Chemical Engineering.*

SECTION C.—GEOLOGY.

Thursday, September 24.

PRESIDENTIAL ADDRESS by Prof. J. W. GREGORY, F.R.S., on *Problems of Geology contemporary with the British Association* (see page 51).

Mr. H. DEWEY and Dr. S. W. WOOLDRIDGE.—*The Geology of the London District.*

The London Basin is nearly coincident with the Thames drainage system, and extends eastwards from near Cirencester to the North Sea. On the south its boundary may be taken as the base of the Lower Greensand escarpment of N. Hants, Surrey and Kent, while the northern margin extends from north of Devizes to the Wash. This extensive area is underlain for the greater part by rocks of Cretaceous age, the centre of the basin being lined with the overlying Tertiary Beds. The solid rocks have been folded into an elongated syncline with its greater limbs puckered into smaller folds, and in part faulted. Across the solid rocks the Thames and its tributaries have cut a series of steps or terraces, and have laid down on them beds of gravel, brickearth and alluvium.

Many years ago the synclinal structure of the Cretaceous rocks led to the inference that a great reservoir lay under London formed of Lower Greensand beds; to tap the supposed reserves of water a borehole was put down at Meux's Brewery in Tottenham Court Road. It failed in its object but attained an important result in proving the presence at no great depth of Palæozoic rocks immediately beneath the Gault. These rocks are Devonian or Old Red Sandstone; the boring therefore proved that Coal Measures are absent under that part of London.

Subsequently about a dozen other boreholes have been made in search of the Lower Greensand, and have found it absent, but have proved the extension of the Palæozoic rocks from Essex to Bucks. At Richmond, Streatham and near Chatham the several divisions of the Jurassic thin out on the Palæozoic rocks. Further south the Palæozoic rocks have not been reached west of Bobbing, near Canterbury, the overlying Cretaceous and Jurassic rocks being of great thickness. This series of boreholes has therefore demonstrated the long history of the London area as one of a gradually sinking landmass that ultimately was overwhelmed by the Gault sea and continued to sink beneath its load of sediment for a prolonged period. A movement of elevation, however, reversed the process, and the Chalk became a land surface with a south-easterly tilt. Before subsidence had resumed its sway great thicknesses of the chalk had been removed, especially in the London area. This fact is proved by the relation of the fossil zones of the Chalk to the overlying Tertiary Beds.

The subsequent earth movements were not uniformly downwards but oscillating with the gain on the side of subsidence. Eastwards the depression was greater than on the west throughout Tertiary times, for the Thanet Sands die out west of Central London, the estuarine and freshwater Reading Beds are contemporary with the marine Woolwich series, the shingle banks of the Blackheath Beds do not extend far west, and the London Clay dies out completely and is overlapped by the Bagshot Sands at Great Bedwyn, Savernake, but after the Eocene the record is too incomplete to continue the history. Exposure of successively lower beds accompanied the depression, so that the Blackheath Beds recline on Chalk of the Cor-testudinarium zone, while the Thanet Sand rests only on the higher Marsupites zone. After the long period of clay deposition of the London Clay sea, when presumably clay beds of earlier periods contributed to its formation, earlier sand formations were tapped to supply the Bagshot Sands, and that these sands were in part at least of the Lower Greensand beds is proved by the occurrence of Hythe Bed chert detritus at the base of the Barton Sands of Surrey. Denudation had therefore locally at least exposed the base of the Lower Greensand before the Alpine movements had commenced with vigour. The Oligocene period had long continued before the Alpine storm sent waves outwards that affected the London district and folded the Cretaceous and Tertiary Beds into the London Basin. Planation occurred in Miocene times, and possibly some upland plains may be remnants from this period.

After the Pliocene uplift had led to over-deepening, the Glacial period began to alter the landforms and to modify the inhabitants.

Man then appeared, and the long-continued struggle to survive by subduing nature commenced. Little is known about him except that almost from the start he had a true eye for form and could not, or at any rate did not, resist the impulse to make his implements beautiful as well as useful. He depended upon raw material for the selection of his dwelling-place, and it was only where flint occurred in abundance that he could make his weapons, and this distribution led to the selection of sites for occupation mainly below the Goring Gap.

Good water has always been a necessary of life, and the London area is well placed for good supplies. First, the river and its tributaries ; next, the springs rising from the base of the terrace gravels ; finally, the artesian water from the Chalk. The last supply has been drawn upon in increasing quantities ; the over-pumping resulting in a permanent depression of the water-table and a progressive depression that continues and increases as more boreholes are sunk. Formerly the water from the Chalk rose under pressure, through the overlying Tertiary Beds, above the surface ; but to-day, under parts of the City and in Westminster, the water-table is below the top of the Chalk. The rate of fall is locally over 3 feet per annum. The exhaustion is augmented by great boreholes in the collecting ground of the Chalk outcrop along the North Downs and the Chilterns, and it may be that the basin will become nearly dry in course of time. This and the difficulty of disposing of sewage will tend to restrict the growth of the town.

The second part of the lecture (S.W.W.) deals with the structural and physiographic evolution of the London Basin as a unit. Attention is called to the systematic variation in the thickness of the several Eocene formations which, studied by means of isopachyte maps, renders it possible to trace the stages of growth of the main syncline and the minor structural features within it.

The later stages of development may be regarded as beginning with the transgression of the Pliocene sea into the main synclinal area. There followed a series of uplifts alternating with periods of base-levelling, recorded in the form of a number of dissected platforms or peneplains whose dates are known within reasonably narrow limits and whose history, including also that of the glacial deposits, forms a natural prelude to that of the Thames terraces at a lower level.

Mrs. Ross.—*The Physiographic Evolution of the Kennet-Thames.*

A short account of the river terraces of the Lower Thames and its tributaries is given, indicating their distribution both in plan and profile. The established terraces of the Thames (the Boyn Hill, Taplow and Flood Plain) have been traced up-stream into the Kennet Valley. Evidence is cited to establish two terraces above the Boyn Hill among the gravels now classed as ' Glacial ' or ' Plateau.' These it is proposed to name the ' Winter Hill ' and ' Binfield ' Terraces.

Mr. J. F. Kirkaldy.—*The Classification and Correlation of the Lower Greensand of the Western Weald.*

This paper summarises the results of a piece of work which has involved the complete revision on the 6-inch scale of the area around Hascombe, a detailed critical examination of the area east of Guildford, in which particularly interesting features are shown, and an outline survey of the Bargate–Sandgate Beds and associated strata throughout the Weald.

Some of the chief points at issue are :—

(i) The non-coincidence of the ' Bargate Stone ' (s.s.) with the ' Bargate Beds,' the strata having been clearly subject to irregular original calcification and patchy subsequent decalcification.

(ii) The widespread occurrence throughout the Western Weald of a phase of calcareous beds in the *Parahoplites nutfieldensis* sub-zone.

(iii) The existence of pre-Folkestone axes of warping which have very clearly influenced the lithology and thickness of the Bargate and associated beds. Renewed movement along some of these axes has affected the present physiography.

(iv) The correlation of the so-called Loamy Folkestone Beds with the Sandgate Beds.

(v) The existence of an important physical break at the base of the Folkestone Sands.

Friday, September 25.

DISCUSSION on *The Evidence of Palæontology with regard to Evolution.* (Sir ARTHUR KEITH, F.R.S.; Prof. H. L. HAWKINS; Prof. H. FAIRFIELD OSBORN; Dr. W. D. LANG; Prof. A. E. TRUEMAN; Prof. H. H. SWINNERTON; Prof. D. M. S. WATSON, F.R.S.)

Sir ARTHUR KEITH, F.R.S.

The oldest fossil remains of man so far discovered are four in number: those of Pithecanthropus found by Dubois in Java (1891–93); of Sinanthropus discovered in China (1926–30); of Eoanthropus found by Dawson at Piltdown, England (1911–13); of Palæanthropus, represented by a lower jaw, found near Heidelberg (1907). There is a general agreement that the four types of beings represented by these fossils must be accounted human, and that they differed in structure so much that they must be regarded as representing four separate genera of mankind, whereas all living races are regarded as members of one species. There is also a large measure of agreement as to the date at which these four extinct types lived, namely, in the oldest phase of the pleistocene period. It is true that the exact geological horizon from which the remains of Pithecanthropus and of Eoanthropus came is still a matter of debate, there being a tendency to allocate the Eoanthropic fossils to a late pliocene horizon and to bring those of Pithecanthropus well within the pleistocene. Besides these four early pleistocene forms there are two others, of later pleistocene date, worthy of mention because of the light they throw on the evolution of man, namely the Neanderthal type of Europe and the Rhodesian of Africa.

What light do these six extinct types throw on the evolution of modern races of mankind? We may at once say that as the geological record now stands we cannot trace modern man backwards to any of these six extinct types. Indeed, two of them we may eliminate at once—Heidelberg man and Neanderthal man. On anatomical grounds alone the transmutation of the Neanderthal type into man of the modern type seems most improbable. So far as concerns Europe the archæological evidence is now definite that Neanderthal man became extinct and was replaced by men of the modern type. On the other hand, the resemblances of the Heidelberg teeth and mandible to Neanderthal teeth and mandible are such that it seems extremely probable that Heidelberg man was ancestral to Neanderthal man. As to *Homo rhodensiensis*, he is the only extinct type so far discovered whose crude features certainly foreshadow those of modern man, but to which of the living races he may stand as ancestor there is at present no certainty. Future finds may clear up his relationships, but on rather meagre evidence. I incline to the opinion that Rhodesian man will turn out to be an early form of negro.

As to the relationship of the three early pleistocene types of mankind—Eoanthropus, Sinanthropus and Pithecanthropus—to each other and to the living races of mankind, there are many speculations but no definite data to guide us. I am less certain than I was that none of these is in the direct lineage of modern races. At one time I believed that the ape-like features in the jaw and teeth of Piltdown man excluded him from modern man's ancestry. The evidence obtained from the changes in the canine teeth and in the chin region of the jaws of other early types of mankind leads one to suspect that what one may call the humanisation of the chin and canines proceeded independently in diverse branches of evolving humanity. Nor do I now hold it to be impossible for the form of brow ridges found in Neanderthal man, Pithecanthropus and Sinanthropus to be converted, or reconverted, into the form now found in all modern races of mankind. The ancestral position in the lineage of modern races of these early pleistocene forms must be left open, to be settled by the discovery of mid-pleistocene representatives of humanity.

Although none of these early pleistocene forms may be ancestral to modern mankind, yet they do tell us much as to the general state which humanity had attained by the end of the pliocene and the evolutionary changes which marked man's progress during the pleistocene period. The skull capacities of the later pleistocene men were greater than those of the early pleistocene; during the earlier half of the pleistocene period the human brain underwent a marked increase in size and in complexity of convolutionary pattern. The jaws and teeth, on the other hand, underwent reduction. In only one of these early pleistocene men—Pithecanthropus—have we

evidence of posture and gait. The femur of the Java man is shaped so like to that of modern man that we must suppose his gait and carriage of body were as in us. Although we do not know the thigh bone of Piltdown man or of Peking man, yet their skulls are so human that we do not hesitate to ascribe to them limb bones which were equally human. We also know the limb bones of Neanderthal man. His carriage was upright but his gait must have been different from ours, for his femur is different.

We may therefore hold it as proved that before the end of the pliocene period man had attained perfect plantigrade progression. The debate which now centres round man's evolution is concentrated on the date at which, and the mode by which, man came by his lower limbs and peculiar carriage of body. We must therefore search older geological records for the evolution of man's carriage of body. The later records of the pliocene yield us little ; but the older half of the pliocene and the younger half of the miocene give us a great deal ; the adjacent halves of these two periods I will refer to as the 'age of the great anthropoids.' Alas ! When we search the records of the great anthropoid age, although we find parts of jaws and teeth representing at least a score of diverse anthropoid apes, several of which may be ancestral to pleistocene man, we have only two fossil limb bones to guide us as to the posture or gait of these extinct anthropoids. One of these is an imperfect humerus—the arm bone of a Dryopithecus—the other a femur of a smaller European anthropoid which Dubois named Pliohylobates eppelsheimensis—a pliocene relative of the modern Siamang of Sumatra. Of the great anthropoids—not a segment of a thigh bone—only fragments of jaws and teeth. How uncertain jaws and teeth can be as a guide to posture and humanity of body in the higher primates the four following instances will exemplify. A fossil tooth, obtained from pleistocene strata at Taubach (near Weimar), Germany, was assigned to an extinct form of humanity by one set of experts and to a great extinct ape (Dryopithecus) by another equally skilled set of experts. Discoveries subsequently made near Taubach revealed identical teeth socketed in a human jaw—that of a member of the extinct Neanderthal species (Ehringsdorf man). A fossil tooth, almost certainly from the same strata as yielded the remains of Sinanthropus (Peking man) was submitted in 1903 to Prof. Max Schlosser, who has made a special study of teeth of higher primates. He had to leave the diagnosis open ; it was simian as well as human in its character ; it might be the tooth of an ape or it might be of an early form of man. Dr. Davidson Black's discoveries prove that it was human. The lower jaw and teeth of Piltdown man have been regarded—and still are by certain experts—as those of an extinct form of chimpanzee—so hard is it to draw a line between the teeth and jaws of the earlier forms of man and the teeth and jaws of the higher kinds of extinct anthropoid apes. Or, take the case of the Taungs anthropoid (Australopithecus). Prof. Dart has succeeded in freeing the jaws and exposing the molar pattern of this extinct primate ; the molar pattern is the most human yet discovered in any ape. If only the teeth and jaws had been found they would have been assigned to an early member of the human family.

The teeth and jaws alone give uncertain guidance as to whether any given fossil form is to be assigned to the family of man or that of the Great Anthropoids. How are we to explain this remarkable resemblance in tooth and jaw of pleistocene man to miocene anthropoid ? By independent acquisition or by inheritance from a common stock ? I agree with Dr. W. K. Gregory in explaining the resemblance as an inheritance from a common stock. The great anthropoids of the miocene period, and the early pleistocene forms of man, are descendants of a common stock and that stock, were it living now, we should assign not to the human family but to that of the Great Anthropoids—with the gorilla, chimpanzee and orang.

All that we know of the great anthropoids of the upper miocene and lower pliocene periods is founded on the discovery of isolated teeth and fragments of jaws, carrying teeth. Let us glance at these fossil remains to see if among them we can recognise the ancestral form of either man, gorilla, chimpanzee or orang. In ancient Europe there were at least three kinds of great anthropoids—all assigned to one genus—Dryopithecus. In the Siwalik deposits of northern India Dr. Pilgrim and others have found fossil jaws and teeth which they have assigned to thirteen different species of great anthropoid. Six of these species are regarded as Dryopitheques, four as Sivapitheques and two as Palæopitheques. When we examine this assemblage of fossil jaws and teeth, representing at least sixteen different kinds of extinct anthropoid ape, can we find amongst them any shaping towards a human form ? Hardly one more than another. Several might be prehuman. I would not be surprised if several of the teeth at present assigned to two of the European dryopitheques—*darwinii*

and *rhenanus*—may yet prove to be assignable to early forms of man. We are equally at a loss when we search for the ancestors of the gorilla, chimpanzee and orang amongst these fossil teeth and jaws. And yet it is probable that, in the assemblage, the ancestral patterns of teeth and jaws of modern anthropoids are represented. Evolutionary changes have converted teeth of the Dryopitheque pattern into those we find in man and gorilla, chimpanzee and orang. Not until we have discovered parts of the skulls and limb bones of these fossil anthropoids can we clear up the relationship which the great miocene anthropoids hold to their modern representatives and to man.

When we go further back in the geological record in search of the ancestral forms of the great anthropoid stock of the upper miocene and lower pliocene, there is only one country at present which can help us. This is Egypt. In 1920 Dr. R. Fourtan made a discovery of the highest importance in deposits of the lower miocene at Moghara. He found teeth and jaws—representing two anthropoids—one a small form of Drypitheque—t he smallest so far discovered. The other there represented was a form of gibbon—Prohylobates. Dr. Pilgrim rightly claims that the Egyptian dryopitheque may be ancestral to some of his Siwalik anthropoids. Dr. Fourtan's discovery is important in another respect; it gives us definite information that the big-bodied higher primate was in course of evolution in early miocene times.

Egypt also yields us the only evidence as to the point of evolution reached by the orthograde primates at a still earlier time—the beginning of the oligocene. In 1910 Prof. Max Schlosser described teeth and jaws, which he assigned to a primitive and small form of gibbon—Propliopethecus; and to a Tarsioid—an early form of old-world monkey—Parapithecus. The teeth and lower jaw of Propliopithecus, the earliest gibbonish form known, serve so well as a prototype of the living gibbon that we feel justified in presuming that it too was orthograde in posture. We make the further presumption, namely, that this posture, peculiar to the higher primates, was evolved, or in course of evolution, at the beginning of the oligocene period. The small size of the jaw and teeth of Propliopithecus also supports the belief that at the beginning of the oligocene period the common ancestral stock of the orthograde primates was of small size. The discovery of Propliopithecus has this further importance—it serves as a fixed point from which students of human evolution may begin their speculations. Whether we suppose the human stock to have broken away in miocene times after the full evolution of the Dryopitheque type of anthropoid —as Dr. W. K. Gregory and Prof. Elliot Smith think most probable; or at a still earlier period—during the evolution of the big-bodied anthropoid type, as I believe; or at still earlier phases—as Dr. H. Fairfield Osborn and Prof. Wood-Jones think, yet we are all agreed to accept Propliopithecus as a starting-point from which to derive the evolutionary history of man and ape.

My instructions from the president of this section were to confine myself to the fossil evidence of man's evolution. Manifestly this is impossible; our search carries us very soon into a world of anthropoid apes; we cannot discuss man's origin apart from their origin. Now as the fossil evidence stands we could not from it alone construct man's genealogical history. The geological evidence is as yet subsidiary: it permits us to verify and amplify the theory of man's evolution which has been constructed from the evidence of anatomy, physiology and embryology. The geological evidence justifies us in regarding the family of gibbons as retaining more of the early common orthograde ancestor than the great apes or man do. It also justifies us in regarding the anthropoid body as earlier and less changed than the human body. Indeed, man is by far the most specialised and changed member of the orthograde stock. Geological evidence also supports the belief that the orthograde posture and the massive body began to develop early in the oligocene. The larger-bodied Dryopitheque type of anthropoid was in course of evolution early in the miocene, and that by the end of the pliocene there were large-bodied, relatively big-brained primates in which the lower limbs had become modified for plantigrade progression, and that during the pleistocene period this peculiar group of primates underwent further changes—an enlargement and complication of the brain with a reduction of tooth and jaw. So far as it goes, the palæontological evidence now available favours the theory formulated by Darwin in 1870, namely, that man and anthropoid apes are the descendants of a common stock. If in the next fifty years geological records accumulate at the rate they have done in these past fifty years, our knowledge of man's origin will be founded not as ours is on reasoned inferences, but on ascertained fact.

Prof. H. Fairfield Osborn.—*New estimates of the Length of Pleistocene Time and Means of dating the Stone Age Man by the Elephant-Enamel Method.*

The more or less perfect grinding teeth of fossil elephants have been found in association with human remains or implements from the first period of archæological discovery. These human fossils have been more or less roughly dated by the fossil elephants and other mammals with which they have been discovered, but quite recently the means has been found of making this dating much more precise. In brief, the enamel composing the ridge plates of the elephant grinders may now be very accurately measured, and to each human type may be assigned a precise elephant or stegodont enamel length. This new method of dating various stages of fossil men may be called *ganometric.* The minimum enamel length in an elephant is the upper Pliocene 825 mm.; the maximum enamel length is the upper Pleistocene 7,300 mm. in one elephant, and 9,700 mm. in another. The maximum enamel length in the stegodonts is far below that attained by the elephants.

This ganometric method may be combined with the most recent estimates of Pleistocene time based chiefly on the glaciological work of Leverett, and of de Geer and his former associate Antevs, who worked in the glaciated laminæ of Sweden and of the coast of New England. These results are put together for the present communication by Dr. Chester A. Reeds of the American Museum, as follows:—

'In general the early time estimates of Penck and Bruckner, in which 500,000 years are assigned to the glacial epoch, have been doubled to 1,000,000 years—a time extension in full accord with the remarkable evolution and migrations during this closing period of the Age of Mammals. F. Leverett, glaciologist of the U.S. Geological Survey, in a 1930 paper discusses the relative length of glacial and interglacial stages in America during Pleistocene time; he concludes that the entire epoch must have been 1,000,000 years in length, and that of this time 300,000 years were glacial and 700,000 years interglacial. He assumes that the glacial periods I–IV were of equal duration, 75,000 years each, and estimates interglacial 3 stage, the Sangamon Peorian, at 50,000 years, thus leaving 650,000 years to be divided between interglacial 2 and interglacial 1. In a revised classification of the Pleistocene soon to be published, entitled "The Classification and Duration of the Pleistocene Period," four epochs (series) will be introduced by Kay, the youngest of which will be the Eldoran.'

These results are presented in a chart 10 metres in length, in which the whole of Pleistocene time is subdivided into hundreds of thousands and tens of thousands of years. To the left of the chart the closing stage of 250,000 years of Pliocene time is added. To the right of the chart is added the 40,000 years of post-Pleistocene and recent time.

Superposed upon the chart are the duration periods of six of the independent lines of fossil Proboscidians, and the total enamel length attained in the evolution of the 3rd superior molars in each of these lines, namely:—

Glaciation IV	80,000	*Mammonteus primigenius.*
3d Interglacial	110,000	*Palæoloxodon germanicus.*
Glaciation III	90,000	
2d Interglacial	330,000	*Parelephas trogontherii.*
Glaciation II	90,000	
1st Interglacial	220,000	*Archidiskodon meridionalis.* *Palæoloxodon antiquus.*
Glaciation I	80,000	*Mammonteus astensis.*
Total	1,000,000	

As each of these elephants has its own duration period we are enabled to calculate the rate of evolution of the enamel plates in each phylum or line of descent as follows:—

Average Ganometric Evolution per 10,000 *years*

Archidiskodon, based on *A. imperator*	154 mm.
Parelephas, based on *P. progressus*	87 mm.
Mammonteus, based on *M. primigenius*	40 mm.
Palæoloxodon, based on *P. ant. italicus*	43 mm.
Loxodonta, based on *L. africana*	20 mm.
Elephas, based on *E. indicus*	50 mm.

As the fossil remains of elephants and of man prior to the period of burial are found chiefly in interglacial gravels and sands, we have double means of subdividing Pleistocene time, namely, by calculations based on the glaciations and on the elephant remains found within the interglacial stages. The most abundant or outstanding species of fossil and recent elephants thus far found are as follows :—

	Maximum Enamel Lengths
1. The Southern Mammoths, *Archidiskodon*	8,000 mm.
2. The Temperate ,, *Parelephas*	10,000 mm.
3. The Northern ,, *Mammonteus*	6,000 mm.
4. The straight-tusked Elephants *Palæoloxodon*	6,000 mm.
5. The African Elephants, *Loxodonta*	2,000 mm.
6. The Indian ,, *Elephas*	7,000 mm.

Dr. W. D. Lang.

Palæontologists are generally agreed that during Geological time organisms have evolved from previously existing organisms by changes of character. They think so chiefly because their material falls into graded series. They use three criteria of relationship, namely (1) Morphic similarity, by which organisms exhibit graded series ; (2) Order of succession in time, which shows graded series in chronological sequence—series which we call *lineages* on the assumption that the seriations are filiations ; and (3) Recapitulation, whereby an organism shows its ancestry in its ontogeny. Few are likely to dispute the principles implied in the first two criteria ; at most their adequacy to give proved lineages will be questioned : but periodically violent attacks, chiefly by embryologists, are made upon the principle of recapitulation. One has lately been launched with considerable force ; but it concentrates upon an exaggerated view of recapitulation, which few palæontologists have held, or are likely to hold, and admits much of what is left when these extreme views have been cleared away. While embryologists may admit recapitulation, palæontologists use it as a guiding principle.

Assuming that our method is sound, and that our supposed lineages are real filiations, it is then seen (1) that a similar evolution is run through by many lineages both contemporaneously and successively, and (2) that individual characters run through a similar development in parallel lineages, and develop in the same lineage to a large extent independently of other characters ; so that, by differences in rate of development of single characters, individual terms in parallel lineages may present a very different appearance, although they may be at the same general stage of development. This is Orthogenesis, as now understood ; and however the environment, by Natural Selection or other means, may modify the course of development, this is the fundamental background on which it has to work ; just as, according to orthodox Darwinism, the environment, by means of Natural Selection, works upon indefinite individual variation. The origin of orthogenetic trends, on the one hand, and of individual variation on the other hand, is yet to be investigated.

Prof. A. E. Trueman.

I. In the investigation of extensive series of invertebrate fossils the characters available for examination are fewer than in fossil Vertebrates, but the larger numbers of specimens make it possible to secure fuller information for statistical purposes. Hundreds of specimens of one ' species ' may frequently be obtained from a single layer, and it is suggested that, for all practical purposes, these may be regarded as constituting a ' community.' Investigations of such series appear to show that :

1. The variability of certain fossils, such as corals and sedentary molluscs, is much wider than that of some ammonites and gastropods, but that the variations are similar in character in both cases.
2. Such communities as have been investigated appear to be homogeneous groups, the extreme types being relatively scarce and the intermediate types forming the main part of the community.
3. Variation in one character appears to be relatively independent of that in another, from which it has been deduced that a ' line ' of evolution consists of a plexus of anastomosing strands rather than a bundle of parallel strands.
4. Most fossil species which have been investigated are therefore to be regarded as 'impure' species, to some e tent resembling those revealed by Mendelian analysis.

II. A critical re-examination of the recapitulation hypothesis in the light of recent ontogenetic studies, chiefly of fossil molluscs, shows that there are admittedly many features in almost every ontogeny which could not be paralleled with any stage in the phylogeny of the species. These features result from the introduction of new (cœnogenetic) characters, from the omission ('skipping') of stages which would unnecessarily prolong ontogeny, from the varied rates of acceleration of different characters (in some cases a group of characters is retained because of its suitability to the embryonic stages), and from other causes. Nevertheless, there appear to be certain features in the ontogeny of most fossil species which may reasonably be regarded as recapitulatorial.

Prof. H. H. Swinnerton.

As time is a vital factor in evolution the evidence of Palæontology is essential to the settlement of differences of opinion reflected in contending hypotheses. The fact that its material is made up mainly of fragmentary remains is more than balanced by the abundance of those remains ranging over vast periods of time. These fragments include the remains of both ancestral and descendant members of the same evolving stocks, and consequently the study of them confirms, corrects or amplifies conceptions based upon the study of embryology and comparative anatomy such as recapitulation, variation, species. Though palæontological evidence has, on the whole, kept closely in the wake of that furnished by other biological sciences, it has lagged behind in its contribution to those problems which are suggested by such experimental work as that associated with the term 'Mendelism.' Whilst the experiments must be with carefully controlled living organisms belonging to immediately succeeding generations, the individual factors are permanent possessions of the whole racial stock, and new arrangements of them may be exhibited by many generations. A large proportion of the factors are preservable in the fossil state, and consequently their behaviour in different genetic stocks is capable of investigation. Factorial features exhibit the same independence of one another when traced along different lineages as they do when followed from parent to offspring through only a few generations. The evidence in that case confirms the findings of the experimenter. On the other hand, the study of fossils seems to indicate that some factors, when followed through geological time, undergo serial change. This runs counter to existing conceptions of the nature of the factor. Even with such limited attention as this aspect of palæontology has received there are indications that its evidence will prove to be just as valuable to this as to other branches of evolutionary inquiry.

Monday, September 28.

Discussion on *Earth Movements in relation to Stratigraphy.* (Prof. O. T. Jones, F.R.S.; Prof. E. B. Bailey, F.R.S., and Dr. J. Weir; Prof. W. S. Boulton; Prof. H. A. Brouwer; Dr. G. L. Elles; Dr. R. G. S. Hudson; Mr. D. L. Linton; Dr. E. O. Ulrich; Prof. P. G. H. Boswell, F.R.S.)

Prof. O. T. Jones, F.R.S.

A discussion on this topic must necessarily cover a wide range, as it is of interest not merely to geologists, but also to geophysicists and economic geologists, particularly oil geologists. I can do little more than call the attention of investigators in these various subjects to the different aspects of the problem. There are obviously two aspects to be considered, viz. the influence of earth movements on sedimentation and of sedimentation on earth movements.

Both aspects bring us into contact with prevailing theories of the nature and behaviour of the deeper layers of the earth's crust. In particular, there has to be considered the importance or otherwise of Isostasy in its relation to sedimentation on the one hand and earth movements on the other.

It is obvious that earth movements exercise a dominant control over sedimentation, for whether the sediments be mainly clastic such as sands or muds, or non-clastic calcareous deposits, the thickness that can accumulate in any given period is limited

by the depth of the original basin unless the basin sinks during the period. The position and size of original basins of deposition have been determined, in general, by movements of the crust, and their continued existence as areas of sedimentation depends on continued depression. In fact, one may almost enunciate a principle—no depression, no sediments.

Further, in the case of clastic sediments, and possibly also in the case of some calcareous deposits, a continuing source of supply during a prolonged period depends upon upward movement of the neighbouring land area. If this ceased to move up the 'feeding-grounds of erosion' would be exhausted, so that, in general, we may say also, no uplifts, no sediments.

Some of our most fascinating deposits, *e.g.* the Jurassic rocks of this country, the playground of zonal stratigraphers, owe their interest to the fact that they were formed in a nearly filled basin subject to periodic oscillation through a small vertical range.

On the other hand, we appear to have a close relation between movements and sedimentation in those areas where in the past an enormous thickness of deposits has formed throughout under shallow water conditions. It has frequently been suggested that these areas were depressed in consequence of the weight of sediments that were being laid down on that part of the earth's crust.

Such an intimate relation between sinking of the crust and the addition of fresh load seems to imply an extreme sensitiveness of the crust, and that the maintenance of isostatic equilibrium is a rule in such cases. If this be the explanation of the relationship it appears to involve the converse possibility, that as material is removed from the area adjoining the basin of deposition the land must rise as it becomes lightened by erosion.

Dr. H. Jeffreys has shown that the accumulation of a thick layer of sediments exercises some effect upon the thermal conditions at the base of the layer, but this does not explain why sedimentation became possible in that particular area in preference to others, nor how isostatic equilibrium could have been maintained during the accumulation of the first few hundred feet of sediments.

It appears to me that the dominant control is exercised by movements of the crust. Depression of an area has begun owing to some previous condition of the crust and possibly uplift of an adjoining region was simultaneously determined. As soon as a basin was formed there appears to have been in the past abundance of material available close at hand wherewith to fill it, and the faster it was depressed the faster it filled.

We appear to have no evidence among past sediments of a basin that remained unfilled during a long period of continuous depression.

It is possible, however, that in the Pacific Deeps we have such basins. These are so situated as to be either too far for much sediment to reach them or so that there is little drainage in their direction from the nearest land areas. It is not improbable, however, that the present condition of the crust is somewhat abnormal in respect of relief and distribution of land and water and depth of continental shelves as compared with past periods.

In conclusion, I refer briefly to another relation between movements and sedimentation which is well known, having been emphasised by Dana, Mellard Reade and others many years ago, and more recently by Haug. The relationship is well illustrated by our great Palæozoic formations. Intense folding and overthrusting has affected in particular those regions of the crust where previously a great thickness of sediments had formed. It will be found in general, however, if the level of the foundation on which the sediments were laid down is compared before and after the movement, that except in very narrow areas it is never raised to the same level after one of these violent movements as it occupied before sedimentation began.

The slow, persistent movements that accompanied the deposition of the sediments were of far greater total effect than the more violent and apparently more effective folding movements that succeeded the sedimentation phase.

Prof. E. B. Bailey, F.R.S., and Dr. J. Weir.—*Submarine Faults as Boundaries of Facies.*

The North Sea coast of Sutherland furnishes a wonderful opportunity for studying the influence of Kimmeridgian faulting upon contemporaneous sedimentation. The exposures belong to the vicinity of a great fault, and illustrate the production of boulder beds, coupled with contortions, sandstone-dykes and current phenomena,

which are attributable to earthquakes and accompanying submarine landslips and tsunamis (sea-waves). The phenomena reproduce many features of the Early Palæozoic succession of the Province of Quebec, but have escaped complication by subsequent mountain-making movement.

The fault mentioned above has been traced from near Golspie, north-east by Brora and Helmsdale, a distance of 20 miles. Everywhere in this part of its course it separates pre-Mesozoic rocks (schists, granite and unconformable Middle Old Red Sandstone) on the north-west, from Mesozoic rocks (Trias and Jurassic) on the south-east. The downthrow is estimated as over 2,000 feet, for this measure is probably exceeded by the combined thickness of the Mesozoic rocks still preserved.

The pre-Kimmeridgian portion of the Mesozoic succession is quite normal for Scotland. It starts with continental Trias, succeeded by marine Jurassic, with, as elsewhere, an estuarine development of the Bathonian. The Kimmeridgian, the only important development of the group known in Scotland, is of extraordinary character. It consists of black shales and boulder beds, continually interbedded. The black shales are full of remains of land plants and often contain ammonites. They have evidently accumulated in a tranquil sea not far from the shore. The boulder beds are composed of angular and subangular fragments, boulders and masses of Middle Old Red Sandstone, generally set in a matrix of shelly sand, crowded with broken oyster, reef coral, *Rhynchonella*, etc.

Two main hypotheses have been advanced in regard to these boulder beds: the material has either been carried by ice or has been dropped from a cliff. The ice-transport hypothesis is universally abandoned at the present time, because: (1) the boulders are unstriated and the boulder beds are certainly not boulder clays; (2) floating ice would drop *boulders* rather than *boulder beds*; (3) the associated plants and animals provide unquestionable indices of a warm climate.

Thus, all observers now agree that the material has been dropped from a cliff. Some regard the cliff as a product of marine erosion, others as a product of faulting. The former view seems to us incredible because: (1) the Jurassic succession at Golspie is seen to rest on the Trias, the usual position in Britain outside of East Anglia; (2) the cliff ran *straight* for over eight and a half miles, and everywhere coincided, at any rate approximately, with a *subsequent* fault; (3) the deposit of recurrent shales and boulder beds is, in our opinion, of great thickness, at least 1,000 feet.

The marine erosion and fault hypotheses can be usefully contrasted in regard to one of the largest boulders that has been identified, the so-called 'fallen stack' of Portgower. This mass of Old Red Sandstone measures 100 by 90 by 30 feet. Its unusually great dimensions have led certain authors to *assume* that transport was out of the question. It has therefore been taken as certain that this boulder is a stack that has fallen over on its side.

The difficulties of this interpretation are: (1) the great boulder is only one item contained in an extensive 50-foot boulder bed, which latter remains unexplained on the 'fallen stack' hypothesis; (2) exposures show shales and interbedded boulder beds emerging from under this particular boulder bed. Similar relations hold everywhere for 8½ miles. The Kimmeridgian everywhere proclaims the proximity of a cliff, but in spite of constant changes of dip, its *base is never exposed*. Thus we are driven to admit that the Portgower boulder has slipped for a third of a mile along the sea bottom from the neighbouring fault-scarp.

Of prime importance also is evidence of *ephemeral currents* that accompanied almost every landslip. Such evidence is afforded by: (1) the distribution of the material as *boulder beds*; (2) the smoothing of the tops of individual boulder beds with *unrounded* rubble and shell debris; (3) many striking erosional phenomena.

As the boulder beds, with their shelly debris, continue right up to the fault, it is clear that the fault-scarp was wholly submarine and that it separated a shelly, sandy, submarine platform from a muddy, tranquil deep.

Mr. D. L. LINTON.

By means of isopachyte data plotted on a series of four maps it has been possible to demonstrate that throughout Upper Cretaceous times contemporary warping occurred in South-east England.

The tectonic features produced appear to be of two clearly defined but contrasted types. In the first type broad areas of subsidence are associated with heavy loading and are margined by areas of peripheral uplift. Such isostatic movement was an

essential feature of the Jurassic and early Cretaceous history of the region. It is displayed by the maps showing the conditions in Selbornian and again in Turonian times, but the loci of maximum sedimentation are to be found further and further east with the progress of time, strongly suggesting a systematic migration away from the source of detrital material.

In the second type, the downwarped areas are to be regarded as compression subsidences foreshadowing the London and Hampshire basins. This type is first evidenced in the Cenomanian epoch, but the compressional forces that produced it must have died away in Turonian times when a reversion to the simpler conditions typical of the Jurassic era took place. They were regathered more intensely in the Senonian epoch when the tectonics assumed a definitely Tertiary aspect. Later, as is well known, the two compression subsidences whose first beginnings are traced here, were further depressed by heavy loads of sediment.

Intimately connected with the eastward migration of the centre of maximum sedimentation and depression is the progressive tilting of Britain that has gone on ever since Cretaceous times. It results from the progressive loading of the eastern regions and is evidenced by the westward convergence of the principal Cretaceous and Tertiary datum planes, and by their *successive* intersections.

Finally, the validity of the conclusions drawn from the isopachyte data is established beyond doubt, by the ability of the method to demonstrate the existence of a series of infra-Cretaceous minor folds, whose reality is abundantly confirmed, although it could hardly have been deduced, by the palæontological records.

Dr. E. O. Ulrich.

The writer has enjoyed exceptional opportunities and devoted much time in the past 35 years to the study of stratigraphic phenomena indicating shifting of areas of marine deposition because of earth movements. In the case of structural domes the movements usually consisted of alternating elevation and subsidence and of differential tilting of their surfaces; in the case of geosynclines they originated and subsequently, with minor modifications of trend and complexity, they developed and largely maintained more or less definitely located persistent longitudinal major troughs and embayments, the bottoms of which were at times submerged, at other times elevated completely or partly above sea level; and in the case of the wide and nearly flat interior areas by differential tilting of the general surface of the continent.

As a rule, the physical evidence on which shifting of epicontinental seas is inferred consists (1) of the pinching-out of a formation so that normally overlying and underlying stratigraphic units come into contact; (2) the reduction in thickness is mainly or solely by loss from below so that the upper beds extend farthest—an impossible condition under the conception of loss by erosion prior to the deposition of the overlying formation. The organic evidence indicates alternating occupancy of the epicontinental troughs and shallow basins by waters and faunas of diverse sources and direction of invasion. Namely, the fossil remains in the concerned formations indicate more or less positively that the often directly superposed faunas originated and developed in, and at times of submergence invaded the submerged areas, in some instances from the north, in others from the east, and in the remaining cases from either the south or the west. Consequently, the organic remains in two or more widely separated formations in a continuous outcrop of, say, Ordovician deposits, are so closely similar in generic and even in specific relations, that no doubt can obtain as to their common origin and the sameness of the oceanic realm from which they invaded the continental basins. However, between these recurring invasions of, say, a northern or an Atlantic fauna, the section commonly displays other faunal zones that may either simulate each other or differ radially, but in neither case be at all like that, or those, of the first set. These intercalated faunas, therefore, must have originated in and invaded from such other oceanic sources as the Gulf of Mexico or the Pacific side of the continent.

Many such instances of shifting of seas because of earth movements in the Appalachian Valley and on the flanks of the Cincinnati and Nashville domes and on the slopes of the Adirondacks and Ozark uplifts were brought out and discussed in the writer's Revision of the Palæozoic System (1911), and others observed in Wisconsin were published in subsequent papers. On the present occasion most of

the previously published cases will be only very briefly mentioned. More time will then be devoted to fuller presentations of the facts concerning many similar conditions discovered since 1911 in the Appalachian region, the Ozark region of Missouri and Arkansas, the Arbuckle and Wichita uplifts in Oklahoma, and finally in the central area of Texas.

Dr. L. J. Spencer, F.R.S.—*Second Supplementary List of British Minerals.*

The first of these lists was presented at the Bristol meeting in 1898. They are intended to supplement the still standard work of Greg and Lettsom's 'Mineralogy of Great Britain and Ireland' (1858). The present list includes 90 names, of which 46 (printed in italics) may be regarded as well-defined species with satisfactory records. This brings the total number of mineral-species that have been found in the British Isles up to 339, representing about one-third of the number of known species.

Aegirine-augite (Tyrrell, 1909).
Aenigmatite (Heddle, 1901).
Afwillite (Tilley, 1930).
Altaite (Des Cloizeaux, 1893).
Anorthoclase (Heddle, 1901).
Arfvedsonite (Seymour, 1900).
Barkevikite (Busz, 1900).
Bassetite (Hallimond, 1915).
Bertrandite (Bowman, 1911).
Botryolite (McLintock, 1910).
Calciopalygorskite (Fersman, 1908).
Carminite (Russell, 1910).
Cebollite (Tilley, 1931).
Celsian (Russell, 1911).
Chamosite (Rudler, 1905).
Chloanthite (Russell, 1922).
Chloroxiphite (Spencer, 1923).
Chonicrite (Heddle, 1901).
Christophite (Collins, 1879).
Claudetite (Hintze, 1904).
Clinozoisite (Thomson, 1908).
Collieite (Brown, 1927).
Cotunnite (Russell, 1920).
Crednerite (Spencer, 1923).
Crocalite (Heddle, 1901).
Crocoite (Brown, 1927).
Cummingtonite (Teall, 1907).
Dechenite (?) (Heddle, 1901).
Diabantite (Garnett, 1923).
Diaboleite (Spencer, 1923).
Diamond (?) (Heddle, 1901).
Diaspore (Tilley, 1927).
Diatomite (Macadam, 1884).
Dopplerite (Moss, 1903).
Dumortierite (Mackie, 1924).
Dundasite (Prior, 1906).
Embolite (Prior & Spencer, 1902).
Epichlorite (Garnett, 1923).
Ettringite (Tilley, 1931).
Forsterite (Clough & Pollard, 1899).
Fuchsite (Heddle, 1901).
Geocronite (Prior, 1902).
Gibbsite (British Museum).
Gilpinite (Larsen & Brown, 1917).
Glockerite (Dana, 1899).
Hastingsite (Tilley, 1931).
Hercynite (Teall, 1897).
Hitchcockite (Miers & Hartley, 1900).
Hydronephelite (Tilley, 1931).
Keilhauite (?) (Heddle, 1901).
Kilmacooite (Tichborne, 1885).
Larnite (Tilley, 1929).
Laurionite (Russell & Hutchinson, 1922).
Lepidocrocite (Heddle, 1901).
Magnesite (Heddle, 1901).
Magnesium-diopside (Rosenbusch, 1905).
Marmatite (Rudler, 1905).
Melilite (Flett, 1900).
Merwinite (Tilley, 1927).
Meta-torbernite (Hallimond, 1920).
Morenosite (Heddle, 1901).
Mullite (Bowen, Greig, & Zies, 1924).
Nadorite (Russell, 1923).
Nigrine (Reynolds, 1871).
Orpiment (Gages, 1860).
Paralaurionite (Russell & Hutchinson, 1922).
Passyite (Rudler, 1905).
Perovskite (Elsden, 1904).
Petalite (McLintock, 1923).
Phenakite (Russell, 1911).
Pimelite (?) (Heddle, 1901).
Prochlorite (Heddle, 1892).
Ptilolite (Bøggild, 1922).
Rammelsbergite (?) (Heddle, 1901).
Scawtite (Tilley, 1929).
Semseyite (Smith, 1919).
Serpierite (Russell, 1927).
Sideroplesite (Heddle, 1901).
Spurrite (Tilley, 1929).
Stilpnomelane (Hallimond, 1924).
Stokesite (Hutchinson, 1899).
Titanaugite (Tyrrell, 1909).
Tschermigite (Shand, 1910).
Ullmannite (Spencer, 1910).
Uranospathite (Hallimond, 1915).
Urpethite (Dana, 1868).
Wurtzite (Noelting, 1887).
Yttrotantalite (?) (Heddle, 1901).
Xanthochroite (Rogers, 1917).
Xenotime (Gilligan, 1920).

Dr. J. W. Jackson and Dr. A. Bulleid.—*The Occurrence of Corbicula fluminalis in the West of England.*

Recent excavations have exposed considerable sections of shelly sands and gravels in the vicinity of Middlezoy, Somerset. These beds, known as the Burtle Beds, contain a large series of common and widely-distributed marine shells, together with the bones and teeth of mammals. In the course of our researches we have had the good fortune to find a few examples of *Corbicula fluminalis* in these beds associated with the marine fauna. The latter is of a mixed facies, consisting of forms common to sandy, muddy and rocky habitats at the present day. The remains of the mammals are rather fragmentary, but we have been able to identify the following forms :—*Elephas* sp. (fragments of tusks), *Rhinoceros* sp., Bison or Urus, and Fallow Deer (strongly suggesting *Cervus browni*).

The occurrence of *Corbicula fluminalis* is interesting as being the first record for the West of England. It has been recorded from West Wittering, Sussex, along with similar marine shells to those obtained by us, and from the 80–90 ft. terrace of the Warwickshire Avon, at Ailstone, near Stratford. These are the two nearest sites to the Somerset locality. The species is known from the Red Crag and subsequent deposits up to the Middle Pleistocene at Crayford, where it makes its last appearance. It has been found in some abundance in the terraces of the Thames and Cam, and in the Oxford gravels ; also in the interglacial gravels at Kelsey Hill, Holderness, associated with numerous marine shells. In France it has been recorded from the estuarine bed, 24 feet above mean sea-level, at Menchecourt, where it occurred with seven species of marine shells identical with those found by us in Somerset.

There is no evidence to suggest that the Somerset beds with *Corbicula fluminalis* are later than Crayford, which is regarded as Early Mousterian from its contained implements.

The Burtle Beds have been assigned to two positions later than the Raised Beaches. One suggestion is that they were possibly formed during an early pause in the elevation of the beaches ; the other that they were possibly formed during a pause at the termination of the subsiding movement which followed the elevation. The beds appear to us to be distinctly related to the Raised Beaches of the neighbouring coast, seen near Weston-super-Mare, being, in fact, the littoral sands deposited during the subsidence which closed with the formation of the beaches. During later elevation the Burtle Beds were ravined and their tops denuded, and on later subsidence the hollows were filled and the surface covered with alluvial and lacustrine deposits.

The Raised Beaches, as is well-known, are overlain by aeolian sand, and that again by Head, the latter being regarded as representing the Magdalenian cold period. This suggests that the Raised Beaches may be Mousterian in age, as in the case of the Crayford Terrace of the Thames. It is interesting to note that *Cervus browni* is recorded with *Corbicula* from Clacton, which is slightly earlier than Crayford.

Mr. F. W. Anderson.—*Phasal Deposition in the Middle Purbeck Beds of Dorset.*

Many geologists have worked on the Purbeck strata since Webster's account of these beds in 1816, and a large number of fossils have been collected by the various workers. The vertebrate fauna has been, for the most part, carefully described and figured, but the Mollusca are almost entirely undescribed. Further, owing to the nature of the deposits, lake and lagoon, rapid lateral change is the rule, and therefore, on lithological grounds also, only a very broad correlation has hitherto been possible.

A study of the Ostracoda, however, combined with a microscopic examination of the beds in which the various forms occur, has demonstrated the existence of depositional phases, which being ultimately due to tectonic re-adjustment, should be of sufficiently wide application to allow of their recognition in other areas.

Three phases may be recognised in the lower half of the Middle Purbeck. The beginning of each phase is marked by fresh-water deposits, grey marls and shell limestones containing *Paludina, Unio, Cyrena* and fresh-water Ostracods. Throughout the phase there was a gradual shallowing of the water and increase in salinity ; towards the end deposition decreased and cherty limestones (shell breccias) are the typical deposit. *Limnaea, Planorbis* and *Corbula* are characteristic of this brackish water stage.

The increase in salinity had a marked influence on the Ostracod fauna. The fresh-water deposits at the commencement of each phase contain *Darwinula leguminella*

(Forbes) and *Cyprione bristovii* (Jones), these are followed by an early form of *Cypridea dunkeri* (Jones) and *Cypridea punctata* (Forbes), which are later in the phase replaced by *Cypridea granulosa* (Sow.), these latter becoming more abundant as the water became more brackish.

The early form of *C. granulosa* had apparently been evolving elsewhere during the long fresh-water period which initiated the third phase. The form which appeared in the middle of this phase being a new variety, *C. granulosa* var. *paucigranulata* (Jones), which, as the water became more saline, gradually changed into the form *C. granulosa* var. *fasciculata* (Forbes).

The third phase was brought to an end by a marine invasion and the deposition of great banks of *Ostrea distorta* (the ' Cinder Bed ').

Tuesday, September 29.

DISCUSSION on *The Genesis of Ores in relation to Petrographic Processes.* (Prof. C. G. CULLIS ; Dr. A. BRAMMALL ; Mr. K. C. DUNHAM ; Prof. P. NIGGLI, Dr. W. R. JONES.)

Prof. C. GILBERT CULLIS.

The origin of ores has long been a subject of speculation. The terms ' neptunist,' ' vulcanist,' ' descensionist,' ' ascensionist ' and ' lateral secretionist '—connoting deposition from an ocean, by igneous injection, or from solutions travelling downwards, upwards or laterally—recall early genetic hypotheses. These theories, though partly valid, were parochial in character because based upon restricted observation. With fuller knowledge of rocks and rock formation broader views as to ores and ore formation have been evolved.

Ore-deposits are local concentrations in which valuable metals have been segregated so as to be present in far larger proportions than in average rocks. The genesis of such bodies implies the geological process or processes by which concentration has been effected.

These concentrations, being naturally-formed mineral aggregates, cannot logically be distinguished from rocks. They are highly specialised rocks, however, as is indicated by their mineral composition and rareness, and by their small dimensions as compared with ordinary rock masses. Regarded in this light they may be genetically referred to the three great modes of rock formation and classified as igneous, sedimentary and metamorphic.

Deposits of igneous origin exceed all others in importance ; indeed, the study of the genesis of ores is in the main the study of the processes of concentration in, or in association with, igneous rocks.

Whether the metals were inherent to the igneous material or whether introduced from a deep-seated ore-horizon, their segregation into ore-bodies seems to have been due to the extreme action of magmatic differentiation.

During this process—by which an original magma separates into sub-magmas—certain metals, *e.g.* platinum, chromium, titanium, iron, nickel, copper, &c., migrate with the basic fraction, and under favourable conditions are concentrated by gravity in magmatic ore-bodies at or near the floors of gabbroid intrusions. Others, *e.g.* tin, tungsten, molybdenum, bismuth, iron, copper, gold, zinc, lead, silver, antimony, mercury, &c., migrating to the acid fraction, form fugitive compounds with water and other volatiles and undergo anti-gravity concentration at or near the roofs of granitic cupolas, as pegmatitic, pneumatolytic, or contact deposits, or pass in hydrothermal solutions up into the surrounding country-rock and are deposited, in a well defined temperature sequence, as cavity-fillings or replacements at various distances from the parent igneous rock.

It is these hydrothermal solutions that have formed most mineral veins, and since filling has been effected by magmatic emanations such veins may reasonably be regarded as igneous intrusions.

Ore-deposits of sedimentary origin are merely special cases, either of detrital rocks resulting from the disintegration of pre-existing ore-bearing formations, or of chemical sediments precipitated from surface waters. In the former type there has been gravity concentration of certain economic minerals, *e.g.* platinum, gold, tinstone, &c.,

in alluvial deposits. In the latter, valuable saline residues have arisen by the desiccation of salt-lakes and inland seas; or, alternatively, bedded ores of iron and manganese, base-metal sulphides, &c., have resulted from chemical reactions set up in lacustrine or marine waters by the metabolism or decomposition of plants and animals.

The surface waters with which sedimentary rocks are connected, by descending, may yield fillings and replacements underground, but it is probable that ore-deposits due to the descent of supergene waters are insignificant in comparison with those formed by ascending hypogene waters. By the differential leaching of rocks and mineral veins at outcrop, however, meteoric waters give rise to important residual blankets of lateritic ores and to the zones of secondary enrichment in metalliferous lodes, this probably being their principal ore-forming activity beneath the surface. Ascending magmatic waters are ore-makers; descending meteoric waters ore-modifiers.

Deposits of metamorphic origin are relatively unimportant. Metamorphism rarely initiates mineralisation; it more often changes earlier ores or protores by heat, pressure, &c. Most contact deposits have been due to thermal metasomatism by metalliferous emanations from igneous intrusions; strictly speaking, they are not metamorphic but igneo-genetic. Pyrometamorphism may occasionally operate alone, however, as in the dehydration of fossil laterite and its conversion to emery. Dynamometamorphic deposits may be exemplified by pre-Cambrian iron and manganese ores, the mineral composition and physical condition of which have been economically bettered by recrystallisation.

From the foregoing considerations it may be inferred that the place of ore-formation in the petrogenic scheme is now realised in general. There is, notwithstanding, a remarkable lack of agreement as to the genesis of individual deposits or fields, and it is a significant fact that for practically no mining field of any consequence has agreement been reached as to the detailed genetic history of its mineralisation. The gold ores of the Rand, the silver ores of Cobalt, the nickel ores of Sudbury, the pyritic ores of Huelva, the copper ores of Northern Rhodesia, the lead ores of Missouri, the zinc ores of New Jersey, the magnetic iron ores of Lapland, and the West Coast hematites of our own country may be cited, at random, as well-known controversial cases. Clearly there is still need for research and occasion for discussion.

Mr. K. C. Dunham.

The Ore Deposits of the Upper South Tyne, Wear and Tees area of the Pennines consist of Galena and Sphalerite-bearing fissure veins which have been worked mainly in the Yoredale series of alternating limestones, sandstones and shales. The gangue minerals include fluorspar and barytes, together with quartz and carbonate-minerals. The assemblage of minerals is one that is known in many districts both at home and abroad; a discussion of the criteria bearing upon the origin of the deposits may therefore have a more than local application.

The theories of origin fall under two heads—Supergene and Hypogene; the rival possibilities include processes either of concentration from the sedimentary rocks of the Pennines, or of injection from a deep-seated magmatic source.

The direction of movement of the ore-bearing solutions has been definitely established from observations upon inclusions in the veins; for example, clear cases are known in which shale has been carried upwards in the vein by the solution. The fact that the veins pass through soft shale beds of considerable thickness suggests, further, that the solutions were ascending under pressure.

Derivation from the country-rock could only have taken place if there were an artesian circulation to ensure the ascension of the solutions. Comparison with the Joplin region of North America reveals many similarities; for example, the monoclinal structure, so essential to artesian circulation; but the lack of any direct evidence of such a circulation, and the presence of great numbers of shale beds militate against hopes of proving a supergene theory. Further evidence to the contrary is provided by mineralogical differences (*e.g.*, fluorspar abundance in Pennines). Observations in mines show that only minerals such as calcite and limonite, together with oxidation minerals, are actually in process of being deposited from the circulating waters, even in places where large quantities of pyrite would cause precipitation as sulphides.

The evidence in favour of hypogene origin may now be reviewed. A definite zonal distribution of materials has been established on a large scale—a fact greatly in favour of magmatic origin from foci. The presence of the Whin Sill, of slightly earlier date than the veins, indicates the existence, in late Carboniferous and early

Permian times, of magmatic conditions under the Pennines, which may have been directly or indirectly responsible for the localisation of the ore deposits. The presence of fluorspar in great quantity, suggests, by analogy with Cornwall, derivation from an acid magma; and indeed, the whole concentrated assemblage of rare elements supports this conclusion.

The chief line of evidence against hypogene origin lies in the impoverishment of the veins in depth, which is without doubt a real fact. It is clear, however, that the veins do not disappear, and it is suggested that local concentration is due to interaction between hypogene solutions and ground waters.

Prof. P. Niggli.

I. The scope of the term '*Useful mineral deposits*' is a *variable function depending on world economics and world industry*. Thus, it did not come to include nickel-bearing deposits until the nineteenth century. The necessary minimum tenor for exploitation varies in very wide limits from element to element and may, even among the ordinary metals, be a million times greater in one case than another. A comprehensive term such as '*mineral deposits*' should, therefore, never be used in the exclusive sense of workable mineral aggregates, for the separation of these from the rest must necessarily seem arbitrary from the scientific point of view.

II. In all new *classifications* of ore deposits attention is paid to questions of *genesis* as being the most important for the prospector and practical investigator. So long as the anthropological standpoint which distinguishes between economic and non-economic deposits is retained, it seems as futile to expect a solution of the genetic problems attaching to mineral associations as it would be to hope to deduce a general theory of evolution in zoology from the study of, say, noxious insects alone.

III. *The chief processes of mineral formation produce mineral deposits of rock-character.* The special deposits mined for certain elements and minerals are generally by-products of rock-forming or rock-altering processes. The technically important parageneses of elements or minerals appearing on these deposits must be accounted for by *general* geochemical laws. Rock-forming processes and those leading to the formation of local accumulations or concentrations of useful elements can only be understood in their full significance if the natural mineral associations are regarded as an entirety, if rock deposits and mineral deposits be accepted as parts of a whole. From the practical point of view this is important, as observations on rocks or valueless mineral occurrences may indicate the whereabouts of workable deposits.

IV. Connections can always be established between economic deposits and one or more of the following major *processes of rock-formation.*

A. Processes of magmatic differentiation and consolidation.

With these are connected concentrations of matter:

(*a*) during the chief period of consolidation of igneous rocks: *Orthomagmatic.*

(*b*) under the influence of the gradual concentration of volatile constituents at high temperature: *Pegmatitic-Pneumatolytic.*

(*c*) in aqueous residual solutions, exhalations, thermal waters, etc.: *Hydrothermal.*

B. Processes of sedimentation (resulting from the interaction of the rock mantle with the atmosphere and hydrosphere).

With these are connected concentrations of matter:

(*a*) in solutions arising from weathering processes and in the products of precipitation of these;

(*b*) in the residues arising from weathering processes and in the products of their transportation.

C. Processes of metamorphism (resulting from the influence of external factors excluding the atmosphere and hydrosphere, on pre-existing mineral associations).

In connection with these, matter may undergo:

(*a*) relative concentration through loss and differentiated migration of certain substances;

(*b*) concentration by lateral secretion during geological processes which affect the country rocks. Crystallisations from descending solutions in the epi-zone.

The general lines of classification of mineral deposits follow naturally from these connections, as do the chief subdivisions to be distinguished in each group.

It is, however, evident *that any given deposit may be associated with more than one major genetic process.* Thus, processes of the magmatic cycle may be responsible for the primary concentration and metamorphism for subsequent alterations, while processes in the zone of weathering (cementation) have produced the features which make the occurrence workable. The same applies to rocks also, and in no way makes classification impossible. The process which has given the main mass its characteristic habit will also determine its position in the classification.

The *genetic description of a mineral deposit* must take three points into consideration :

1. The process which produced *in loco* the concentration of elements which are characteristic for the mineral deposit and may determine its economic value. This is the primary geochemical concentration process.

2. The character of the mineral paragenesis to which this primary geochemical process gives rise.

3. Subsequent alterations within the geological body which may be accompanied by the appearance of newly-formed minerals and secondary displacements in concentration.

a. Alterations during the chief process of formation, inseparable from the latter and belonging to the same major geological process (successions, crystallisation-sequences, type-transitions, auto-metamorphic formations, reaction series, etc.).

b. Specifically later and secondary mineral alterations, migrations of matter and changes in fabric in which the atmosphere takes no part and which are independent of weathering processes at the surface. This is metamorphism (*sensu strictu*).

c. Alterations, migrations of matter, new mineral formation in connection with weathering processes (in the zones of oxydation and cementation of ore deposits, etc.).

By investigating the questions arising from 1 an attempt is made to determine the primary geochemical type of the deposit. A definite conclusion is not always possible. Coal, for instance, represents a concentration of carbon due to enrichment of organic matter at the surface, but later reactions and changes after covering-in had taken place resulted in a loss of oxygen, *i.e.*, a further concentration of carbon. Garnierite veins are another example of concentration in two stages. If the mineral composition is chiefly determined by 2 and 3*a*, the mineralogical type to which the deposit may be counted coincides with the primary geochemical type. If the typical character of the deposits is mainly defined by 3*b* or 3*c*, the present significance of the deposit must be considered a result of secondary processes. The primary concentration of matter and the present features are due to separate processes.

V. A considerable measure of agreement has been reached as regards the importance of the collective treatment of the ore deposits belonging to groups *A* and *B*, and the various systems of classification proposed differ from one another chiefly in more or less insignificant questions of detail. It is, therefore, proposed to deal here with *metamorphic ore deposits* and to shew that, once again, a full understanding of the phenomena involved requires the application of the results obtained by the investigation of rocks and, in this case, of rock-metamorphism.

The considerations which point to the existence of metamorphic ore deposits are quite similar to those which lead to the separation of the metamorphic rocks as an independent class. It has long been known that certain ore-occurrences shew exceptional mineralogical and structural developments which make classification difficult and seem to contradict the derivation from purely magmatic or sedimentary processes. *A profusion of rare minerals and such which are peculiar to the localities is often a typical feature of such deposits.* Three examples may be quoted to support this statement.

Each year brings a series of new minerals from the Langban mine. In 1923 Flink quoted over 200 species, of which rare manganese silicates, lead silicates, lead arsenates and magnesium, magnesium-manganese, calcium-magnesium-beryllium silicates are examples. It can hardly be doubted that many of these minerals were formed during a period of metamorphism and originated from solutions of unusual composition, subjected to physical conditions not usually encountered by ore solutions. Nils H. Magnussen supposes that a deposit of iron and manganese ore was first subjected to contact metamorphism which produced a body termed by him a reaction skarn (*i.e.*, a skarn originating from already existing material through thermometamorphism). In a later period folding took place in the area, which was subjected to a slight dislocation metamorphism (sköl-forming process) accompanied by exchange of matter between leptite layers and the ore and skarn masses. Fissures were opened, some

sulphide-material introduced, and, as a result, vug and fissure minerals of remarkable composition formed. Minerals of at least three periods, though only some as relicts, are, therefore, present on this deposit.

Another occurrence shewing a great variety of minerals is the zinc-manganese ore deposit of Franklin Furnace, New Jersey. About 100 different minerals have been quoted, including franklinite, willemite, zinkite and a great number of rarer species such as tephroite, schefferite, gahnite, polydelphite, etc. Perhaps sphalerite, pyrite and rhodochrosite are the original minerals which, after a phase of oxydation, underwent a metamorphic process, possibly due to pegmatites.

Of exceptional complexity, again, is the sulphosalt deposit of Lengenbach (Binnental). A series of sulphosalts, including Hutchinsonite, Binnite, Seligmannite, Skleroklas, Lengenbachnite, Rathite, were first described from this locality. There is no doubt that alpine dislocation-metamorphism of the pennine type is responsible for the formation of the sulphosalts. The original deposit was of a perfectly normal type.

In view of the fact that a large number of rock-forming minerals such as grammatite, actinolite, vesuvianite, grossularite, sillimanite, cyanite, staurolite, chlorite, serpentine, talc, epidote, zoisite, sericite, etc., etc., are practically restricted to metamorphic rocks, it is not surprising that the number of minerals peculiar to metamorphic ore deposits should be considerable, even though the occurrences are comparatively rare.

Some deposits which shew no great particularities as regards mineral composition have, nevertheless, been *variously interpreted in respect to questions of genesis.* No doubt primary traits have in many cases been obliterated by redistribution of matter.

An example is offered by the Witwatersrand gold deposits, which in the opinion of the writer, are undoubtedly sedimentary placer deposits which have been subjected to epi-metamorphism accompanied by migration of matter. The vein-like development of the ore recalls the torsion-fissures of alpine type, and is not a result of magmatic action. Metamorphism has converted siderite and greenalite rocks of the Marquette and the east Mesabi district to magnetite-grünerite schists and to specularite deposits. Some fahlband deposits have been recrystallised during a period of rock metamorphism. According to Schneiderhöhn the peculiarities of the copper area of Northern Rhodesia may be ascribed to metamorphism. In such areas it is particularly interesting to reconstruct the processes of migration carried out by the solutions as they offer the best insight into the exchanges and replacements of material which take place during periods of rock metamorphism.

From the standpoint of *structures* and *textures*, exceptionally interesting conditions are found in the veins of the ore deposits which accompany the quartz, adularia zeolite veins of alpine type and have been subjected to dislocation metamorphism. Swiss occurrences have recently been described by Koenigsberger, Friedländer and Huttenlocher, those of Rammelsberg and Meggen (Germany) by Frebold and Ramdohr, and examples from the United States and British Columbia by Emmons, Lindgren, Uglow, etc. Many such deposits are to be found in Scandinavia, and possibly the pyrite deposits of Rio Tinto and other similar occurrences should be included in this type. In some cases the mode of formation was sedimentary, in others of the combined sedimentary-volcanic type. In certain instances, again, a distinctly hydrothermal veinlike development can be observed. Dislocation metamorphism has produced similarly-directed alterations at the various localities so that the resulting products converge to form a new type.

All the fundamental phenomena of rock metamorphism as, for example, clastic, blastic and plastic changes, *mechanical deformation*, *recrystallisations*, etc., must be taken into account in order satisfactorily to explain the details of such deposits. Of technical importance is the tendency to form compact, very finely-granular *granoblastic or diablastic aggregates*—such as ' Bleischweif ' and the so-called ' Misch-' and ' Meliererze.' The fine-grained intergrowth of the different minerals causes difficulties in the metallurgical treatment of the ore. Banded layers of galena or such which shew the effects of gneissose lamination have suffered mainly plastic deformation accompanied by the formation of strain centres and the bending of glide lamellae. Recrystallisation has converted such material into the compact diablastic or granoblastic ore. The process of recrystallisation has been aided, as in all natural mineral aggregates, by solution, although in the case of relatively soft metallic ores, recrystallisation without the aid of solvents also occurs. Whereas *plastic deformation* is not very frequent with silicates, certain ore minerals such as galena, and, to a certain extent, zincblende, shew properties approaching those of the most

deformable metals. Pyrite, on the other hand, fractures under similar conditions. that is to say, it suffers elastic deformation. With minerals representative of the whole range of mechanical properties available for observation, it becomes increasingly evident how highly selective the action of dislocation metamorphism is. It will be necessary to investigate the variation among ore minerals of the susceptibility to plastic deformation and to work out the sequence of their tendency to crystalloblastic regeneration. Under the influence of differential movements ore solutions and plastic ore masses may migrate along fractures and fissures, but their tendency to do so is different, with the result that selective impregnations may result. Fragments of the country rock may be folded into the plastic ore masses if the stress be sufficiently strong, veins may be drawn out to lenses, etc. The resulting ore bodies are often less to be described as veins or beds than as mineralized zones with irregular distribution of the ore. Thin and polished sections of the ore often present highly interesting aspects of the *replacement*, *unmixing*, *folding* and other processes which have taken place. In the deformed deposits of the alps, *albite* is frequently a characteristic newly-formed mineral, zincblende is usually poor in iron and light yellow in colour and, although the metamorphism took place at relatively low temperatures, magnetite and pyrrhotite are often present. A number of other typical features could be quoted for these deformed ore deposits of which the type-characteristics will have to be worked out by comparative study. It may already be said that nothing but a *thorough knowledge of the mechanical, physical and chemical fundamentals of rock metamorphism* will lead to a satisfactory understanding of these deposits and the great practical difficulties they often raise.

VI. The natural outcome of a treatment of mineral deposits which draws no sharp line between rock formation and the formation of local (accessory) mineral deposits of economic or non-economic character is the conception of *rock* and *mineral provinces*. In this connection it is necessary to work out the relations between the various mineralizing processes in any given geological unit and to compare the results obtained for different processes in such natural units. An insight is thus obtained into the causality existing between geological and geographical factors and world economics.

It may be recalled that in the circum-pacific area the mesoid major folding was accompanied by intensive magmatic movement, and consequently very extended ore mineralization. (North America, Central and South America, Japan, are all important ore areas.) In the alpine area, which witnessed the superimposition of two continents, magmatic solutions generally found their upward way barred. (The central alps are poor in young ore deposits.) Similarly, the extent and importance of sedimentary ore deposits and such connected with weathering, can only be fully estimated by attempts at reconstruction of the distribution of land and water and of the climatic factors, etc., which obtained at the time of formation.

The reconstruction of the geological history of a given part of the earth's crust with reference to the processes of mineral formation (including rock formation) which accompany every major geological act, enables a prediction to be made of the economic deposits which are possible and may be found in that part of the crust. Earth history, which devotes equal care to all types of mineral associations, is not merely a science which treats of the past. It is no less the starting-point for the estimation of the world's reserves of raw materials and for the framing of future economic policy.

Dr. A. Brammall.

Magmatic ore-deposits are as closely the concern of the petrologist as are peridotites and greisens, anorthosites and aplites: Prof. P. Niggli has shown that their problems are inseparable from those concerned with magmatic differentiation.

But discussion of these problems hitherto has been based largely on a threefold assumption: that (*a*) differentiation is the main, if not the sole, process concerned in the evolution of igneous rocks; (*b*) the primary magma is gabbroid or basaltic; and (*c*) all other magmas are differentiates from this basic magma, the normal rock-sequence being peridotites, perknites, &c. → gabbros → diorites → granodiorites → granites or their extrusive phases. The conception is thus essentially restricted to that of a basic magma worked upon by an evolutionary process, *i.e.* to the initial raw material and the machine.

Admittedly the machine is adjustable to tectonic and other factors at any stage of the process, and the raw material is not precisely standardised. But one is entitled to doubt whether this process alone, unaided by either assimilation or palingenesis,

is competent to produce granite in bulk commensurate with the West of England complexes and others enormously larger elsewhere. Moreover, basaltic parentage does not lead us to expect that a granitic end-product is likely to be specially enriched in tin, tungsten, and tourmaline. But the Hercynian (dominantly alkalic) granites of the West of England are so enriched; whereas the Caledonian granites (calc. alkali) are essentially barren. To explain these contrasts we are at liberty to assume either (*a*) contrasts in the composition of the initial basalt-magma, or (*b*) differences in the tectonic or other controls affecting the trend of differentiation, or (*c*) some combination of both factors. Any one of these explanations may be quite valid, but the scanty *ad hoc* data available warrant neither assertion nor denial: the problem is essentially untouched.

But differentiation alone, working on presumably fixed quantities of initial basic magma, does not cover the whole case so far as the petrologist is concerned. The problem engages both assimilation and palingenesis, the effects of which may serve to explain some of the many departures from the general rules associating magmatic ore-deposits with igneous rock-types.

Both assimilation and palingenesis are accorded due recognition by Petrology as a Science; but not all petrologists are willing to acknowledge the quantitative importance of either process. On the other hand, Prof. J. J. Sederholm attributes some of the Archæan granitic massifs of Fennoscandia to palingenesis, and Von Bubnoff believes that the Hercynian granites and diorites of the Schwartzwald and Odenwald originated by the regional fusion of the bases of ancient gneisses. In a personal communication to the writer, Prof. A. Holmes has suggested that the Hercynian granites, from the West of England to Spain, originated by palingenesis in the roots of deep-sunk orogenic folds. In the mass of data concerning the West of England granites in particular there is nothing inconsistent with this view; on the contrary, there is much to support it.

At any stage in the process of differentiation, substantial additions can be made to the material undergoing differentiation: acid magma, whether evolved or regenerated, may be basified, and basic magma, whether primary or derivative, may be acidified. The added material may be igneous, sedimentary, or metamorphic, and in each case the introduction of ore-metals and fluxes into the magma is a possibility. The bearing of this fact on the problem of magmatic ores is obvious.

The basification of acid magma is convincingly demonstrated by occurrences recently described by Drs. A. K. Wells and S. W. Wooldridge (1931: Ronez, Jersey), Dr. H. H. Thomas and Mr. W. Campbell Smith (1931: Trégastel-Ploumanac'h, Côtes du Nord), Mr. S. R. Nockolds (1931: Dhoon, Isle of Man), and Dr. D. R. Grantham (1928: Shap, Westmorland). Earlier proofs include the classic cases described by Prof. W. J. Sollas (1894: Barnavave Hill, Carlingford), Dr. A. Harker (1895: Carrock Fell, Cumberland), Prof. A. Lacroix (1898–1900: Quérigut, &c., Pyrenees), Messrs. F. D. Adams and A. E. Barlow (1910: Haliburton-Bancroft district, Ontario), and Prof. S. J. Shand (1921: Sekukuniland, Transvaal). The acidification of basic magma has been described by Dr. H. H. Thomas (1922, Mull), Prof. H. H. Read (1923: Aberdeenshire), Messrs. A. L. Hall and A. L. du Toit (1923: Bushveld), &c. Both these lists could be considerably extended. Hybridisation processes receive extended treatment in Prof. Daly's well-known work (1914), and representative cases are critically reviewed by Prof. Shand ('Eruptive Rocks,' 1927).

The hybrid character of the main biotite-granites of Dartmoor is demonstrable from field evidence, petrological fact, and analytical data alike. The composition of each facies satisfies the generalised equation:—

Biotite-granite $=m$ per cent. peraluminous acid magma $+n$ per cent. assimilated basic igneous rock approximating to diabase.

In this equation the value of n ranges from 2·5 to not more than 16·3; the 'peraluminous' character of the 'acid magma' provides, in part, for the felsic constituents derived from assimilated shales. Variation, which engages the silica-range 55 per cent.–75 per cent., recapitulates the normal phenomena commonly attributed to differentiation alone, the closing phases being characterised by tourmalinisation, greisenisation, kaolinisation, and the formation of lodes. But this variation is a bi-generic feature, differentiation and assimilation having proceeded simultaneously until low temperature and high viscosity put an end to both.

BaO-content affords a delicate check on conclusions drawn concerning both the fact and the quantitative importance of shale-assimilation :—

(*a*) In the granites, and in the granitised basic xenoliths (which range from small nodules to rafts) the BaO-content varies from ' nil ' to 0·02 per cent. In the majority of cases it is either ' nil ' or ' a trace.'

(*b*) But in the aureole shales, from various localities, the BaO-content is 0·05 and 0·07 per cent. (for two Leusdon shales) ; 0·02 per cent. and 0·06 per cent. (for two Yarner shales) ; 0·03 per cent.–0·11 per cent. av. ; 0·07 per cent. in a serial suite of nine (Burrator contact). In four shale-xenoliths it is 0·03 per cent.–0·08 per cent.

(*c*) The Burrator marginal granite, which has incorporated about 30 per cent. of shale-substance derived from the contact-shales, contains 0·05 per cent. BaO ; whereas the Burrator quarry-granite remote from the contact contains only ' a trace ' of BaO. In the porphyritic felspars, to which shales have made substantial contributions, BaO amounts to 0·05 per cent.–0·08 per cent. In a cordierite-rich biotite-granite (Sweltor quarry), the BaO-content attains the exceptionally high figure of 0·24 per cent.

Quantitative spectroscopic analyses afford an equally delicate check on the conclusion that a large proportion of the biotite in these granites has incorporated magnesia, iron-oxides and titania derived from assimilated pyroxene-bearing basic igneous rocks. Chromium, nickel, cobalt, silver, strontium, lead, and even iridium are traceable to hornblendic (altered) basic xenoliths.

Assays for gold and silver give the following results—expressed in grains per long ton :—

Tor-granites (av. of four) : 1·2 Au and 21·3 Ag ; quarry-granites (av. of seven) : 2·6 Au and 46·2 Ag ; an amphibole-bearing pegmatitic hybrid (av. of eleven samples) : 64·8 Au and 117·9 Ag.

At one period (1924–26) the writer was inclined to regard these metals as primary. This conclusion became increasingly doubtful as the evidence for assimilation was strengthened ; values are highest in the pegmatite described above.

A check assay of biotite separated from a quarry-granite yielded 1·5 grains Au and 108 grains Ag per long ton ; still higher Ag-values are found in the biotite arising by reaction from amphibole in the basic xenoliths.

Reverting to the hypothesis that the Hercynian granite-magmas originated by the fusion of deep-sunk fold-roots :—

At a depth of 10 kms. the temperature is normally about 300° C. For the roots of a moderate fold Prof. Holmes estimates a depth-locus of about 20 kms., where the temperature ultimately attained would be about 1200° C.—either below or above this mean figure according as the original material of the fold-roots was less, or more, radio-active than the material forming the overlying 10 kms.

But long before the material forming the fold-roots could be raised to a temperature of this order, selective (*i.e.* differential) fusion of sialic material would be inevitable. Moreover, both the estimated depth and the temperature of initial liquefaction would be reduced in proportion to the amount of water co-operating in the fusion.

If the initial magma originated as a ' dry ' melt, its composition would approximate to that of quartz—alk. felspar eutectic ; on the other hand, a ' wet ' melt would acquire excess silica together with alkali-silicates, basic solutes, fluxes (from fluoriferous micas, apatite, tourmaline, &c.) and possibly also heavy-ore metals present (as traces or in more significant amounts) in the rocks undergoing fusion.

But heat adequate for the fusion of the fold-roots could be carried directly to the sial by an initial uprise of basaltic magma, and this case entails the possible co-existence at some stage of two magmas, the one being acid and of palingenetic origin, the other being basic and primary ; the two could of course co-mingle.

The source of the ores, &c., characterising the Hercynian granites presents a highly intricate problem, to which palæogeography and a geochemical study of sedimentaries appear to be specially applicable.

Dr. W. R. Jones.

Mining geologists welcome heartily the co-operation of petrologists in the study of ore-deposits, particularly of those types of deposits which are so closely related to igneous rocks that knowledge of the formation-processes of these rocks leads to a better understanding of the mineralization ; the two processes are often so intimately related genetically as to be inseparable.

Mutual benefit follows from this closer association of the two subjects. The mining geologist, on the one hand, is guided by the important principles established by petrologists in relation, for example, to magnetic differentiation, and the assimilation of adjacent rocks to which Dr. Brammall has to-day made a notable contribution ; and petrologists, on the other hand, will not be content to base their conclusions as to rock formation on minerals that are transparent in thin section, but will pay more attention than formerly to the significant presence of opaque minerals, especially now that modern technique enables even small disseminated grains of these latter minerals to be determined as separate species.

There is some satisfaction in knowing that petrologists are confirming that experienced mining geologists have been pursuing their subject on right lines. The intimate genetic relationship between certain ore-deposits and the containing or neighbouring igneous rocks, has long been established, and forms one of the main principles that have guided the mining geologist in his search for new deposits, and in the study of mineralized areas.

The recognition of the genetic relationship of igneous rocks over considerable areas, as in petrographic provinces, has enabled petrologists to form conclusions of the greatest importance. Similarly, the recognition of the genetic relationship of ore-minerals in metallographic provinces, enable mining geologists to advance conclusions of great economic importance. Again, the differences observable in certain related igneous rocks have been shown by petrologists to be due mainly to differences in the temperature and pressure conditions under which they consolidated ; these same factors have been shown by mining geologists to account, in great measure, for the variations in mineral deposits of primary origin, when followed in depth or in a lateral direction.

This brings me to the point I should like to emphasize, namely, that mineralizing processes related to igneous rocks are best understood, not mainly from the examination of specimens under the microscope, or on evidence from one part of a mineralized area, but from the study of the mineralization in relation to the metallographic province in which it occurs, paying particular attention to the zonal arrangements of the minerals. Consequent on changes subsequent to the primary mineralization, parts of a mineralized area may show evidence that can be very misleading. It is my opinion that certain hypotheses, now discredited, on the genesis of mineral deposits would not have been advanced if the mineralization had been considered on these broader lines ; for although the workable parts of a deposit may be very confined, yet the mineralization in the scientific sense is often as widespread as are the related igneous rocks. This is a fact of great significance, and I am convinced that, in some cases, disseminated grains of ore-minerals can furnish as reliable evidence of the genetic relationship of separated outcrops of igneous rocks as do rock-forming minerals, for both groups of minerals are often of common parentage. The Cornish granite masses, for example, are to some extent stanniferous ; the cassiterite is genetically intimately related to the granite and granitic rocks, and its presence appears to me to be as helpful an indication of the common parentage of the various granite masses as any other mineral common to them. The general predominance of wolframite over cassiterite in parts of Lower Burma ; the predominance of cassiterite over the tungsten mineral in Malaya and the Dutch East Indies ; the occurrence of these two minerals in closer proportions at intermediate places ; and the fact that the tin and tungsten minerals are present to some extent in innumerable areas where granite and granitic rocks occur in the belt stretching for hundreds of miles from Lower Burma to Siam, Malaya, and the Dutch East Indies, lead to conclusions of great scientific and economic importance.[1] It is no coincidence, but the result of differences of relative temperatures of formation of the tin and tungsten minerals, that in the wolframite-cassiterite zone the metamorphosed sedimentaries have a general lower cassiterite ratio to wolframite than in the granitic rocks, and that there is a general predominance of the tungsten mineral in the northern and less denuded part of this metallogenetic province. It is not surprising, when these mineralized areas are considered as parts of the same metallographic province, to find striking similarities in the granites and granitic rocks from Lower Burma southwards to the Dutch East Indies, and in the metalliferous minerals they contain.

[1] Jones, W. R.: 'Tin and Tungsten Deposits: their relative temperatures of formation.' *Trans. Inst.M.M.* 1919–1920, pp. 320–376.

Some few parts of this metallographic province, like parts of a petrographic province, show evidence that appears, if considered in the parochial manner to which Prof. Cullis referred, to contradict these general conclusions; but the broader the basis, especially when fortified by detailed work over extensive parts of the mineralized belt from its northern to its southern limit, the more stable should be the theory built upon it.

Prof. H. Fairfield Osborn.—*Continental Migrations of the Jurassic Sauropoda and the Tertiary Mammalia.*

The sauropod-migration theory advanced in this paper is that the Upper Jurassic—Lower Cretaceous migrations of the giant reptiles known as Sauropoda paved the way for the primary migrations of the primitive Jurassic mammalia, thus giving a world-wide distribution to the pro-mammalia, pro-monotremes, pro-marsupials and pro-placentals. This sauropod theory would explain the presence of similar-stock mammals from which the higher forms adaptively radiated, each centre of radiation starting with its distinctive prototypes and ending through adaptive radiation with many parallelisms and convergences. The presence at widely scattered points of similar multituberculates has been reinforced recently by the discovery of *Notostylops* in South America, Central Asia, and the Rocky Mountain region.

To present this theory clearly, a new equal-area world map has been prepared to display the great migration routes of the sauropods, probably from a Central Asiatic stock into all the continents, including Australia and the great island of Madagascar. Recent studies of the sauropods by Matthew, von Huene, Longman, and Matley, following the earlier researches of Owen, Marsh, Cope, Osborn and Gilmore, all showing the world-wide distribution of similar forms of sauropods, tend to confirm the central Asiatic theory of origin.

On this new zoogeographic map are plotted the successive discoveries of great mammalian centres of adaptive radiation, namely, (1) Europe, (2) India, (3) Australia, (4) South America, (5) Central Asia, which leave two very important centres still to be discovered: (6) northern Asia and (7) western Africa. The radiations from these seven great mammalian centres have gradually been traced back into Lower Eocene and finally into Upper Cretaceous time, facts consistent with the sauropod migration theory.

Mr. V. S. Swaminathan.—*The Materials obtained by Dredging and Drilling in the Tuticorin Area, South India.*

The paper embodies the results of the examination of materials obtained by borings connected with the improvement of the Tuticorin Harbour. Recent changes in the coastal outline—during geological and historic times—are discussed and the source of the materials touched upon. This is followed by a detailed description of the strata penetrated along:—

1. The Reclamation Line and Shore Turning Basin;
2. The Canal Line;
3. The Devil Pass Channel;
4. The Turning Basin and
5. The Berth Borings.

Some six charts and diagrams illustrate the paper.

Dr. C. A. Matley.—*The Harlech Dome.*

This communication deals with the stratigraphy of the mountainous Cambrian country of Merionethshire between the Vale of Festiniog and the Barmouth estuary, and is founded on the field-work of the late Prof. Charles Lapworth and Dr. T. Stacey Wilson, whose survey of the region on 6-inch and 25-inch scales was left unfinished and unpublished, except for a brief statement of the stratigraphical succession which appeared in a paper on the Dolgelley Gold Belt by A. R. Andrew, who described the south-eastern part of the area in 1910.

The present author, assisted from time to time by Dr. Wilson, has made an extensive re-examination of the earlier surveys and completed much of the unfinished ground. Maps showing the results are exhibited, together with a number of the maps of Lapworth, Stacey Wilson and Andrew.

The workable Manganese Ore bed has been found of great value in elucidating the detailed structure of the Harlech Dome, and Lapworth and Wilson's statement that it occupies a single geological horizon is confirmed. J. G. Goodchild's conclusion that there are two beds of ore separated by at least 850 feet of sediments appears to have been based chiefly on deceptive repetitions of one bed by strike faults.

The folding, faulting and cleavage of the region are described, with their effect on the numerous igneous intrusions. The dykes are often displaced by an important system of faults that trend on the whole about N30°E.

Mr. A. James and Prof. E. W. Skeats.—*The Basaltic Ridges and Valleys of the Stony Rises near Colac, Victoria.*

The district lies west of Colac, which is 92 miles west of Melbourne.

Topographic and geologic features are all basaltic, and geologically recent, consisting of ridges and valleys between 400- and 600-foot contours, hills and craters rising above the ridges and numerous lakes, both shallow and deep, whose margins approximate to the 400-foot contour line.

Scores of thousands of ridges and valleys occur within an area of roughly 20 square miles. The ridges vary in height from 18 inches to 50 or 60 feet, in length from 30 yards to about half a mile. Characteristically hogbacked in shape, with side slopes from 30° to nearly vertical, many show longitudinal undulations, some terminating in steep knolls. Longitudinally straight and curved forms occur. Many ridges show gaping summits up to 20 feet in depth, and are known locally as breached barriers. In a few cases the breach is occupied by dyke-like basalt of different texture. Others show depressions of the apex of the hogback amounting to from 1 to 5 or 6 feet. The basalt is usually fresh, with a smooth or vesicular surface, a general absence of soil, and with columnar jointing sometimes closely set, sometimes gaping, at right angles to the exterior slope of the ridge. Near the shores of Lake Corangamite the ridges are small, widely spaced, rising above a flat surface of bedded tuffs and the typical valleys are wanting. To the S.W. away from L. Corangamite the ridges occur at a higher elevation, are confluent and rise 50–60 feet above valleys, which are original features constituting the space between the ridges, and are clearly not erosion features, for they show no constant longitudinal slope but considerable undulations in level. No surface flowing stream traverses the Stony Rises, but rainfall sinks below the surface and emerges at low points as strong, fresh-water springs.

Hypotheses as to the origin of the ridges, knolls and valleys have been discussed in the field.

They are not normal lava flows, do not proceed directly from any of the volcanic cones and two linear trends, N. and S. and N.W. and S.E., can be distinguished. Sometimes one set alone occurs, but in the heart of the Stony Rises both sets often occur. They are believed to develop from thousands of small fissures beneath them, the rise of the basalt magma through each fissure and its limited superficial extrusion may be connected with pressure on a basaltic reservoir beneath, due to the sagging of the surface to which many of the lakes are due. Renewal of the sagging may cause further rise of basalt, causing the solidified crust of the ridge to form a gaping fissure by pressure, and in a few places to form a dyke in the gap. Most of the breached ridges away from Lake Corangamite, however, probably owe the development of the breach or sag to subsequent withdrawal of molten basalt below the crust. This withdrawal may be connected with relief of pressure beneath, developed by explosive activity, by which the scoria and tuff cones of Mt. Porndon and other volcanoes were formed. This view is supported by the presence of basalt caves near Mt. Porndon with a hogback floor, whose arched roofs bear pendent basalt stalactites indicating that withdrawal occurred while the basalt below the crust was still molten.

Wednesday, September 30.

DISCUSSION on *The Genesis of Oil Pools in the Sedimentary Cycle.* (Prof. V. C. ILLING; Prof. P. G. H. BOSWELL, O.B.E., F.R.S.; Prof. W. S. BOULTON; Dr. G. M. LEES; Dr. J. V. HARRISON.)

Prof. V. C. ILLING.

That oil is produced from the organic matter in sediments is now accepted generally as the only reasonable theory. Where diversity of opinion enters is, firstly, as to the type of organic material involved and, secondly, the cause of the transformation. So far as the present discussion is concerned, the first problem will be glossed over, accepting merely the broad statement that the basic material is organic and may include a diversity of chemical compounds. So far as the mechanics of the change is concerned, the different views may be broadly assembled into two camps. The first postulates heat or other internal physical agencies as the reacting agent, the second considers that the process of change is biochemical. One of the essential differences in these two theories lies in the fact that the one presupposes oil formation as a separate and later process after the sediments have been changed into rocks, whereas the biochemical theory considers oil formation as a part of the process of sedimentation, a change taking place in the sediment as it is being laid down.

The metamorphic theory has the sanction of the chemists as a perfectly feasible process, though there are geological arguments which render its application doubtful; the biochemical theory has not yet been proved as a possibility, but it has gained many adherents on the indirect evidence. A theory is not necessarily true because it is simple, but when a series of separate ideas can be linked together to form a chain of evidence, the probability of the theory is greatly increased.

In this case the speaker puts forward as a support of the biochemical theory the fact that it, and it alone, links up satisfactorily with the experimental evidence of migration to explain the characteristics of our oil accumulations. Origin, migration and accumulation linking up as a complete chain of events without any interlude. It is now many years since Munn put forward his Hydrostatic theory of migration; but it has never been emphasised sufficiently that the migrating water currents which he presupposes are so much more general in the phase of compaction, that it is only on a theory of oil formation *pari passu* with sedimentation that the Hydrostatic theory can receive its full development.

One of the basic facts of oil accumulation is that the oil always occurs in the coarsest available rocks; Dolomite in limestone, sands in clays, or coarse sands in fine sands, the essential being a coarser porous rock surrounded by a finer non-porous or water-bearing rock. This has been explained as due to the greater surface tension of water and the displacement of the oil from the finer strata, but such displacement is difficult to reproduce experimentally and it is quite possible experimentally to reverse the liquids.

During the course of experimental work on the subject, it has been found that when two immiscible fluids are forced from a fine to a coarse rock, there is an uninterrupted passage of both liquids, but when the flow is reversed the fluid which wets the grains passes freely from the coarse to the fine, while the other liquid is retained at the coarse-fine interface and can be accumulated and concentrated there. By suitable arrangements it was found possible to trap completely the gas and oil carried by water in sand pockets, and to concentrate oil from very dilute mixtures.

Such free migration is only possible before the clays are thoroughly compacted, and it appears probable that the processes of oil formation, migration and accumulation can be linked into one continuous chain initiated and continued through the phase of sedimentation.

Prof. W. S. BOULTON.

During the last 20 years or so of oil-field development, the great accumulation of geological fact and experience has given us a reasonable explanation of the concentration of oil into pools from the areas in their immediate vicinity. The relative

importance of the governing factors which control such concentration, such as gravity, hydrostatic pressure, gas pressure, capillarity, etc., is still, however, a matter of dispute in different fields. All these factors doubtless operate in inducing migration in varying degree. An arrest of the slow migration of the oil, whether upward in wet rocks or downward in dry rocks, and brought about by geological structure or diminution in porosity, tends to form oil pools. It is practically certain that Salt Domes and Plugs act in arresting and accumulating oil, and are not the source or carriers of oil in quantity.

The problem of the origin of oil, and its occurrence in some geological systems, as in the Tertiary, in great pools in many parts of the world, while similar strata in other systems are barren of oil, is still unsolved or in serious question.

The weight of evidence is in favour of an organic origin, and from minute organisms, such as Diatoms, Foraminifera and others destitute of hard parts. Shallow water marine deposits with such abundant organic remains, and with intercalated porous beds capable of acting as reservoirs, are all potentially oil-bearing.

It is likely that the necessary conditions for the decomposition of the organic matter and generation of oil are deposition in stagnant waters, and the sealing up of the sediment to permit of the action of anerobic bacteria.

In some cases the oil may have been deposited with clay in shallow marine waters, the oil having previously been formed by the decomposition of vegetable matter in swamps and lagoons, and afterwards carried by streams to the sea, as first suggested by Murray Stuart for the Pegu series of Burma.

The most abundant supplies of oil occur in Tertiary shallow water marine strata which fringe the great geosynclines of the Old and New Worlds, and flank the chief mountain ranges.

The fact that Tertiary strata contain most of the known oil supplies may be partly explained by their recent age, the oil not having had time up to the present to escape in sufficient quantity. The presence of oil pools in some older systems, *e.g.* the Mississippian of North America, may be due to their great lateral extent and comparative quietude since the deposition of their sediments, together with their effective cap rocks.

Is it possible that in Carboniferous and Cretaceo-Tertiary periods the food supply for oil-generating organisms derived from excessive vegetable growth on the lands was more plentiful than in other periods ?

Dr. G. M. Lees.

Bituminous rocks, oil shales or traces of oil are not uncommon in many countries at one or perhaps several horizons throughout the sedimentary succession ; but the formation of oil in quantity, and its subsequent concentration into oil pools of commercial value, requires a set of conditions which is undoubtedly exceptional. Oil may be carried by later migration, whether in vertical or horizontal direction, far from its original source rocks, and in consequence it is often difficult to obtain unequivocal evidence regarding the oil mother rock and the nature of the source material.

Oil appears to have originated throughout long eras of geological time under various conditions of sedimentation, and probably no single source material was responsible for all the oil. The source material for coals of different type was equally varied, though all coals have one fact in common, namely, a vegetable origin. Most oils, though not necessarily all, have been formed in rocks of marine or estuarine origin. There is no known proved case of oil as such being formed at the present day, but oil can be obtained by distillation from many marine muds or oozes. Investigation of the bottom of the Black Sea has proved the presence of yellow and white ether soluble fatty bodies in the mud at a depth of 920 metres.

The original source material of present-day oil underwent a ‘ cracking ’ process during the immense course of time when it lay buried to a depth of many thousand feet under later sediments. In many cases this depth is from 10,000 to 20,000 feet, or exceptionally even more. This represents approximately as many pounds per sq. inch pressure as feet of overburden for average sediments and a temperature of 150° to 250° C.

Oil is associated in many countries with salt and gypsum deposits, but the writer knows of no case of oil in quantity occurring in such rocks where there is not a probability of its having its origin in normal marine rocks. Algæ flourish in many

present-day brackish lagoons, or organic material may be carried in, forming source material for thin oil or bituminous shales, but it is unlikely that such beds are the source of appreciable amounts of oil.

The Miocene of Egypt, Persia and Iraq has often been cited as instances of a salt-gypsum petroliferous series, but in all three cases there is much stronger evidence pointing to an origin in marine foraminiferal marls.

The most constant bituminous formation in Sinai, Palestine, Transjordania, Syria, Iraq, and Persia is the foraminiferal marls of the Upper Cretaceous. Locally similar lithological conditions extend upward into the Eocene or downward into the Lower Cretaceous and Jurassic, and where this has occurred these higher and lower formations are also bituminous. The Senonian is, however, the richest horizon and the most constantly so. The rocks range from marls to chalky marls and marly limestones. They are very rich in smaller foraminifera, in particular globigerina, but are poor in macro fossils, except locally at one or two definite horizons.

In the oilfields belt of S.W. Persia, the Upper Cretaceous at one time lay under a cover of 15,000 to 20,000 feet of later rocks, and with a pressure of as many pounds to the square inch and a temperature of about 200° C. the original organic material has been completely transformed into oil or bitumen. Subsequently, upward migration took place, the oil in many places reaching as high as the Upper Fars or Bakhtiari beds of Pliocene age. The principal oil reservoir rock in the Persian oilfields is the Asmari limestone of Lower Miocene and Oligocene age.

Dr. J. V. Harrison.

For the development of an oil pool two conditions seem to be necessary. Firstly, a petroleum mother rock must be laid down, and secondly, forces must act on this potential oil supply to make the oil available.

A petroleum mother rock is a deposit laid down in stagnant, sulphur-polluted water in which consequently a normal fauna cannot live. It is a fine-grained, ferrous sulphide-bearing sediment, rich in organic matter, often of animal origin. Decay of this organic matter proceeds slowly anærobically, usually with the operation of sulphur-loving bacteria. In general, such deposits appear to be of marine origin, although lagunal, estuarine, and perhaps even fresh water conditions can also be favourable. The conditions of stagnation may be temporarily disturbed by local currents which may give rise to a layer of grit before further slime is deposited when stagnation recurs.

In the fossil condition, a mother rock is found as a marcasite or ferrous sulphide rich bituminous shale. Fossils are confined to the remains of organisms which have fallen into the original slime, such as the scales, teeth, and excrement of fish, tests of foraminifera, ammonites, or belemnites and rarely reptilian and mammalian skeletons. The calcareous remains have frequently been replaced by marcasite or pyrite, and phosphatic residues such as glauconite are commonly encountered. When variable conditions have occurred, a thin bedded shale results, the layers being separated by thin partings of calcareous sands.

The stagnation occurs in seas of a comparatively restricted area such as the Black Sea, or in some oceanic deeps where the disturbing influence of ocean currents does not obtain. In lagoons the stagnation may be accompanied by settling crystals of salt and anhydrite, or, where abnormally large amounts of organic matter are available, mother rock may develop. East of the Rio Unare, near Piritu in E. Venezuela, a present-day example of such a saline slime occurs, where abundant organic matter is furnished by the bodies of fish which are killed annually by the progressive desiccation of the lagoon during the dry season.

From a petroleum mother rock an oil pool can be produced, given proper tectonic conditions to drive out the potential supply and a reservoir rock to contain the oil generated. Folding or loading may supply the necessary force, and a reservoir may be encountered either above or below.

In Persia, a mother rock answering closely to this definition occurs in the Eocene Cretaceous as globigerina, sulphide, rich and rarely glauconitic marls. The Pliocene orogenesis was probably effective in driving out the oil from the mother rock into the Miocene-Oligocene limestones, in which it is now found.

In England, the Oxford and Kimmeridge Clays are mother rocks, but the treatment they have received since deposition has not been favourable to concentration of the potential oil into a workable oil pool.

SECTION D.—ZOOLOGY.

Thursday, September 24.

PRESIDENTIAL ADDRESS by Prof. E. B. POULTON, F.R.S., on *A Hundred Years of Evolution* (see page 71).

Prof. J. S. HUXLEY.—*Development and Evolution.*

Mr. E. B. FORD.—*Mendelism, Genetics, and Evolution.*

Dr. F. A. DIXEY, F.R.S.—*Natural Selection and Evolution.*

Natural Selection is the keystone of the Darwinian position. Its recognition by Darwin and Wallace rationalised previous evolutionary theory. Natural Selection is a logical necessity, arising from the fact of over-production of offspring. But in the absence of inheritable variation it could not lead to progressive development. Inheritable variation, however, is an ascertained fact. Mendel has shown under what limitations it can appear in successive generations of offspring. Subsequent work on the lines laid down by Mendel, and especially in the hands of Fisher, Haldane and Ford, has established that selection is a necessary feature in the application of Mendelian fact to evolutionary theory. It is impossible to over-estimate the importance of adaptation ; but to suppose that a complicated adaptation, such as that of an aquatic derived from a terrestrial mammal, has arisen as a sport or saltation, is equivalent to reviving the doctrine of special creation. Adaptation has evidently been gradual, and can most reasonably be supposed to have taken place by means of natural selection. That some kinds of variation imply a certain amount of correlation has been thought to be a difficulty ; but it has been shown that one germinal element, or perhaps a linkage of two or more elements, can control more than one somatic feature. This kind of correlation is obviously different from the elaborately specialised adaptation of an aquatic organism such as a whale. On the other hand it has been argued that a simple change of small amount can have no selection value. Evidence to the contrary can be brought from many quarters, and especially from the phenomena of insect mimicry, from which it may be presumed that similar conditions prevail elsewhere. Natural Selection has been shown by Darwin and his successors to be a rational factor in the course of evolutionary development ; it has been recognised as such by the chief workers in the field of genetics ; and objections brought against its competence in the formation of species disappear on close examination.

Prof. H. FAIRFIELD OSBORN.—*Nine New Principles of Evolution revealed by Palæontology.*

The ratio of vertebrate species known in Darwin's time—8,767 species—to those known in 1925—65,939 species—nearly 8 to 1, is about the measure of the biological progress of the first century of evolution. From the completely solved ' origin of species ' problem we now concentrate on the ' origin of new characters and co-ordination of single characters ' in the lower animals and in man as the outstanding problem of the second century of evolution, to which the combined research of zoologists and palæontologists should be applied.

To the palæontologist experimental zoology is pure Darwinism camouflaged ; the zoologist sees the temporary and accidental, the palæontologist the secular and eternal. As compared with *eleven* principles of adaptation revealed in zoology from Aristotle to the present time, intensive palæontology reveals *nine* principles of biomechanical adaptation.

The eleven zoological principles of individual development, named in the order of discovery and prefixed with the terms *onto-*, *auto-* or *co-*, are : retrogression, progression, compensation, economy, change of proportion, co-adaptation, acceleration,

retardation, self-adaptation, saltation, continuity of germ plasm; palæontology confirms nine of these principles and recognises with Weismann that they are primarily germinal or 'geneplasmic' phenomena.

Through intensive study of *phyla* extending over millions of years, palæontology reveals (1869–1931) *nine* additional principles which may be prefixed with the term *phylo-*, namely: mutation of Waagen, *mutations-richtung* of Neumayr, acceleration and retardation of Hyatt, continuity of evolution, potentiality, predetermination, rectigradation, 'aristogenesis.'

To the palæontologist in search of causation, what relation do Principles 1–11 bear to Principles 12–20? If there is invariable antecedence and consequence, as assumed by Lamarckians, Principles 1–11 would inevitably precede 12–20; *such antecedence and consequence are not observed.* The sum of these principles is as follows:—

BIOMECHANICAL ADAPTATION IN ONTOGENY.

Discovered in Anatomy and Embryology.

1. Biomechanical *onto-retrogression* (Aristotle), degeneration, atrophy.
2. ,, ,, *progression* ,, development, hypertrophy.
3. ,, ,, *compensation* ,, metatrophy and eutrophy.
4. ,, ,, *economy* ,, ,, ,,
5. ,, ,, *proportioning* (Lamarck-Darwin), change of proportion (allometry Osborn).
6. ,, *co-adaptation* (St. Hilaire), co-ordination, correlation.
7. ,, *onto-acceleration* (*v.* Baer) of organs into earlier growth stages.
8. ,, ,, *retardation* (*v.* Baer) of organs into later growth stages.
9. ,, ,, *adaptation* (Goethe) through principles 1–8.
10. ,, ,, *saltation* (St. Hilaire), sports, discontinuity.
11. ,, *continuity of germ plasm* (Weismann).

BIOMECHANICAL ADAPTATION IN PHYLOGENY.

Discovered in Palæontology. Unknown to Darwin.

12. Biomechanical *phylo-mutation* (Waagen, 1869), orthogenesis.
13. ,, ,, '*mutations-richtung*' (Neumayr) (trend).
14. ,, ,, *acceleration* (Hyatt, 1880) in the evolution of organs.
15. ,, ,, *retardation* (Hyatt, 1880) in the evolution of organs.
16. ,, ,, *continuity* (Osborn, 1906–31) *vs.* discontinuity.
17. ,, ,, *potentiality* (Osborn, 1906–31) in the origin of new biocharacters.
18. ,, ,, *predetermination* (Osborn, 1906–31) in the origin of new biocharacters.
19. ,, ,, *rectigradation* (Osborn, 1906–31) in the origin of new biocharacters.
20. ,, ,, *aristogenesis* (Osborn, 1906–31) in the rise of organisms.

Palæontology is the two-edged sword of Biology; it cuts hypotheses unfit to survive; it strengthens hypotheses fit to survive. It calls for conceptions of a new and physico-chemical order to supplant outworn hypotheses dating back to Empedocles (600 B.C.). It disestablishes the 'entelechy-vitalism' of Aristotle (300 B.C.) and establishes his 'firm and undeviating order of Nature.' It establishes the 'direct action of environment' (Buffon, 1755–Wagner, 1870). It disestablishes the habit-inheritance law of E. Darwin and Lamarck (1790–1809) and establishes habit, through onto-adaptation, as a guiding principle in evolution. It disestablishes the third and fourth principles of C. Darwin (1859) and establishes selection as a universal and outstanding principle from the beginning of time. It firmly establishes the 'W. Mutations' of Waagen (1869). It establishes the 'continuity of germ plasm' and the 'separateness of somaplasm' of Weismann (1880). It disestablishes the hyper-selection of the neo-Darwinians. It excludes entirely from biomechanical evolution the 'D. Mutations' of De Vries. In its earliest as well as its latest phases palæontology undermines the primitive idea of *created evolution*; of recent years it firmly establishes *creative evolution* or 'aristogenesis.'

DISCUSSION on *Evolution* (following previous communications).

Friday, September 25.

Discussion on *Vertebrate Embryology* :—

Prof. E. W. MacBride, F.R.S.—*The Formation of the Layers in Vertebrata.*

The question of the method of the formation of the layers, *i.e.* the primary tissues ectoderm and endoderm and mesoderm, is of fundamental importance in embryology, since if there is a recapitulatory basis for development this method should give some hints as to the steps by which a Protozoon was converted into a Metazoon. Amongst the higher vertebrata, the early stages are complicated by the influence of yolk, uterine development and so on; hence reliance is placed on the development of amphibia in which the yolk is of moderate amount and the whole egg segments into cells.

Recently Vogt has developed a method of marking the living eggs of Urodela and Anura by pressing against them in the stage when segmentation has just been completed small fragments of agar soaked in neutral red, Bismarck brown or Nile blue. As the cells which are stained move, these marks are carried with them. Segmentation, as is well known, results in the formation of a blastula or hollow ball, with small cells above and large yolky cells below. At a point in the midst of the yolky cells a small pit appears; this is the beginning of invagination, which is an active process of inflow; the initial pit is subsequently found at the extreme anterior end of the gut. The inflow involves not only the large yolky cells which become converted into the endodermic epithelium, but at the dorsal lip of the opening and at its sides small-celled material; the dorsal material gives rise to the notochord and the lateral to the mesoderm. The mesoderm does not, as Hertwig supposed, originate from the dorso-lateral wall of the gut, but grows in from the edges of the blastopore and tends forwards and upwards. After the endoderm has been invaginated there is an active creep or '*epiboly*' of the blastoporal rims over the hinder end of the yolk (the yolk-plug) so that the blastopore is eventually closed, leaving in Urodela a small residual opening which is the anus. The blastopore in its later stages becomes a vertical slit; the nerve-cord is not lengthened by the meeting of the edges of this slit, but these edges give rise to the tail bud.

Vogt is inclined to agree with Lwoff in regarding all the small-celled material as ectoderm; this is equivalent to saying that ectoderm and endoderm are sharply differentiated before invagination takes place, and that the mesoderm and notochord are ectodermic invaginations. There are grave, and, it seems to us, decisive objections to this point of view: (1) not all the mesoderm grows in from the edge of the blastopore—there is in Elasmobranchs an anterior vesicle cut off from the front end of the gut which gives rise to the eye-muscles; (2) the development of the simplest known egg, that of Amphioxus, which has a minimal amount of yolk, gives no countenance to Vogt's views. In Amphioxus all the nuclei of the blastula have vesicular nuclei which absorb little stain and all the nuclei of the invaginated cells, those which will eventually form the notochord as well as those destined to form the gut epithelium, retain this type of nucleus. On the other hand, the nuclei of the outer cells which form the ectoderm and the nerve-cord become dense and absorb much stain. The limits of the two kinds of nuclei can be seen sharply defined at the edges of the blastopore. The mesoderm arises from five hollow pouches of the gut, one median, ventral and anterior which corresponds to the pouch which in Elasmobranchs gives rise to the eye-muscles, and two dorso-lateral pairs close behind it. In the short, thick, early embryo the openings of the two pairs are close to each other; the openings of the hinder pair being well in front of the blastopore. The front pair correspond to the mesoderm, which in Elasmobranchs gives rise to the superior oblique eye-muscles; the hinder pair correspond to the 'mesodermic bands' of higher vertebrata which become metamerically segmented into myotomes. As the embryo grows in length the openings of this hinder pair of pouches become carried back to the hinder end of the embryo, whilst *pari passu* the front ends of these pouches become segmented into myotomes. These pouches are well represented in the embryo of Balanoglossus and in the Tornaria larva: the anterior median one being the rudiment of the proboscis cavity, the anterior pair the rudiments of the collar cavities, whilst the posterior pair give rise to the trunk coelom. The openings of the posterior pair are near those of

the anterior pair in the Tornaria larva, but are carried back by interstitial growth exactly as in Amphioxus as the trunk lengthens, but they are not segmented into somites.

The earliest traces of Vertebrates are found in freshwater beds; it is strongly indicated that the segmentation of the mesoderm, which is a diagnostic feature of the vertebrates, was brought about when the ancestral marine group entered the rivers and had to maintain themselves by vigorous lateral blows of the body against being swept away by the current.

Mr. J. H. Woodger.—*Germ Layer Formation in Birds.*

Prof. J. Graham Kerr, F.R.S.; Mr. G. L. Purser, and others.

Saturday, September 26.

Discussion on *Population*:—

Prof. J. S. Huxley.

Prof. A. M. Carr-Saunders.—*The Population Problem.*

Prof. L. T. Hogben.—*Experimental Biology of Population Growth.*

Prof. J. B. S. Haldane.—*Some Causes and Consequences of Differences in Viability.*

General Discussion. (Prof. E. W. MacBride, F.R.S.; Prof. F. A. E. Crew.)

Prof. E. W. MacBride, F.R.S.—*The Problem of Population.*

It is a commonplace that in Nature every species of animal produces so many offspring that in a few years, if all survived and reproduced as did their parents, every species would overrun the entire globe and crush out every other form of life. Thus a pair of thrushes in ten years, which is the normal life of a thrush, would produce 19,000,000 offspring, whilst the brood produced annually by a pair of herring would, if they all survived, cause the herring population to increase 5,500-fold a year. Whilst in general natural selection limits the increase so that the constituents of the population remain relatively constant, occasional enormous increases in the number of certain species take place. The age of herring can be computed by counting the rings on the scales. When this is done it is found that the herring hatched in one particular year may supply the bulk of the catch landed for four years in succession. The same is true of other food-fish. In cases such as that of the plaice, where the eggs float, the average number of eggs laid each year can be reckoned; it is then seen that 'a good fishing year' is due not to an increased birth-rate, but to an increased survival rate, because in certain years an increased supply of microscopic plants is present to provide food for the young fry when the maternal food-supply is exhausted. The highest mortality takes place at this point. If a fish survives this crisis its expectation of life is enormously increased.

In land animals the same periodic increases in population occur due to the same causes: the overcrowding resulting from them acts as a stimulus to mass migration, which produces devastating effects. Thus Uvarov has shown that an ordinary harmless grasshopper becomes changed into a devastating locust.

The study of human history discloses the fact that periodic migrations of human tribes were common in ancient times, and that civilisations, notably the Roman civilisation, have been swept away by them. Tradition indicates that these migrations were due to periodic increases in population. One of the most notable increases in population which has occurred in comparatively recent times has taken place in our own island, and, as everyone knows, it has been accompanied by an enormous

emigration, mainly to N. America. There were about 5,000,000 people in England and Wales in 1700, 8,750,000 in 1800, and about 33,000,000 in 1900. It is to the credit of Miss Buer to have shown that these increases are due not to an increased birth-rate but to an increased survival rate, which she attributes to improved medicine and to humanitarian effort. Of children born in London three-quarters died before reaching the age of five in 1730 ; in 1830 only one-third of those born died before the age of five.

Since the war emigration which corresponds to prehistoric migration, has been so restricted that relief from this source is minimal ; and though the birth-rate is falling off, this is due to decreased fertility amongst the prudent and thrifty, not amongst the stupid and careless. If this goes on the constitution of our population will be radically changed, and our greatness will disappear. The only remedy seems to be the spread of the knowledge of the means of birth control, and in the last resort compulsory sterilisation.

Monday, September 28.

DISCUSSION on *Classification with reference to Phylogeny and Convergence.* (Dr. C. TATE REGAN, F.R.S. ; Dr. F. A. BATHER, F.R.S. ; Dr. W. D. LANG, F.R.S. ; Dr. J. STEPHENSON, F.R.S. ; Dr. HUGH SCOTT.)

Dr. F. A. BATHER, F.R.S.

Classification must be practical, its object being to enable us to name and place assemblages so that we can make statements concerning them. Classification must be based on verifiable fact, not on inference. If we knew all, could we construct a Phylogeny ? If we could, then could we express it in classification ? We do know enough to see that we should meet many difficulties.

Beginning near the base, with apparently homogeneous populations, we know from genetic experiment that they are often heterogeneous. The composite nature of many fossil populations of a single species has been shown by Trueman and others. The species itself comprises many races (Jordanons in the Linneon). Parallel forms or isomorphs appearing in allied species, as shown by Vaviloff, may represent identical genetic factors. The species then is heterogeneous. Genera also are heterogeneous, not because each is composed of distinct species, but because its constituent species represent lineages that have passed into it from some pre-existing genus or genera (parallelism or convergence). For examples, refer to Bather, Presidential Address Geol. Soc., July 1927, and Schindewolf, *Palæont. Zeitschr.* IX, 122–169, August 1927. The difficulty arises in deciding whether to select grades of structure or lines of descent as bases of genera. The latter, besides being inferred, are difficult of definition, except as (supposed) descendants of a (presumed) single ancestor. To adopt both vertical and transverse divisions is to produce an arrangement like a crossword puzzle, in which each generic pigeon-hole may contain but one species. While such parallelism is common, it is very difficult to find clear evidence for the splitting of one species into two, such as may ultimately give rise to two genera. Divergence generally affects diverse forms. A generic grade may arise in different places (*Metaxytherium*), or at different times (*Gryphæa*), or both (*Balanocrinus*). Phylogeny *à outrance* leads to pulverisation of genera ; we want rather to return to larger genera, and to make more use of the grade concept. A nomenclature and a classification based on ascertainable fact will enable us to construct any number of phylogenies according to each worker's interpretation.

Similar difficulties are met with in the larger groups, and are to be overcome in the same way, by greater use of the Grade.

Though classification must be based on structure, it needs a rational foundation—an expression preferable to ' natural ' and not a whit more subjective. Seeking the causes of structure we find : (*a*) the material, *i.e.* all inherited structures and qualities ; (*b*) the forces that fashion it, *i.e.* environment, both outer and inner, this also expressed as ' functional.' But the hereditary material itself was once fashioned by the environment, and so continuously backward. Distinction between the successive characters is possible only through palæontology. It is the relative age that confers morphological importance and decides what is convergence and what only parallelism.

In all cases, from species to phyla, material originally indifferent or heterogeneous has been integrated into classifiable groups by the environment. It is, no less than descent, worthy of consideration as a basis of rational classification (see Presidential Address to S.W. Nats. Union in its *Proceedings*, II, 73–91, January 1931).

Dr. W. D. Lang, F.R.S.

Increasing specialisation in Systematic Zoology leads to increasing difficulty in expressing classificatory facts in terms of Linnean nomenclature; but, so long as Recent organisms only are dealt with, the difficulty is mainly one of complication, and no new principle is involved; with certain reservations, morphic similarity is usually considered a safe criterion of phyletic affinity. On the other hand, when organisms are studied in the light of stratigraphy, and the Recent fauna is seen to be but a cross-section of innumerable lineages, then systematic units based upon Recent organisms often are seen to be polyphyletic, and the difficulties of fitting the supposed facts of phylogeny into an appropriate frame of orthodox systematic nomenclature become so formidable that one is tempted to divorce systematics from phylogeny. Rather than this it would be better to re-cast our system of nomenclature; but this is not inevitable. The stage we are considering is this: A—B—C—D are terms in a lineage. The lineage is now a genus, and the terms, species. With complete knowledge our species would tend to become arbitrary points in a continuous series.

But a complication arises. We find parallel, often contemporary, lineages with corresponding terms; and immediately there is the difficulty of diagnosing the genera. But still we should keep to principle, and act on the faith that it is our lack of perception that prevents us from indicating a common character running through A—B—C—D by which the lineage differs from a—b—c—d; even if in practice we can only distinguish the corresponding early or late terms, and have to define the lineage, that is the genus, as a unit composed of a term, or terms, possessing certain characters, and including other terms leading up to, or away from, that point.

But a second complication arises. We are told that, in certain cases, parallel lineages interbreed; and so, instead of a bundle of parallel lineages, we have an anastomosing complex. Here systematic nomenclature is up against a formidable difficulty. Still we must not sacrifice what we hold to be phyletic facts to the exigencies of systematic nomenclature. It is generally conceded that at any rate certain species of plants interbreed, or have lately interbred, and the hybrid races have become, or are becoming, new species. The fact is recognised, and the plant is given a new specific name. But in the case we are considering lineages (which we are calling genera) are involved. I cannot see any way but to consider the hybrid species as constituting new genera, and to name them accordingly.

Dr. J. Stephenson, F.R.S.

While we must, of course, see to it that near relations are not too widely separated in our schemes of classification, experience shows that a classification may be useful, and yet be independent of phylogeny; moreover, we often know too little about phylogeny to use it for classification; and where we do know the phylogeny, it would sometimes be highly inconvenient to express it in classification. As to convergence:—the particular group of morphological characters that distinguishes a genus has sometimes been evolved in more than one way; in other words, a genus may have more than one ancestry (may be polyphyletic)—which again cannot be indicated in classification.

Dr. Hugh Scott.

These remarks deal with the bearing of the subject under discussion on zoogeography. Zoogeographers are wont to base far-reaching conclusions on the occurrence of organisms of the same group in widely-separated parts of the world. A consideration of the geographical distribution of terrestrial animals often involves the supposition that either certain species or groups possess means of dispersal over the sea, or land connexions formerly existed between countries now separated by the ocean. Such conclusions are, however, plainly untenable if the members of a

group living in different regions have attained their characteristic form independently, by parallel or convergent evolution. Therefore a group under review from a zoogeographical standpoint should be critically examined as to whether its origin was probably monophyletic or polyphyletic.

What criteria are to be used? Comments are made on this difficulty and a distinction is drawn between resemblances due to the independent adaptation of organs to perform a similar function, and cases of convergence which present no evidence of functional adaptation. Some examples are discussed and illustrated with diagrams: (i) instances in which the modifications characterising a group affect only a single organ or several closely correlated organs, and have almost certainly arisen independently from different parent-stems, while the animals so modified are classed together in one group mainly for convenience (polyphyletic origin); in these cases the modifications may be a direct response to environment and, since the modified forms arise independently in different countries, conclusions of the kind indicated above cannot be based on their geographical range; (ii) an example is given of modifications affecting several different parts of the organism, not apparently correlated nor arising as a direct result of environmental conditions, and it is contended that the group of species so modified had a monophyletic origin, and therefore their presence in widely remote lands is important from a zoogeographical standpoint. Emphasis is laid on the need for observation in the field and the application of experimental methods, whenever possible, so that direct evidence may be gained as to which classes of characters arise by convergence and which have a deeper-seated, phylogenetic, origin.

Tuesday, September 29.

Symposium on Variation and Genetics:—

Prof. J. W. Heslop Harrison.—*Recent Work on Induced Mutations.*

Dr. W. R. Thompson.—*The Species Concept in relation to the Problem of Evolution.*

Dr. W. H. Thorpe.—*Biological Races in relation to the Problem of Evolution.*

A biological race is a sub-specific group of individuals separated from other members of the same morphological species by definite and constant biological differences; structural and pigmentary differences being either completely absent or else very slight and inconstant. The characters by which biological races are distinguished usually concern the food, egg laying and mating preferences, and seasonal distribution. Thus, whereas a geographical subspecies is characterised by differences of average size or pigmentation and is isolated by geographical barriers, a biological race is characterised primarily by biological characters and is isolated by biological differences such as food preferences and mating preferences.

Biological races are known to exist in most of the main invertebrate groups. They appear to be most highly developed in the *Insecta*, *Arachnida*, and the *Nemathelminthes*. There is also evidence of their existence in *Cœlenterata*, *Porifera*, and *Protozoa*. In the two latter groups, where morphological characters are often indefinite and unreliable, the concept of a species fades insensibly into that of a biological race. This, of course, does not imply that the morphological characters of a species, even in the higher groups, are any more fundamental or important than the biological; they are accentuated merely for the sake of convenience in systematic work.

With regard to insects the host plant preferences which provide the basis for most biological races may be either germinally fixed or may be merely 'memory reactions.' The latter are renewed in each generation owing to the preference of an adult insect for oviposition on the particular food plant on which it was nourished as a larva—the so-called 'Host Selection Principle.'

In the case of those which are germinally fixed the probability that such biological characters may be of survival value is obvious. But even where the characters are of no direct value or where they are not germinally fixed they may well be of evolutionary

importance in that they would provide physiological barriers—lines of cleavage in an otherwise homogeneous population—thus aiding the spread and establishment of any new variants which might arise.

There are a number of cases in which true biological races appear to have been induced experimentally. In most of these cases a Lamarckian explanation has been suggested. As regards insects there does not seem to be any one instance in which all other theories are completely ruled out, but there are one or two cases concerning plant feeding Nematodes which seem to make a Lamarckian explanation much more difficult to dispense with.

Taken as a whole the phenomenon of biological races seems to provide a considerable amount of circumstantial evidence for some form of Lamarckian theory.

Dr. O. W. Richards.—*Geographical Races and Evolution.*

Prof. D. M. S. Watson. F.R.S.—*The Evolutionary Importance of the Study of Lineages.*

Prof. J. B. S. Haldane.—*The Genetical Analysis of Interspecific Differences and the Mathematical Theory of Evolution.*

Wednesday, September 30.

Symposium on *The Past and Present of the Overfishing Problem.* (Prof. W. Garstang, F.R.S.; Dr. A. Bowman; Dr. E. S. Russell.)

Dr. A. Bowman.—*The Haddock Population of the North Sea.*

The haddock population of the North Sea plateau may be regarded as a self-contained one, and changes in its population can be studied without reference to contemporaneous changes occurring in such haddock producing regions as Faroe, Iceland, etc.

A census of the total population in this area during the last ten years has been obtained by a combination of three methods :—

1. Collection of statistics and measurements of fish landed at the various markets.
2. Ichthyometrical observations at sea on selected vessels to ascertain the proportions of unmarketable sizes in the total catches.
3. Periodic trawling surveys by the Scottish Research Vessel to obtain scale material and data with regard to the smaller sizes of fish which escape capture by the ordinary commercial gear.

The market statistics collated in a series of monthly charts by the Ministry of Fisheries (England) form the basis of the scheme. Each fishing vessel providing information is in effect a research vessel. The debt we owe to the friendly co-operation of the industry in the collection of such comprehensive data is incalculable.

Scale-age-assessment of the voluminous data has been accomplished by the examination of copious representative samples.

The most striking feature of the annual recruitment of the population is the enormous variation in its incidence. The ratio of the most successful brood to the least successful has been of the order of approximately 50 to 1. Briefly summarised, the period has produced three exceptionally good spawning years and five poor ones for brood survival.

No direct relation can be shown to exist between the weights of the spawning shoals (numbers of eggs produced) and the respective numbers of the broods surviving to reach the sea floor. Under favourable natural conditions, the weight of spawning fish has been sufficient to yield a bounteous crop of baby haddock. Regional differences in growth rates in the earlier years of life remain consistent to type and it cannot be inferred from the investigations that variations in these regional differences from year to year arise from variations in the densities of the respective broods.

The entry of a brood of great numerical strength into the marketable stock, particularly if it follows bad brood years, is immediately reflected in the ‘ quality ’

of the catches. The percentage proportions of ‘extra-smalls’ and ‘smalls’ in the market landings shoot up rapidly.

The results of the age analysis of the market landings bring out clearly that the North Sea haddock fishery is dependent for the most part on the exploitation of young fish from two to four years of age. There is a progressive and rapid decline in the yield of a brood after the third year.

The modern trawl, the apparatus of most importance in the haddock fishery, takes toll of the population before it has grown to marketable size. Thinning of the unmarketable sizes begins before the end of the first year of life, is of greatest magnitude in the second summer, and ceases in the early summer of the third year as laggards in growth attain marketable size.

The gear used effects total capture of sizes much less than that of mean size at first maturity and also less than that of minimum marketable size. Large numbers of small haddock are, therefore, destroyed annually in the course of normal fishing operations, the magnitude of the destruction varying with the numerical strengths and rates of growth of the upgrowing broods and with the incidence and intensity of fishing.

To the annual production of haddock by the region as measured by the weights of fish landed there must be added the weights of fish caught and returned dead to the sea.

The haddock as a weight producer is improving in efficiency during the first years of life. If only half of the unmarketable sizes destroyed at present escaped and were captured one year later the same weight of haddock but of greatly enhanced market value would be obtained. Since the numbers of brood present in a particular area have no marked effect on the mean rate of growth a haddock just under marketable size alive in the sea is of greater potential value than a dead one.

The process of elimination is the same whether the brood is rich or poor in numbers, and during the period under review there have been three rich broods when thinning might have had some small effect on the rate of growth of the uncaptured stock, but there have also been five meagre broods when thinning undoubtedly accentuated the depression arising from the initial deficiency in numbers.

The present method of exploiting the haddock population of the North Sea plateau is wasteful, squandering the riches of years of good brood survival and accentuating the depressing effects following years of scanty brood production.

Fluctuations in brood production are inevitable, but by postponing the time of capture of the young growing stock the average age of the marketable stock would be raised and the fishery, dependent on a larger number of age classes, would thereby have greater stability.

Dr. E. S. Russell.

The overfishing problem now appears to us as much more complicated than it did in the past. We have learned to regard it as a complex problem of ecology. The main factors affecting the abundance and average size of the fish composing a stock are (1) intensity of fishing, (2) the supply of young fish, (3) rate of growth. All these factors are subject to variations. Theoretically there is for each state of a stock an optimum intensity of fishing, and an optimum size at which to begin capture. Our knowledge is so far insufficient to state what these optima are, and they obviously vary according to conditions. Wasteful destruction of fish is to be avoided, particularly the destruction of fish just under a commercially valuable size. Our aim should be to obtain every year the maximum yield which is compatible with maintenance of stocks at a steady productive level.

Symposium on *Insects and Human Welfare*:—

Dr. A. D. Imms, F.R.S.—*Insect Behaviour in relation to Control Measures.*

Mr. J. C. F. Fryer.—*Practical Achievements in Agricultural Entomology in Britain.*

In England the modern era in agricultural entomology (including horticultural entomology) began some twenty-five years ago, and the subsequent period has been

fruitful in practical achievement as well as in entomological discovery. Practical achievement implies that entomological discovery has led to the elaboration of methods of dealing with injurious insects in the field and that these methods have actually been adopted by the sections of the industry concerned. This does not mean that the methods are completely satisfactory, but that they are sufficiently so to have justified their wide use by practical men. The extent of such achievement may be gauged by reference to some sixty species of insects, which are among the most injurious in agriculture or horticulture. In the agricultural section, methods of control have been adopted successfully in some 20 per cent., tentative or partial measures in some 23 per cent., but there has been little practical advance in the case of 57 per cent. In the horticultural section, new measures of control have been adopted by the industry with success in 53 per cent., tentative or partial control measures are in use in connexion with 33 per cent., and in only 14 per cent. has little or no practical advance been made.

The reason for the greater success in horticulture is economic, the horticulturist being able to adopt measures too costly for the farmer.

The types of control measure may be classified as (1) *Direct*, in which the insect is destroyed by a direct attack made by chemical or other means, (2) *Indirect*, in which the environment is rendered unfavourable to the insect, either by cultivation, the use of resistant varieties of crop, or by the encouragement of natural enemies.

Of the control measures successfully adopted in the cases analysed above, it is found that 91 per cent. are of the direct type, the majority involving the use of insecticides.

It is deduced that while indirect measures may as a result of research become predominant in the future, the entomologist now in practice among farmers and growers must rely chiefly on direct measures.

Finally, not least among the achievements of agricultural entomology in England is the fact that the entomologist has largely overcome the prejudice formerly held against him by farmers and growers, and that he now finds in them both keenness for assistance and readiness to collaborate in experimental work.

Dr. C. B. Williams.—*The Trinidad Sugar-Cane Froghopper and Co-operation in Entomological Research.*

Sugar-Cane Froghopper known sixty years in Trinidad; most damage in last twenty-five years. Life cycle known. Usually three broods, eggs in soil and trash, nymphs suck roots, adults suck leaves. From point of sucking of adult damage spreads for several weeks but no trace of virus; requires co-operation with plant physiologist. Damage varies from year to year according to climate, especially rainfall, and from place to place according to soil, especially texture and chemical composition. Problems for co-operation with meteorologist, geologist, soil chemist and soil physicist.

Also damage varies according to variety and age of cane and agricultural conditions such as drainage; problems requiring co-operation of botanist and agriculturist. Damage, and particularly recovery, closely related to root fungus infection, requiring co-operation of mycologist.

Soil relations are particularly interesting. Always commoner on clay, heavy, acid, wet soils with lower pH, but spreads to lighter, less acid, soils in bad years. Recently experiments have shown that, when given choice, insects select heavy soil for egg-laying.

Control methods tried or in progress :—

(1) *Trapping by lights.*—Only catches males.
(2) *Trapping by nets.*—Only possible for very short period each day.
(3) *Liming.*—Very expensive, but under investigation, and most likely to produce permanent result.
(4) *Killing adults and nymphs with* $Ca(C.N)_2$ *dust.*—Under investigation; appear to be difficulties in getting suitable duster.
(5) *Local parasite complex.*—Under investigation. *Mongoose* sometimes considered as cause of damage by killing off birds, lizards, &c., but doubtful if correct. *Green muscardine fungus*, parasitic on froghopper, too dependent on climatic conditions.

(6) *Search for outside parasite.*—Investigations 1916–1918 showed many species of grass-feeding froghoppers in Central America each with very small range, but known parasites range from Mexico to Brazil. More recent investigations equally unsuccessful. More promising to go further afield, *e.g.* to Africa, but with native pest, old crop and continental fauna problem more difficult.

Dr. Ll. Lloyd.—*The Passing of Insect-borne Diseases from Britain.*

Britain has suffered in the past from serious epidemics of bubonic plague, typhus, relapsing fever and malaria. There is nothing in the meteorological records to indicate that any graded climatic change is responsible for their more recent absence.

Plague was dependent on insanitary habits in towns and fragile houses, which, respectively, afforded food and shelter to the black rat and allowed it to multiply greatly. With change in these conditions enforced by legislation the black rats were reduced in numbers and plague ceased. Rapid reduction in numbers of the human flea is still in progress.

Typhus and relapsing fevers, the louse-borne diseases, had their foci especially in gaols and insanitary camps, and when these were reformed the continuance of these diseases was due to the vagabond class. Outbreaks followed especially on famines, and the ability of the British people in recent times to make up any deficiency of food by importation has been an important cause of their cessation. Social measures which have tended to reduce the condition of utter vagabondage have helped in this.

Malaria was widespread in England and Scotland up to 1800, and since then in several outbreaks a tendency to restriction of its northerly range is noted. It is suggested that an old strain capable of transmission at a somewhat lower temperature than present-day strains has died out, probably owing to very cold years. Other factors which have or may have played a part in the reduction of malaria are better treatment of the disease, increased drainage through improved farming, possibly also a change in the feeding habits of *Anopheles* such as Hackett and Missiroli think has abolished malaria from parts of Italy.

Dr. P. A. Buxton.—*Studies in the Biology of Anopheles Mosquitoes.*

The control of malaria rests partly upon reduction in the number of *Anopheles*; we no longer attempt to control all species indiscriminately, for, of the 130 species which inhabit the world, only about 30 are incriminated as carriers of the *Plasmodium*. Inasmuch as each species chooses a particular type of water as its breeding-place, the problem of control is narrowed. In selecting a place in which to lay eggs, the females of one or two species appear to be limited by factors which are easily perceived; for instance, one species seems always to seek out cold waters, and a few select water which is very salt. But the behaviour of most of the species is very difficult to understand, and it seems to be influenced by several factors. The students of these problems work very much in the dark; they observe that one species breeds in tiny trickles of water in jungles, and another in rice fields. But when they try to analyse this behaviour they can only hope to discover the determining factors by a hit-and-miss method; they collect and tabulate a large amount of information about acidity, light, dissolved oxygen, nitrates, &c., and then endeavour to establish a correlation between the observed distribution of a species of *Anopheles* and one of these factors.

The matter might be approached experimentally, eliminating all variables except one pair, and observing the female mosquitoes' choice under these conditions. It is difficult to do this with *Anopheles* which is an intractable insect, but one can easily experiment with *Aëdes* (*Stegomyia*) *argenteus*, a species of mosquito which lends itself to experimental study. It is possible that understanding of the behaviour of *Anopheles* might come more rapidly if attention were first given to unravelling the factors which influence *Aëdes* in choosing a place in which to lay eggs.

Studies on the biology of *Anopheles* tend to narrow the problem of control by defining the objective. There is also the possibility, which has been discussed for many years, that if we possessed more detailed knowledge we might be able to alter the environment, rendering a particular breeding-place unattractive to a certain species of *Anopheles*.

SECTION E.—GEOGRAPHY.

Thursday, September 24.

Dr. VAUGHAN CORNISH.—*The National Park of Northumberland, a Shrine for Hadrian's Wall.*

The National Park Committee in the Report presented to Parliament, April 1931, record their opinion that:—

'... The Authority will no doubt desire to give effect as far as their funds will permit to Dr. Cornish's principle of including at least one supreme example of each principal type of scenery' (page 20).

In England the selection of supreme examples is comparatively simple in the case of mountain peaks, sea cliffs and river gorges, but in the great tract of moorland from Kinder Scout to the Scottish Border it is difficult to select any one area sufficiently pre-eminent in natural beauty to ensure the permanent popularity necessary for success as a National Park. It is, however, manifestly desirable that the principal type of wild scenery in the North of England should be represented. I submit that the problem of selection would be best solved by giving the status of National Park to the moorland which surrounds the finest part of Hadrian's Wall, where, between Chesters and Gilsland, this unrivalled historic monument runs for seventeen miles across the chine of Britain. Only by the preservation of this landscape in the wild state can the purpose and significance of the northern bulwark of Mediterranean civilisation be pictorially displayed.

The value of the Park as a place of resort would be especially great for those societies and educational organisations whose work the National Park Committee desires to foster (*vide* Report, p. 11). It would also attract foreign visitors from countries which share with us the heritage of Rome.

The proposed Park is set within an extensive area of wild scenery in which rivers, woods and waterfalls, and far-reaching views, provide attractions for ramblers; it is accessible from the industrial districts of Scotland; and it is unique in its historic appeal to all three constituent nationalities of Great Britain.

DISCUSSION (The Right Hon. Sir HALFORD J. MACKINDER, P.C.).

Dr. H. R. MILL.—*Geography at the British Association: a Retrospect.*

Many scientific men shared in the foundation of the Royal Geographical Society in 1830 and of the British Association for the Advancement of Science in 1831; Sir Roderick Murchison taking a leading part. In 1834, when the early informal Committees of the British Association were organised as Sections, Murchison was mainly responsible for the creation of Section C, 'Geology and Geography,' or, as it was often termed, 'Geography and Geology,' and at his instance from 1838 to 1850, 'Geology and Physical Geography.' In practice the geologists ran the Section, the travellers, explorers and colonial officials who then represented the geographical side, being held of little account.

In 1851, when the old Section E—Medical Science—had been abandoned on account of the growing strength of the British Medical Association, Murchison secured the divorce of Geography from Geology and its union with Ethnology as a new Section E, which was known until 1868 as 'Geography and Ethnology.' From 1869 onwards Section E has remained 'Geography' without addition or qualification. Under Murchison's direction the Section outdistanced all the others in popularity, the famous travellers and explorers whom he induced to attend drawing immense audiences.

The changes of designation hint at the gradual development of the conception of Geography as an independent science combining the physical and the human elements as aspects which were at first in contrast, but gradually combined to embrace the description and explanation of all the phenomena of terrestrial distributions both static and dynamic. It cannot be claimed that this Section was the main factor, but it undoubtedly supplied an important stimulus towards the development of Geography as now understood—a science based on mathematica conceptions of

form, position and motion, rising through physical explanations of the relief of the Earth's crust and its influence on the movements of the fluid envelopes to the consequent control of biological distributions, and culminating in the complicated actions and reactions of man and his environment.

In Murchison's time, and for a decade after his death in 1871, Section E was little more than a prolonged summer meeting of the Royal Geographical Society. The President was usually a leading member of the Council of the R.G.S., the Recorder was almost always either the Secretary, Librarian or Map Curator of the Society and the Committee contained few active outsiders. They organised the work of the Section, so far as there was any organisation at that time, and brought with them the huge wall-maps of the continents that used to grace the evening meetings of the Society in London. Many of these early meetings throbbed with dramatic thrills. When the Section assembled at Bath in 1864 to listen to what was expected to be a bitter controversy between Burton and Speke on the discovery of the great lakes of Africa, the tense audience heard instead the news of Speke's tragic death from a gun accident on the previous afternoon. No less dramatic was the first appearance of H. M. Stanley before an English geographical audience at the Brighton meeting of 1872, when he could scarcely conceal his chagrin at what he considered the humiliation of having to describe his finding of Livingstone to a provincial audience in a sea-side resort instead of to a distinguished gathering in the largest hall in London.

The original papers read to the Section were habitually published in the *Proceedings* of the R.G.S. up to the time of the foundation of the Royal Scottish Geographical Society in 1885—the year in which the author attended the first of twenty-five consecutive meetings of the Association. The Secretariat of the Section then received the permanent addition of a member of the staff of the Scottish Society and many of the papers read have since been printed in the *Scottish Geographical Magazine*. Progressive developments ensued when, following the initiative of the R.G.S., lectureships or chairs of Geography were founded in British Universities, and the academic element in the management of the Section has now come to predominate over all others, the Recorder and most of the Secretaries since 1914 having been holders of University posts and independent of the Societies. This change coincides with the growing recognition of the scientific principles of Geography and with the great increase in the number of persons professionally engaged in geographical teaching, surveying and map-production.

The 78 presidents of Section E since 1851 included 16 surveyors, cartographers or explorers, 13 University teachers of Geography or officials of the R.G.S., 13 whose life was mainly devoted to Geography but not as a profession (of these, Murchison counts as 7), 11 soldiers or sailors, excluding Royal Engineers and hydrographers, 10 diplomatists or administrators, 8 men of science not geographers, and 7 archæologists or classical scholars. The change of presidential qualification with time stands out in the comparison of two of these groups for the 78 years and for three consecutive periods, the figures being given as a percentage of the whole number of presidents in each period :—

	1851–1930.	1851–1884.	1885–1916.	1919–1930.
Soldiers and sailors, excluding Royal Engineers and hydrographers .	13	15	19	0
University teachers of Geography and R.G.S. officials . . .	17	0	22	50

The author has followed, and to the best of his ability assisted, in the evolution of mere description of exploration and surveying into the exposition of problems of geographical research which throw light equally on the conditions of the home-land and of remoter regions.

Committees subsidised by the Association on the nomination of the Section have made important contributions to the exploration of the East African Lake Region, the determination of comparative level of land and sea in and around the British Isles, the survey of Palestine, and the exploration of New Guinea, the Indian Ocean and the Polar regions, to mention only those purposes for which more than £200 was granted.

Geography is so interwoven with the fabric and the functions of the world, both in its physical and in its human aspects, that much of the geographical work of the Association has been and still is done in collaboration with other sections, and the habit of holding joint meetings of Section E with Section A in such matters as map-projections, climatology and tides, with Section C in the study of land-forms and recent surface changes, with Sections D, K and M in investigating the geographical distribution of animals and plants, with Section L on Education and with Sections F, G and H on economic, ethnological, and engineering matters has made possible much of that welding of individual specialists into a true brotherhood of science which it was the earliest, and is still the most important, function of the British Association to promote.

Sir E. John Russell, F.R.S.—*Soil Resources of the Empire: a suggested Survey.*

In recent years the agriculture of the Empire has undergone a fundamental change. In its earliest stages each region was self-supporting; the agriculture was therefore general, with, however, export of some particular commodities such as grain, wool or skins, that could be easily carried. Three factors have caused the great change: improvement of transport, the development of refrigeration, and the development of plant breeding, which now allows considerable adjustment of crop to environmental conditions. In consequence, each region now tends to concentrate on the agricultural commodities it can produce best, and to rely on importations for all other commodities; the purpose is no longer to be self-supporting.

Modern agricultural development has therefore become a study in adaptation of crops and animals to geographical conditions. The geographical conditions are the soil and the climate; the soil, however, is so profoundly affected by the climate that under similar climatic conditions the general characteristics of the soil are the same. The effect of topographical conditions, particularly elevation, is also climatic. Climate, therefore, is the most important single factor determining the type of agriculture. In general, under similar climatic conditions, similar agricultural systems can be adopted and similar products obtained, the plant-breeder making the necessary small changes. From the rainfall and temperature maps it is possible to make a good forecast of the probable agricultural products. Broadly speaking, they are as follows:—

Tropics: Plants.—Oil, fibre, alkaloids (including tea, coffee, cocoa), sugar cane.
Animals.—Grazing cattle for low-grade beef except where insects drive them out.

Sub-tropical. (*a*) *Moist or irrigated:*
Plants.—Fruits, *e.g.* citrus, bananas. Rice.
Cotton. Lucerne or certain clovers, grass.
Animals.—Dairy cattle (fed on lucerne). Bullocks.
(*b*) *Drier:*
Plants.—Maize: if there is a cool season; wheat, barley.
Animals.—Grazing sheep for wool, especially Merinos.

Temperate. I. Continental Type. (Hot summers, cold winters.)
Arid. Plants.—Wheat, barley.
Animals.—Sheep for wool and low-grade mutton (Merino or Persian crosses).

Humid. The above and, in addition,
Plants.—Sugar beet, tobacco, maize, lucerne, clover of various kinds, grass.
Animals.—Beef, dairy cattle, pigs, poultry.
II. *Insular Type.* (Cool summers, mild winters, rain in all seasons.)
Crops.—All British fruits. Potatoes, all British vegetables. Wheat, barley, oats (Rye in colder climates), grass.
Animals.—Dairy cows. Sheep (especially for lamb production). High quality beef cattle, pigs, poultry.

We expect to find, then, the following products :—

(1) *Coastal regions : Cool.*—Grass, dairy products, lamb production.
Warmer.—Fruit : apples in cooler, plums, pears in warmer, grapes, peaches in still warmer conditions.

(2) *Inland drier regions : Cool season.*—Wheat, barley, increasingly combined with sheep for wool or with tilled crops (potatoes, forage) for dairy farming where fresh (not brackish) water is always obtainable for the animals.
No cool season.—Maize combined as above.

(3) *Irrigated regions.*—Valuable crops, *e.g.* cotton, citrus, grapes for raisins ; rice, lucerne and dairy cattle. Human food crops only in crowded countries : India, Ceylon.

(4) *Tropics.*—See above.

For most parts of the Empire the British market dominates the demand. The inquiries of the Empire Marketing Board and of the Ministry of Agriculture show approximately what this demand is ; there is great need now for a survey of the soil resources of the Empire, which would show how much land is immediately available for these various products and how much more could on present knowledge be made available should economic conditions justify this course. The Geographical and Agricultural Sections might well consider the possibility of collaborating with the Imperial Soil Bureau in making a preliminary survey ; the work, once started, would readily be developed.

Afternoon.

The Right Hon. Lord Lugard, P.C., G.C.M.G.—*Africa in Transition.*

It is appropriate that the changes which have taken place in Africa in the last half-century should be considered at a joint meeting of the Geographical and Anthropological Sections. From the standpoint of political geography great changes have taken place in the temperate zones in the north and the south of the Continent. In the former Egypt has become independent and Italy and France have consolidated their possessions in Libya, and in Tunis, Algiers and Morocco ; in the latter the Union of South Africa has taken shape as a Dominion, and the two Rhodesias have been added to the Empire. Dividing the half-century into three periods we find great changes in Tropical Africa, viz., the regions between 17° N. and 17° S. Lat. In the first seventeen years it was, with the exception of Abyssinia, partitioned between the powers, boundaries were demarcated, and a great campaign took place against the slave trade. The foreign governments were engaged in establishing law and order. In the second era from 1898 economic development by means of railways was pushed forward, defence forces created, and slave trading finally suppressed. Trade increased rapidly. The final period saw the elimination of Germany, the creation of the mandate system and a definite policy of trusteeship for the natives. Communications were improved by the advent of motor vehicles and aeroplanes.

The new exploration of Africa lies in research regarding native institutions and the adaptation of methods of government. This became an international interest largely owing to the mandate *régime.* Under that system supervision is exercised by the Mandates Commission until the people can stand alone. The advent of Europeans into the interior has naturally had a great effect on African modes of life and thought. The abolition of tribal war and slave raiding has enabled natives to travel freely, and contact with white men has disintegrated native society. In particular, it has introduced the conception of individual as opposed to communal responsibility.

There are two schools of thought in regard to the method of administration of native races, each varying in application in different regions. The first is that of ‘ Assimilation,’ in which the object is to supersede native rulers and institutions by those of the Suzerain Power and to Europeanise the African. The second, known as ‘ Indirect Rule,’ aims at building on African foundations and gradually improving them. This involves a close study of native tradition and custom, on the one hand, and on the other instruction of native rulers in civic responsibility, the delegation of

authority and the control of public funds. The system has been criticised, but compares favourably with its alternative.

The time has now come to consider the ultimate development of this system by central advisory native councils in the political sphere, and how far it is possible to adapt religious teaching to African conceptions of the spirit world by missionaries and education departments.

The effect of contact with white settlers in East Africa and the problem of the different systems of political development of the two races affords a special field of study which is being explored by various research institutions. This should throw new light on the great changes which are taking place in African life and thought and the organisation of African society.

Friday, September 25.

PRESIDENTIAL ADDRESS by the Right Hon. Sir HALFORD J. MACKINDER, P.C., on *The Human Habitat* (see page 96).

Prof. GRIFFITH TAYLOR.—*The Geographer's Aid in Nation-planning.*

The writer was for more than a dozen years the only professional geographer in Australia. The paper deals therefore with the growth of geographical knowledge in a new land and with its bearings on national problems in Australia. The science was founded mainly on the sound geological training given by two men, Sir Edgeworth David in Sydney and Prof. J. W. Gregory in Melbourne. The latter produced perhaps the first scientific geography (a study of Victoria) in 1903. The writer was appointed lecturer in Economic Geography at Sydney University in 1907, and held allied positions most of the time thereafter until 1928.

In those early days there was available a large fund of information as to the rainfall of Australia and a general knowledge of the geological structure of the continent. But details of the factors controlling settlement had not been worked out. The address points out the various lines of research adopted by the writer in attempting to account for the large area of 'Empty Australia.' In 50 per cent. of the continent there lives less than one per cent. of the population. Three major factors account for the empty character of this large area. The very restricted *season* of rainfall, the great *unreliability* in much of the area, and lastly the *edaphic* features (mainly the distribution of vegetated dunes—chiefly controlled by geological considerations) are very important factors in our problem.

An examination of the growth of population shows very clearly that the Australian is unconsciously exemplifying the geographical controls. Of recent years there has been either no growth or actual diminution in much of the tropical and arid parts of this great new land. Method of comparing climates by new graphs (called hythergraphs) are illustrated. These show that large areas in north and western Australia have very similar climates to those in the south of India, in the northern Sahara and in inland Nigeria. Yet many Australians believe that important white settlement is possible to-day in such Australian areas.

A second great problem is that of 'White Australia.' The writer has always had much sympathy with the endeavours of a young nation to keep as free as possible from racial complications. At the same time he felt it proper that Australians should know something of the racial and social data of such peoples as the Chinese and Japanese. This led him to many years of ethnological research, in which he came to the conclusion that probably the peoples of the centre and east of Eurasia are a later development of the human race than most of those in western and southern Europe. This view was naturally not palatable to many good Australians.

Further research of a comparative nature enabled me to give a first approximation to future white settlement in the empty areas of the world. Using Europe to-day as a criterion with 500 millions of people, it seems likely that North America (if desired) could support 700 millions, while Australia could support some 60 millions. This gives us a clue to the relative values of two great centres of future white settlement. The meaning of the word 'Desert' roused much controversy, mostly ill-informed it seemed to the writer. As the writer has shown, about 42 per cent. of Australia is arid (though half of this is fair pastoral country in good seasons); another 30 per cent.

is good pastoral country ; about 3 per cent. (in Queensland) is well suited for tropical agriculture ; finally, some 21 per cent. (including much rugged country) has a climate suitable for close white settlement. Indeed, no region in the world in the writer's opinion offers such a field for 10 *million* settlers as the inadequately utilised agricultural part of Australia. My studies have shown that no arid or tropical lands resembling Australia's *empty spaces* have been settled by any noteworthy white population. My counsel to Australian statesmen has always been : 'Break up the large alienated holdings, but leave the empty spaces to the pastoralist for whom nature meant them. Tackle the tropical north and the arid lands later—much later—and leave the desert to its loneliness.' Had this advice been followed more closely it would have eliminated a number of hopeless projects, and would have gone some way to preventing the present deplorable crisis in Australia.

Prof. W. Werenskiold.—*Variations of Glaciers and Climate in Norway.* (Slides.)

The Norwegian geologists, P. A. Öyen and J. B. Rekstad, organised a systematic survey of glacier oscillations from the 'nineties of last century. The distance was measured between the front of the glacier and some fixed point, and much material has been collected in this way.

Prof. H. W. Ahlmann of Stockholm has made some good surveys and investigations of glaciers in the Horung group.

Since 1928 Mr. Adolf Hoel and the lecturer have surveyed the glaciers near Galdho tind with the help of the Nansen fund for scientific research. The mapping is made with stereophotogrammetric and simple photogrammetric methods ; a base line has been measured, and a trigonometrical net has been established, connected with the trigonometrical points of the official survey.

As yet no measurements of velocity, melting, &c., have been made.

In this region, among the highest summits of Norway, the glaciers lie in separate troughs, divided by sharp ridges, and each glacier is an economic unit. The status depends on the balance between income (snowfall) and expenditure (melting), and a closer study of the behaviour of the glaciers will certainly lead to interesting results.

Till the beginning of the eighteenth century the Norwegian glaciers were even smaller than now, but during the years 1720 to 1740 they burst forth with great violence, damaging or destroying several farms. This advance has left very distinct moraines. In the years 1740–42 the barley did not ripen, and a general famine resulted, a token of extraordinary climatic conditions. The glaciers maintained their great size for almost a century, but later on they have, on the whole, retreated, with short periods of advance after years of heavy snowfall (1906–1909).

Our work does not cover a period long enough to ascertain whether the glaciers are growing or not.

Monday, September 28.

Symposium on *Geographical Problems of the Earth's Crust.* (Prof. J. W. Gregory, F.R.S. ; Mr. A. R. Hinks, C.B.E., F.R.S. ; Prof. A. Holmes ; Dr. H. Jeffreys, F.R.S. ; Dr. G. C. Simpson, C.B., C.B.E., F.R.S. ; Dr. de Graaf Hunter ; Dr. J. H. J. Poole.)

Mr. A. R. Hinks, C.B.E., F.R.S.

The problems lie in the boundary territory between Geography and the other sciences whose names begin with Geo-. One hears complaints that contending parties do not always play the game, especially in neglecting mathematical arguments. How should geographers confront mathematics they cannot refute, but against whose conclusions they can produce evidence ?

Mathematical processes are sometimes misapplied, as in Hayford's determination of the 'depth of compensation' and Wegener's argument from the frequencies of heights. They are sometimes based on insufficient premises, as Kelvin's determination of the Earth's age. Mathematical theory enables us to arrange facts in an orderly way to confront it, which is a principal duty of geographers. But mathematicians

may insist that geographers and geologists and climatologists do not simply ignore them, as Joly and Simpson have done. And astronomers may resent a geologist's assumption that the earth captured the moon in Cretaceous times. Such people play fast and loose with every science but their own.

The Rules of the Game are comprehended in the proposition : No man may adopt a principle contrary to the best opinion in another man's subject. He should be encouraged to marshal evidence against the principles of others, but must give time for the defence and not anticipate the verdict. Mathematicians maintain firmly that any considerable wandering of the polar axis in the body of the earth is impossible, and any considerable drift of the whole crust unlikely because there is no sufficient force to cause it. Differential drift of continents is even more difficult to allow. Geographers and geologists must, therefore, refrain from making contrary assumptions as a matter of convenience, and be content with accumulating evidence, carefully arranged in regard to the various mathematical objections.

When the mathematician is able to allow enough thermal contraction to explain the Alps, the geologist is apt to demand immediately eight times as much to explain Central Asia, and to declare the contraction theory bankrupt because he cannot be allowed it. Thermal contraction must be allowed to play a large part in mountain building, and contraction with decrease of flattening something more. It remains to be seen if the two are insufficient.

The theory of isostasy according to Bowie implies some uniform depth at which compensation is complete. Dutton originally used the word to denote conservation of the earth's external figure against the processes of denudation and sedimentation. The doctrine of compensation at a definite depth is Pratt's. Airy's doctrine of the 'roots of the mountains' is now definitely adopted by Jeffreys, and the basis of his mathematical reasoning. The word 'isostasy' is so closely bound up with the abandoned theory of Pratt that it should be dropped. Even the idea that the earth's crust rapidly adjusts itself to compensation seems now to be doubtful from the discovery by Vening Meinesz of a belt of strong negative anomalies which coincides with the modern equivalent of Wallace's line and is, therefore, a very old feature.

Afternoon.

Discussion of communications at Morning Session.

Tuesday, September 29.

Dr. S. W. Wooldridge.—*The Geomorphology of the London Basin.*

The paper included a brief introductory survey of the evolution of the land-forms of the region, that is, its geomorphology strictly so called. Greater stress will be laid on the physical controls operative in water-supply, settlement and in the development of the conurbation of London itself.

The London Basin is an asymmetrical syncline, pitching gently eastwards and drained by a longitudinal consequent stream and its tributaries. The major features of its present relief and structure bear a recognisable resemblance to those of earlier geological times, from which they may be regarded as inherited. The later (post-Miocene) stages of planation and dissection have followed a regular and orderly course which has imposed a definite scenic plan on the area.

The London Basin is unique among the Tertiary basins of Europe in the number and variety of contrasted deposits which lie in geographically significant juxtaposition. Geographical contrasts are accordingly clearly marked, and regional subdivisions are readily made on a basis of soil-type and availability of water. Justification for the subdivisions proposed is found both in the history of post-Roman settlement and in the present utilisation of surface within the area.

Mrs. H. Ormsby.—*The Geographical Factor in the Growth of London as shown by Population Maps based on early and recent Census Figures.*

The population figures for the London area, as recorded in the report of the 1841 census (the first systematic census carried out under the control of a central office), have been plotted, parish by parish and ward by ward, on the 6-inch map. The

geographical distribution has been determined by the utilisation of contemporary maps, particularly Cruchley's map of 1843.

The general result bears a striking resemblance to the distribution conditions to-day, in spite of the much smaller area covered. That is to say, a thinning of population in the City, a crowding on the outskirts, and, radiating from this zone of greater density, a ribbon development along the arterial roads and new circular roads. Development of estates lying between the radiating high roads marks the beginning of a fresh cycle of rapid growth.

The 678 acres of the City had a resident population of 123,500 in 1841 as against 13,700 in 1921, but depopulation was going on actively owing to reconstruction, particularly in the neighbourhood of the Bank.

Geographical conditioning is clear in the close adherence to the valley gravel areas, except along the river banks and in the neighbourhood of the docks, and in the general avoidance of the alluvial and London Clay areas, which were used mainly, where built on, for cemeteries, small-pox hospitals, asylums, workhouses, refugee settlements and railway termini.

Apart from actual extension of building, the outstanding feature of the modern distribution, as shown on a map of Greater London based on the 1921 census, and prepared in a similar way, is the spread of population on to the London Clay areas, made possible by improved drainage, sewerage and water supply.

Prof. J. Schokalsky.—*New Hypsometrical Maps of the U.S.S.R.*

Reliable hypsometrical maps of this large territory are very necessary for many branches of science; but since only relatively small parts have been the subject of regular topographic surveys, the preparation of the maps has involved an immense amount of research carried on in the last 25 years. The maps for which the author is responsible and which were exhibited are as follows:—

1. *Hypsometrical Map of the European Territory of the U.S.S.R.*, 1 : 2,500,000, in six sheets, of which two of the three western sheets are presented. Fifty years ago a hypsometrical map of European Russia south of the 60° parallel was constructed by A. Tillo. On this map the relief depended on about 51,385 altitudes. For the new map a very much greater number have been determined or collected and checked. Thus, in the sheet including Leningrad, Moscow and Kieff, some 50,000 points were utilised; moreover, this sheet includes most of the strip of regular surveys along the western border of the U.S.S.R. On the other hand, the sheet to the north—mostly Karelia and Archangel—has only 570 reliable altitudes. These two sheets are now ready for printing; and the third of the western sheets—including the Black Sea coast and western Caucasus—is now drawn.

2. *Hypsometrical map of the whole of the U.S.S.R.*, 1 : 12,000,000. This map was first published in 1914 and it was used by Bartholomew in preparing the map of Siberia on a smaller scale in the 'Times Atlas.' The edition now presented has been completely revised for the Climatological Atlas of the U.S.S.R. now in preparation. It offers the best picture of the relief which can be made with the existing data.

Capt. J. G. Withycombe.

The Ordnance Survey, from its birth, has aimed at mapping all natural and artificial features up to the limit possible at the scale in question. Our maps are, then, general purpose maps, and although they offer an excellent basis for training in accurate and scientific method generally, they do not illustrate fully any one particular geographical aspect.

Amongst our recent publications are two historical maps—Roman Britain and Seventeenth-century England. These are good examples of special purpose maps. Geological maps are the most widely used and best known of the 'special' class.

It is curious that, whilst in the great war no geographical service issued more varied types of 'special purpose' maps than did we, in peace Great Britain stands behind the Continent in the cartographical illustration of her resources and development.

The present series aims at meeting this reproach in one particular direction, and may possibly serve as a model for other similar developments.

The collection will provide a cartographical basis for the scientific study of our own

(i) Land Forms.
(ii) Settlement Forms in relation to Land Forms.
(iii) Special Settlement Forms (cities, ports, &c.).
(iv) Land Utilisation (Pastoral and Agricultural Forms).

The complete work will thus consist of four series each containing 12–15 sheets. Each series will have an introduction, and there will be a general introduction to the work as a whole.

Each individual sheet will be made up from Ordnance Survey maps as ordinarily published, but the ' map-area ' will not necessarily coincide with any individual sheet. The areas are selected as being ' typical,' each ' map-area ' representing a single type feature, or simple combination of type-features. Thus only so much of the topography shown on the map will be printed as is relevant to the typical forms under discussion. The map-sheets, in fact, will be ' analytical abstracts ' of published Ordnance Survey maps, though they will embody certain special features and special ways of emphasising, cartographically, the points at issue.

Besides the ' map-areas,' each published sheet will include an explanatory text (with bibliography) and illustrations (views, sections, diagrams, &c.). Each sheet will thus present a scientific analysis and exposition of some typical form of the geographical landscape.

The work is being compiled by a committee of university geographers aided by contributors from various universities and colleges, and it will supersede the foreign works of a similar nature which have so far been available. The Ordnance Survey will reproduce.

Wednesday, September 30.

Prof. J. H. Wellington.—*Land Utilisation in South Africa.*

Dr. C. Fenner.—*The Structural and Human Geography of South Australia.*

The State of South Australia is bounded on three sides by mathematical lines: on the west by longitude 129° E., on the north by latitude 32° S., and on the east by longitude 141° E. The southern boundary is an irregular and considerably broken coastline extending obliquely from 32° S. south-easterly to 38° S. The State thus lies marginally between the permanent high pressure trade wind zone on the north, and the belt of circling highs and lows that marks the polar front along the south. For these reasons the northern continental portion constitutes an arid, uniform, and sparsely settled area of 295,000 square miles, with a rainfall of 10 inches or less per annum, mostly summer and monsoonal. The southern portion open to the oceanic influences from the west is a varied and fertile area of 85,000 square miles, with reliable winter rains from 10 inches to 45 inches according to locality.

Just as the State is, from the climatic point of view, marginal between two highly contrasted belts on the north and south, so, from the structural point of view is it marginal between two equally contrasted geological regions on the east and west. The western portion is dominated by the great stable Westralian shield of ancient crystalline rocks, with uniform physiographic features; on the east it enters into the less stable, younger, and more varied rock types and structures of Eastern Australia. Between the two is a broken zone or ' shatter-belt,' heavily faulted, with much differential uplift and depression, giving us the mountains, plains, and gulfs that dominate the geography of South Australia and, indeed, make possible the existence here of a relatively closely settled province.

In Pre-Miocene times the whole area of the State, which then continued further to the southward, became a very complete peneplain. During the Miocene the southern portion of this level area was submerged by the sea; this was followed by uplift in the Pliocene, and in Pleistocene to Recent times there was considerable block-faulting, some warping and sagging, and differential movements along fault lines (the ' Kosciusko Epoch '). The most important economic areas are those to the south of the Gawler-Olary upwarp.

To this area in 1836 came the small body of pioneers who founded the province, and these have steadily increased to 600,000 at the present day. A study of the

growth and distribution of the population during those 95 years shows the powerful influences exerted by the undermentioned factors in the order named :

(*a*) Climatic, chiefly the amount, incidence, and reliability of rainfall.

(*b*) Soils, varying from the brownearths of the southern ranges to the silts of the river flats, the light sandy soils of the Mallee plains, and the mulga and salt-bush areas of the arid north.

(*c*) Transport and communication, modified by climatic and other factors, hampered by the roughly East-West barrier that marks the southern margin (10″ isohyet) of the arid area, and by the four north-south barriers that occur in the more populated portion : Mt. Lofty Ranges, Murray River, Spencer Gulf, and Gulf St. Vincent.

(*d*) Productions, partly determined by climatic and soil conditions, and partly by remoteness from markets, requiring primary products that will keep well and repay high transport costs ; in this case, mainly wheat, wool, wine, and dried fruits.

Under the pressure of the requirements of a growing population, mainly dependent upon primary production, and influenced by the physiographic and climatic conditions outlined, there has been built up a unique scheme of water supply, a widespread railway system, a very complete series of coastal ports, and the commencement of a system of excellent arterial roads. The camel of the interior has given way to the motor car ; the railways are being hard pressed by motor transport, and there is a slow but definite development of aerial transport.

The progress of settlement has been : The cultivation of the fertile and well-watered Adelaide Plains and the valleys of the nearer ranges ; the pastoral occupation of the more remote areas, including the mulga and salt-bush country, wherever water for stock (water-holes, artesian bores, wells, rock-holes, &c.) was available ; parallel with this came the discovery of rich copper deposits at Kapunda, Burra, and Wallaroo, with consequent railway construction and the 'opening up' of those localities to agriculture ; then, with the development of artificial manures, special wheats, and a farming technique suited to the prevailing conditions, wheat farming spread to the Middle North, Yorke Peninsula, the Murray Mallee, and Eyre Peninsula. The growth of the capital city has more than kept pace with the development of the country.

Mr. LAWRENCE J. BURPEE.—*The Geography of Canada.*

An attempt, with the aid of lantern slides and a motion picture in the form of an animated map to put into concise and understandable form an account of the geography of the Dominion from Halifax to Victoria. The animated map illustrates more specifically the history of discovery, the contributions of each explorer being added until the Atlantic, Pacific and Arctic coasts are complete as well as the physical features of the interior.

SECTION F.
ECONOMIC SCIENCE AND STATISTICS.

Thursday, September 24.

Mr. R. R. ENFIELD.—*The World's Wheat Situation.*

The chief changes in distribution of the wheat area as compared with pre-war years. The redistribution of international trade in wheat and flour. The course of wheat prices in the post-war period ; the deviation in the movement of wheat prices from the movement of wholesale prices ; the factors contributing to the heavy fall in wheat prices in 1930–1931.

The post-war trend in production and acreage. Developments in the principal countries. The special position of Russia. The accumulation of stocks.

Summary of the short term factors influencing the present depression of wheat prices.

The long term factors. The trend of wheat prices in relation to wholesale prices since the middle of the nineteenth century. Some of the contributing causes of the gradual fall in wheat prices in comparison with wholesale prices. Progress in

technique of crop production. Lowering of costs of production and distribution. Biological advances; drought-resisting and early maturing varieties. Engineering advances; harvesting machinery and the tractor; changes in consumption.

Economic consequences of the spread of mechanisation and the use of new wheat varieties. The extension of the wheat area into drier and colder regions.

The position of Europe; Europe is the market for the world's surplus wheat, but remains also one of the chief wheat-growing regions. The contrast between the objectives in agricultural policy in the new and the old countries respectively. European agriculture founded on a tradition of the peasantry. The influence of this tradition on farm technique; its political and social aspects; its consequences in national policy. Tariffs and other protective measures. The degree of artificiality which has developed in consequence of the restrictive measures adopted by importing countries. Wide divergences in wheat prices between free importing countries and protectionist countries.

Measures of artificial control in exporting countries; the wheat pools and the Federal Farm Board. The effect of their policy on the present depression.

The reappearance of Russia as an exporter; Russia's wheat programme; its effect on the world situation.

Summary of present position. The expectation for the future.

Discussion upon the Report of the 'Macmillan' Committee upon Finance and Industry, introduced by Mr. P. Barrett Whale.

Friday, September 25.

Presidential Address by Prof. E. Cannan on *The Changed Outlook in regard to Population*, 1831–1931 (see page 110).

Dr. Raymond Unwin.—*Town and Country Planning: its Use in securing the better Distribution of Industry and Population and its Effect on Land Values.*

The increasing scale of activities, the growing interdependence of industries and the adoption of motor transport have changed the scope of Town Planning, which has become, in the new Bill, Town and Country Planning. Laying-out of new roads and open spaces, allocation of areas for dwellings, shops or industries, on the assumption that the whole area must be regarded as potential building land, has proved inadequate for modern needs.

The efficiency of industry and commerce, the welfare of the people and the needs of local administration, all demand the better distribution of industry and population over the region; the prevention of sporadic development; the protection of the highways from ribbon building. More compact and self-sustaining units of development are needed, provided with adequate open land for recreation, each equipped to minister to the everyday needs of its inhabitants. Suburban development must be grouped into planned units round local centres, and further out there must be planned other satellite units, and garden cities.

In the nineteenth century it was held that if owners used their land for the most profitable purpose, this would promote the common welfare. The congestion and squalor which resulted in large towns forced thinking people to realise that the individual interests of a multitude of owners do not coincide with the public good. The dangling of a carrot of increment before the reluctant owner, and even the additional spur of a tax on values, were plausible expedients, so long as increased wealth was regarded as synonymous with the public good, and owners held back their land from development.

Conditions have changed, and traditions weakened. Where land is held in small parcels, it is now evident that the best use of it, whether for private or public interest, can only be secured by a degree of foresight and co-ordinated planning beyond the

reach of individual owners. Public control of land development has therefore grown, culminating in the Town Planning Acts.

Enlightened owners of extensive estates had sometimes planned their land with skill. Town planners sought like benefits of foresight, by enabling local authorities to apply good planning, however numerous the ownerships. Planning by public authorities is not opposed to private interest ; it gives to many owners the benefit of having their lands developed as part of a larger whole.

Planning involves redistributing the uses of land and changing conditions ; reserving for dwellings land which could have been sold for shops ; taking for parks that which could have been used for residences ; making roads to estates otherwise not accessible ; protecting good residential sites from the intrusion of objectionable buildings, or developing efficient industrial areas. The prospect of reaping building increment will clearly be reduced on some land, increased on other ; land values will in fact be redistributed ; the assumption by the public of duties and responsibilities for which enjoyment of increment by the owner could be regarded as remuneration, tells in favour of the public right to a share of such increment. The planner, however, is concerned with good development, and with values mainly as affecting it. Proposals which diminish the owners' prospect of increment presents the difficulty. The argument derived from swings and roundabouts, applies when one person owns the whole fair. The planner is seeking a method to apply it when there are many owners ; he thinks the improved values due to planning on some land should compensate for the reduced values on other land.

Monday, September 28.

Mr. J. Morris.—*Rationalisation and the Cotton Industry.*

The organisation of the cotton industry has of late been adversely criticised in Government reports and by other observers within and without the industry. The prolonged depression in Lancashire as her share in the world's trade in cotton has dwindled accentuates the value of this criticism. The revival of prosperity in an export trade for which Lancashire possesses natural and acquired advantages is an urgent necessity.

The industry is characterised by its multiplicity of small units, its horizontal sectionalism and its employment of numerous middlemen. The comparative stability of this structure for several decades suggests that it might represent the best response to the economic peculiarities of the industry. But foreign competition has developed and has directed attention to the possibility of forming amalgamations, which would aim at attaining the competitive cost of production essential in an export trade. This policy is preferable to wage reductions, to price and output agreements, and to the gradual elimination of redundant firms by internal competition between small units.

Amalgamations may be vertical or horizontal or both, and may combine a few or many firms. Hitherto progress in reconstruction has been partial and slow, owing to the conservatism of the industry and the reliance on makeshift policies. There has been a tendency to blame particular sections, such as the finishing section, or to ascribe the depression to causes beyond the control of the industry.

The paper will concern itself mainly with a discussion of the feasibility and value of amalgamations. The reorganisation of the merchanting section, particularly in the staple trade, is essential if fragmented uneconomic orders are to be avoided, but the will to amalgamate seems absent. The spinning trade offers examples of large-scale amalgamations. Examination of the organisation of the Lancashire Cotton Corporation shows that definite economies can be secured, and that these are probably not outweighed by the difficulties inherent in the management of large-scale organisations.

The effects of rationalisation will depend on the response of demand to lower prices. Competing industries abroad will attempt to improve their organisation. Recent events show that many factors may prevent foreign customers from buying Lancashire goods even if they are cheap. It is clear, however, that these external forces merely intensify the urgency of eliminating wasteful methods from the industry.

Mr. R. L. Hall.—*Difficulties of Wage Regulation in Australia.*

The system by which wages are regulated in Australia has often been described ; but the attention now given to the problem of rigid wage levels makes the occasion a suitable one for a review of recent developments, and especially of the difficulties, which became very marked with the appearance of the present trade depression.

Wage fixing, which began with minimum wage legislation, developed in the various States either by arbitration courts or by trade boards. The Federal Government also set up an arbitration court to deal with disputes extending beyond the limits of a single State. This court in 1907 fixed a living wage according to the cost of what was then considered to be reasonably necessary. Since then the Commonwealth Court has gradually assumed a predominant place, partly owing to the ease with which it was possible to make a dispute extend beyond State boundaries, partly because of the anomalies which arose when two regulating bodies fixed wages in the same district. Thus, the State courts or boards came to follow the Commonwealth practice.

This led to great uniformity of wages, as practically all the workers in the country had their wages fixed, either at the minimum or with additions for the skilled or unpleasant nature of their work. Only one alteration, of about 6 per cent. upwards, was made in the basic wage of 1907 ; the amount was varied automatically according to changes in the cost of living as calculated by the Commonwealth statistician. But it is improbable that real wages rose by more than 10 per cent. in the period 1907–1929.

In view of improvements in industrial technique and the large amounts of capital borrowed abroad during the period, such an increase seems small and unlikely to lead to serious difficulties. Nevertheless, when the depression occurred Australian production costs were much out of relation with world costs. Such figures as are available indicate that the efficiency of production, especially in manufactures, must have fallen off during this period. This can be attributed partly to the arbitration procedure which gave the appearance of litigation to all wage questions, and embittered industrial relations. But the reason why such a large maladjustment as was finally revealed occurred, was due to the political reactions from the general conception of a fixed standard of living. All Governments were compelled to do what they could to preserve this standard, and both the tariff and foreign borrowing were used to obscure the real position. The high prices received after the war for wheat and wool, the principal exports, also gave a temporary assistance.

Thus, a tradition of irresponsibility among workers, and to some extent also among employers, was developed ; and the Government was expected to remedy all difficulties. After the fall in world prices the exchange position became serious, and it was almost impossible to borrow abroad ; this was followed by unprecedented unemployment. The wage-earning classes were extremely reluctant to see any decrease in the standard of living, but after about a year of depression the arbitration courts decided that wage reductions were inevitable. These are now in progress, showing that the wage-fixing mechanism is capable of adaptation ; the chief difficulty seems, therefore, to have been connected with the political effects of fixing a wage without reference to productivity.

Mr. R. G. D. Allen.—*On the Foundations of a Mathematical Theory of Exchange.*

1. *Introduction.*

British Association papers by Jevons and Edgeworth. The references given in the Appendix illustrate the development of pure economic theory.

Economics is a science, and the only social science which has, so far, passed into the quantitative phase. As in all sciences there are two branches : (1) empirical ; descriptive economics ; (2) rational ; pure economic theory. The rational branch starts from initial propositions to which deductive reasoning, *i.e.* formal logic and mathematical analysis is applied. Some sections of rational economics are concerned with complex operations involving magnitudes, so that formal logic is insufficient and mathematical analysis necessary. It is therefore essential that the premises on which a mathematical theory can be constructed should be enumerated and stabilised. The object of this paper is to discuss the foundations necessary for the construction of a mathematical theory of exchange equilibrium.

2. *The definition of the problem.*

Pure economic theory relates to the actions of man as manifested in the processes of exchange and production of goods. As a first step, the static problem can be distinguished from the more general dynamic problem and the process of exchange can be separated from production.

Adopting the unique method common to all sciences, the rational branch of economics must be connected with the empirical branch. The connection is made by the choice of the initial propositions, assumptions and definitions, on which the rational theory is based. If pure economic theory is to be 'true,' *i.e.* if it is to explain and connect the empirical laws obtained from observation, the initial propositions must be translations of empirical facts reduced by abstraction to their simplest elements. The initial propositions must be precise and rigorously formulated; observed phenomena are confused and complex, and give only vague ideas and representations. The problem is to translate the latter into the former.

The assumptions and definitions, serving as premises for the construction of a general theory of exchange equilibrium, can be specified under two heads :—

(1) Those relating to the action of the free individual.

(2) Those relating to the mutual relationship of individuals on a market.

In addition it is necessary to specify :—

(3) Further assumptions required for the discussion and development of the equilibrium position but not for its construction.

3. *The action of the free individual.*

Definition of economic actions and economic goods, their quantitative expression and their geometrical and analytical representation.

Definition of indifference loci and lines of preference, and the assumptions relating to them. Method of obtaining their differential equations. The preferential force vector and its components, the marginal or specific utilities.

It is not necessary to adopt the hedonistic hypothesis or to make any assumption as to the measurability of utility.

An analogy with mechanics and other mathematical sciences is instructive; wants and exchange movements in economics correspond to force and motion in mechanics.

For the static problem of exchange equilibrium the ratios of the marginal utilities are required only; the dynamic problem requires the actual magnitudes. This is illustrated by an analogy with mechanics.

4. *The mutual relationship of individuals on a market.*

Definition of a market; the liaisons or restrictions imposed upon free individual action by market conditions. Definition of price and of price units.

The various problems of man's economic activities differ only as to the liaisons imposed by the problem; the assumptions relating to the action of the free individual apply to all problems. The liaisons under competition and under monopoly.

Equilibrium is attained by the interaction of the individuals' preference forces and the restrictions imposed by the market, by the conflict of wants and limitations. As in mechanics, the interaction of these forces is expressed by a system of simultaneous equations.

5. *Further assumptions necessary for the discussion of the equilibrium position.*

Assumptions relating to the integrability of the differential equations for the lines of preference and the orthogonal indifference loci. Total utility and function indices; individual action considered as a problem of relative maximum.

Assumptions relating to the marginal utilities, the components of the preferential force vector. The shape of the indifference curves and surfaces. Collective indifference loci and the assumption of continuity. The classification of economic goods as independent, competing and completing goods.

Appendix.

References and mathematical notes.

Tuesday, September 29.

Mr. Maurice Dobb.—*Current Economic Theory and the Five-Year Plan in Russia.*

The unique background of economic problems in U.S.S.R. is composed of three leading features: (1) a Planned Economy; (2) a Socialist Economy; (3) an abnor-

mally rapid process of industrialisation. How far does current economic theory have application to these problems? How far do the institutional contrasts of a socialist and a capitalist economy invalidate the application of current economic principles to the former?

Economic theory may be (*a*) a normative study, or (*b*) a study of equilibrium designed to establish certain absolute relations between economic quantities. The core of a normative theory is the balancing of utilities and costs and the consideration of an *optimum* equilibrium (the principle of the equalisation of marginal net returns). Such principles seem inapplicable to a socialist economy owing to the inadequate nature of the conceptions of cost and utility. There seems no reason to think that the demand-valuations and supply-valuations on a free market have any major significance from the standpoint of such a problem, and no reason to think that the automatic valuations of a free market are likely to approximate any nearer to an *optimum* than the conscious calculations of a planning body.

The most significant relation dealt with by equilibrium theory is that of the interest-rate. The question: Are Tractorstroi, Gigant, Magnitorstroi, &c., 'economic'? A common answer is 'No, in view of the height of the interest-rate in U.S.S.R. compared to the cheapness of labour.' This reply is fallacious, because in a Planned Economy the interest-rate is conditioned by planning decisions, not conditioning. There seems no reason why the concept of an interest-rate in its customary form should exercise a governing influence in the calculations of a Planned Economy (as Henderson and Cassel claim).

Further, there are special reasons why the principle of equi-marginal yield should be deliberately violated in the allocation of capital. This is connected with considerations of obsolescence of equipment and the planning of capital accumulation into the future. The analogy with Pareto's 'pursuit-curve.' The relation of this problem to the problem of uncertainty: uncertainty in a Planned Economy tends to be altered and considerably narrowed.

The Monopoly of Foreign Trade in relation to industrialisation. Its effect on the relations between industry and agriculture. The question of the so-called 'exploitation' of agriculture, and its connection with the problem of the interest-rate. How far is a 'closed system' a necessary adjunct of industrialisation? It seems to be necessary if industrialisation requires the aid of inflation or the exploitation of agriculture, and such aids may be essential in capitalist economy where the height of the prevailing interest-rate precludes industrialisation. This latter question does not necessarily arise in a socialist economy; and a 'closed system' is not essential to industrialisation without these aids. The relation of internal and external price-levels in U.S.S.R. and its relevance to the problem of 'dumping.'

The balance-sheet of a Planned Economy. It seems possible that in a Planned Economy 'economic' problems may give place to 'technical' problems and economic theory (whether of Marshall or Pareto) have little application.

Mr. J. W. F. Rowe.—*Artificial Control of Raw Material Supplies.*

The main object of this paper was to enquire whether artificial or 'conscious' control, as a normal method of economic organisation for the supply of staple raw materials, is likely to achieve a better average approximation to the theoretical equilibrium of supply and demand than is at present achieved under the 'unconscious' control of *laissez-faire.*

The failure of unconscious control in the long period, that is in securing a correct adjustment of productive capacity, appears at first sight almost as complete as it is lamentable. Fluctuating prices, nevertheless, have certain merits in practice as compared with the relative stability of the theoretical equilibrium price; but a closer approximation to theoretical stability would in most cases be desirable. Analysis of the existing instability, however, suggests that conscious control cannot remedy the root causes. These considerations limit the potential superiority of conscious control, which, in addition, has to face difficulties of its own, some of which in practice and in the world of to-day seem almost insurmountable. The chances that in the long period, conscious control will be superior to the present system at the moment appear small.

In the short period the primary problem on the supply side is that of variations in yield from a productive capacity which in practice will probably not be exactly

adjusted to requirements, though for the purpose of our examination correct adjustment may be assumed. Analysis reveals that from the producer's point of view the count against unconscious control is serious ; the consumer is to-day only indirectly and much less seriously affected, so that the problem of varying yields can be considered in the main as a producer's problem. Further examination indicates that, under a wide range of conditions, producers themselves can, in theory, hold the surplus of a bumper crop until the compensatory crop deficiency at less cost to themselves than by allowing merchants and speculators to do so. In practice, however, stock-holding by producers involves war to the knife with the merchants and speculators, and this results in many difficult problems of operating technique ; moreover, the producers are faced with the great temptation to try and make the consumer pay in the short-period for the costs of the operation. Given resistance to this temptation and the evolution of a satisfactory technique, there appears to be a strong probability that such conscious control schemes would be an improvement from the producer's point of view, though their difficulties cannot be completely solved.

In the short period, there is also the problem of temporary fluctuations in demand, and the count against unconscious control is here again serious. A temporary contraction of demand can with advantage be met by concerted action on the part of producers, provided that such action does not interfere with current long-period adjustments in respect of productive capacity ; this is not an insurmountable difficulty, though the technique of operating such controls again presents most difficult problems. On the other hand, from the point of view of securing a greater measure of stability, control by producers to counter a temporary contraction in demand should be accompanied by control to counter temporary expansions in demand, *i.e.* by schemes for the maintenance of reserve stocks, or reserve producing capacity. No such form of control has yet been attempted, for though the boom which results from a sudden increase in demand is often a boomerang for the producer, yet the opportunity of squeezing the consumer means large profits here and now, and is usually irresistible. Therefore, since the only alternative, namely control by the consumer in his own interests, is usually impossibly difficult to organise, it seems probable that conscious control to meet the short-period variations of demand will remain one-sided until producers learn the merits of a more long-sighted attitude. Nevertheless, both producers and consumers may find even such incomplete conscious control more advantageous than *laissez-faire*.

Wednesday, September 30.

Prof. H. Stanley Jevons.—*The British Steel Industry.*

Mr. P. Ford.—*The Southampton 'Booth' Industrial Survey.*

DEPARTMENT OF INDUSTRIAL CO-OPERATION (F*).

Thursday, September 24.

Discussion on *The Aims, Methods and Progress of Research into Management Problems.* (Chairman : Mr. B. S. Rowntree, C.H.) :—

Major L. Urwick.—*International Position.*

Sir Horace Wilson, K.C.B., C.B.E.—*British Position.*

Dr. H. S. Person.—*American Position.*

Dr. Hellmuth Boller.—*Austrian Position.*

AFTERNOON.

Discussion on *The Means of Co-operation between Industry and Educational Institutions in promoting the Training of Managers.* (Chairman: The Rt. Hon. The Viscount Leverhulme):—

Dr. J. A. Bowie.—*The Manchester Experiment.*

A University School of Business must cultivate and maintain the closest contact with the world it serves. The need for such co-operation is generally admitted, but achievement lags far behind. No Government report in recent years that touched on the question of education for industry has failed to draw emphatic attention both to the need for intimate co-operation and to our obvious failure to achieve it. The essential reason for this lies in the existence of an educational tradition which finds the new methods necessary very little to its taste. The resultant lack of vocational opportunity at all leaving stages in the educational system is widely deplored.

Can this admitted defect be remedied ? I shall not attempt in this short paper to deal with general questions, but merely to throw open for consideration and criticism the actual developments in this direction that have been made by the Department of Industrial Administration, Manchester.

First, co-operation with industry, to be effective and continuous, must be explicitly organised. Second, it is for the School to take the initiative, and to invite and organise its contacts, preferably by setting up definite machinery for this purpose. The forms of co-operation may consist of (*a*) study-visits to industrial establishments ; (*b*) investigation work in actual businesses ; (*c*) periods spent in actual employment ; (*d*) lectures by business men on special problems ; (*e*) placement of students ; (*f*) research work in conjunction with committees of business men ; (*g*) contacts through advisory committees of business men, and (*h*) teaching methods.

An account was given of how these contacts had been developed in Manchester, and a comparison made with the practice of some of the more important University Schools of Business in America.

M. Pierre Jolly.—*The Paris Experiment.*

Economic evolution has raised business administration to a degree of complexity in which success demands a perfectly qualified higher staff. This can only be provided by intimate collaboration between the school, the shop, the factory and the bank.

Unlike British and American Chambers of Commerce, French Chambers hold themselves responsible for providing facilities for industrial education. Although already controlling twenty-two technical educational establishments, serving 6,000 students, there was still a felt need for a school for heads or " staff officers," which was launched, after careful preparation, in 1930.

This consists of two parts, the Business Preparation Centre, which is a French counterpart of the Harvard Graduate School of Business Administration, instructing by means of the ' case system,' using the ' deductive ' method.

Variants of the ' case system.'

Case material provided by the second section of the organisation, *i.e.* the Bureau of Industrial and Commercial Research.

Description of methods of obtaining material and of overcoming resistances.

The importance of careful selection of teaching staff. Those at the Business Preparation Centre have practical business experience in addition to University degrees.

It is important that such schools should be administered by business men. The Paris School is under the direction of a board including only members of the Paris Chamber of Commerce.

Prof. M. P. McNair.—*The Harvard Experiment.*

Prof. P. Sargant Florence.—*The Birmingham Plan.*

The value of university teaching, experience and selection in providing managers for rationalised business. Commerce as a university subject can be made to combine

and fuse a liberal mental education, great practical interest, and an essential vocational training.

The Birmingham Faculty of Commerce, founded in 1901 by Sir William Ashley, has passed beyond the experimental stage, and its experience may be of greater value to more recently founded English schools of business than the usual references to American and German institutions.

Characteristic features are: (1) a standard three-year undergraduate course for full-time students, as against cramming information in the last year, or in 'overtime'; (2) graduate research into business practice and a linking up with the problems of local firms through the Management Research Groups; (3) individual tuition and freedom of choice of certain subjects, with the training of judgment and encouragement of active enquiry by preparation and discussion of reports, visits to works, and, if possible, vacation employment; (4) a curriculum which integrates economic theory with questions of industrial organisation and management, and emphasises the need for statistical control and scientific method generally. A Readership in 'Accounting and Administration' has just been founded. This new move indicates the importance attached to teaching modern methods of co-ordinating management with accurate financial tests of its efficiency in operation.

Friday, September 25.

Afternoon.

Discussion on *The Effectiveness of Labour Incentives.* (Chairman: Prof. P. Sargant Florence; M. H. Dubreuil; Mr. Christopher Lee; Dr. G. H. Miles; Prof. J. H. Richardson; Mr. Clifton Robbins.)

M. H. Dubreuil.—*Concerning the Best Means of Obtaining the Unreserved Collaboration of Workpeople in Production.*

The fact alone that methods for evoking the unreserved collaboration of labour with ownership are still a subject of study, is adequate proof that the problem has not yet been solved. Paradoxically, to find the secret of the best wage system, we must seek a method in which wages are abolished.

'Ca' canny' can only be overcome by one of two methods, justice and trickery. The latter is too often chosen.

Comparison of risks and incentives activating respectively the workman, hired for wages, and the 'entrepreneur' on business on his own account.

Optimum output is only forthcoming when the worker can identify the success of the business as a whole, with his own personal profit. As most men have short views, ordinary profit-sharing in large concerns fails to achieve this object.

Technological progress has brought about a state of affairs where division of responsibility is necessary among members of the higher administrative staff in large concerns, the business being divided into sections having budgetary autonomy, despite integration within the whole framework.

This process of devolution should not be confined to the management, but also to the labour force, the units of which should be broken down to such a scale as to enable the ordinary workman to identify the efficiency of his own department or shop, with his personal reward.

The fault of 'incentive systems' designed to spur men on to work, including those very complicated methods often styled 'scientific,' is that they attempt to imprison human personality within a mechanised structure designed to obtain labour by force.

The only effective system will appeal, on the contrary, to those natural forces which urge men from *within* to all forms of spontaneous activity. The solution can never be found in any system based on external pressure.

Mr. Christopher A. Lee.

The attitude of mind of the worker as a factor in production.
Conditions that favour the successful application of incentives.

The selection of the right incentive.
Arousing enthusiasm.
The part played by leadership.
Non-financial incentive.
  The sense of loyalty and ownership.
  The sense of achievement.
  The sporting element.
Financial incentive.
  As compensation for ineffectual leadership.
  Limited application.
Combined financial and non-financial incentives.

Prof. J. H. Richardson.

The application of satisfactory labour incentives is a central problem of industrial relations, and the establishment of mutual confidence and goodwill between management and workers is an essential preliminary for the effectiveness of such incentives. Mutual confidence and goodwill are facilitated by regular joint machinery (*e.g.* works councils) for frequent discussion about changes in methods of work and conditions of labour, and consideration of workers' suggestions, by the application of agreed principles for dealing with labour problems, and by the joint review of information about the economic position of the industry and undertaking.

Among the chief direct incentives are (1) the allocation of definite responsibility to the worker or group of workers; (2) systematic promotion based on records of performance; (3) payment by results. Whichever incentive or combination of incentives is applied, it is necessary to establish a reasonable relation between the earnings of different grades of workers. Allocation of responsibility has been practised more among supervisory grades and clerical staff than among manual workers, but there are wide opportunities for organising manual work so as to confer definite responsibility upon the individual worker or group of workers. The incentive of responsibility, with a time wage or time wage *plus* output bonus, has moral advantages over the mere monetary incentive of piece rates, while group responsibility introduces valuable *esprit de corps* in place of the rivalry especially of individual piece work. The application of the incentive of responsibility involves the grading of workers according to capacity and should be combined with promotion, based on proved ability, to higher wage or salary levels. For promotion, transfer or discharge it is essential that adequate performance records should be used and favouritism eliminated. The incentives of responsibility and promotion, which can be applied to time work, should receive close attention, as very large numbers of workers are paid on a time basis.

Systems of payment by results are of value where the workers control speed of work and where the product is uniform and readily enumerated. The most widely used and generally effective system is the straight piece rate, usually with guaranteed time minimum. With regard to more complicated so-called scientific systems, it should be recognised that there is nothing scientific in the rates themselves; these are inevitably fixed arbitrarily. The scientific side is in the time and motion studies and in the psychological effects of different systems on the worker. The various systems of differential piece rate or task and bonus systems, as variants from the time or straight piece rate systems, may be applied reasonably or seriously abused. They have received relatively small application; their complication is often a disadvantage, especially in small and medium-sized plants, and their use is likely to be limited to certain industries and processes where circumstances are specially favourable. Though foremen and other supervisory grades are chiefly influenced by the incentive of responsibility, they should participate in additional monetary rewards based on output, especially where their workpeople are paid on results.

Evidence shows that piece rates, bonuses and other systems of payment by results have resulted in increased production and earnings, and lower labour costs, and also in improvement of relations between management and workers (largely because of the need for less supervision and pressure from above). There is, however, need for more systematic investigation and the pooling of experience about the effectiveness of these and other labour incentives.

Monday, September 28.

Discussion on *Patents and the Protection of Scientific Discovery.* (Chairman : Mr. J. Swinburne, F.R.S.)

Mr. A. G. Bloxam.—*Improvements in Patents from the Inventor's Angle.*

1. The inventor desires a valid patent but too often finds himself possessed of one having no subject-matter.

2. One reason for this is that the Patent Agent, owing to the official search as to novelty, has ceased to be the judge whether a patent application shall be filed and has had the duty thrust upon him of obtaining a patent for an invention verbally novel but devoid of subject-matter.

3. The British Science Guild recommended that the search should be made as soon as a provisional specification had been filed, but the Patent Committee rejected the recommendation as impracticable.

4. This impracticability cannot be admitted. A preliminary claim could be submitted with the provisional specification, but not as a part thereof, and could be subjected to the Examiner's search.

5. The search would then, if necessary, be conducted in two stages—the preliminary search on the preliminary claim and a subsequent search on the claims submitted in any complete specification which might be filed in continuation of the application.

6. By this procedure the applicant should receive the result of a search in time to prevent waste of effort and money in pursuing the invention ; fewer futile applications would be completed.

Mr. Hubert A. Gill.—*Expedition in Patent Litigation.*

The present system of rewarding inventors by the grant of patent monopolies has led to abuses because litigation over patents is extremely slow and expensive. Various remedies have already been proposed, including the introduction of a special division of the High Court with judges with technical qualifications, modifications in procedure in Court, and resort to arbitration.

An enlargement of the powers of the Comptroller to enable him to try patent cases has also been discussed and is suggested as a possible remedy.

It is also suggested that counsel experienced in patent litigation might be appointed to the post of an Official Referee for trying minor actions, and that such a Referee might be given powers different from those of the Courts, so that he might give rulings on the validity, scope or infringement of a patent mainly on papers submitted to him.

Mr. H. E. Potts.—*A Comparison of British and Foreign Patent Systems.*

The British patent system is clearly in need of reform. The paper first discusses the reports of the British Science Guild and the Board of Trade Committee. It is remarkable that these reports have not paid adequate attention to the experience of other countries. The paper draws such a comparison based, not upon the collation of documents, but upon actual personal experience. Concrete proposals are made which involve little or no expenditure, as the machinery is already in existence. The prejudices which have hindered the adoption of these reforms are discussed, and it is believed that technical opinion will be mobilised in their favour.

Dr. J. N. Goldsmith.—*Scientific Property.*

'Le sort du savant dans la cité est un problème de tous les temps : l'essai de construction juridique auquel il a donné lieu remonte à quelques années seulement. Assurer aux auteurs de découvertes scientifiques, brevetables ou non, une part légitime dans les profits matériels résultant de l'utilisation industrielle des dites découvertes, tel est l'objet de ce qu'on a voulu appeler la "propriété scientifique."'

This idea was put forward in 1921 by L. Klotz. Draft laws were submitted to the French Parliament by J. Barthelemy. The subject was taken up by the International Committee of Intellectual Co-operation of the League of Nations as the result of the intervention by the International Confederation of Scientific Workers.

Senator Ruffini's Report, 1923, approved by the Council of the League of Nations.

Further proposals by T. Quevedo and Gariel.

Declaration of a meeting of the I.I.C.I. of January 5, 1927, that there is, *a priori*, no legal impossibility in the recognition of the rights of authors of scientific discoveries.

Circulation of a questionnaire by a committee of industrial property of the International Chamber of Commerce, 1926 and 1927.

Meeting of the Committee of Experts, Paris, 1928, drawn from the Economic Committee of the League of Nations, the International Labour Bureau, the International Bureau of Industrial Property, the International Chamber of Commerce and the International Confederation of Scientific Workers.

Draft International Law submitted to the League of Nations, July 1928.

Replies from different Governments.

Discussions at the Geneva and Rome Congresses of the International Association for the Protection of Industrial Property.

Proposed application of insurance to cover the risks resulting from the protection of scientific property, C.L. 155, 1930.

C. J. Hampson's Essay.

American, German and British views of the impracticability of the propositions.

Dr. H. S. Hatfield.—*On Suggestions for Revision of the Patent Law.*

The British Patent Law was the first of its kind, and has been copied almost universally; it was also one of the causes of our long start in industry. While still generally approved by investigating committees, it notoriously fails to secure to individual inventors a suitable reward, and hence they now find it almost impossible to obtain financial backing.

The Law confuses two quite separate desirable aims: (1) the reward of the actual inventor and his backers in the developmental stage; (2) the encouragement by limited monopoly of new manufactures. The latter was the original intention of the Statute of Monopolies upon which our Patent Law is founded, the general idea of protecting individual creative work of all kinds having grown up later.

The result is that simple registration and assertion of priority is beset with difficulties and expense which baffle most individual inventors, while the grant of monopoly by the Crown is treated with complete laxity. Hence, it has hardly more value than a lottery ticket.

The remedy is to separate the two issues. The inventor should only be required to register his priority by an account of what he is doing, and should not receive a grant of monopoly. This should go to a manufacturer on application with production of evidence of *bona fides*, and be secured to him absolutely during continuance of manufacture and general good behaviour, due advertisement having given objectors a limited period to appear. The monopoly would normally carry with it a fixed percentage charge, to be divided among claimants to having had a share in the invention and development to practicality of the new manufacture. Claims would be decided by a commission. This would also deal with cases in which inventions were being manufactured without application for monopoly having been made.

The economic future of this country depends upon its export trade, and this can only flourish if we are and continue to be technically in advance of the rest of the world, since standardised goods can now be manufactured everywhere, often more advantageously than here. Hence it is necessary to utilise our creative ingenuity to the utmost. In this respect institutional systems have proved disappointing, as in all other fields of original and creative work. However ridiculous the monomaniac enthusiast may appear to other and quite different types, accustomed to dealing calmly and prudently with affairs, the product of his (possibly pathological) mind is urgently necessary to our future prosperity.

Mr. Kenneth R. Swan.—*The Inventor and the Employer.*

1. Conflicting claims of employer and employee to patent an invention, and the need for some amendment of the law to provide for suitable procedure for dealing with such claims without resorting to the charge of 'obtaining.'

2. Consideration of the question who is entitled to the benefit of the invention of the patent when obtained, and an examination of the various classes of employee and the circumstances under which inventions are evolved.

3. Desirability of some amendment in the law to allow wider scope to the Courts or other suitable tribunal to adjust differences between employer and employee by making awards to the employee or by directing the grant of licences in favour of employer or employee as the case may be.

4. The desirability of basing monetary awards upon the profits derived from the working of the patent.

5. Formulation of general principles applicable for the settlement of disputes between employer and employee regarding proprietorship of inventions.

6. Consideration of the position of the employee where the State is the employer.

7. The Commission of Awards to Inventors and recommendations of recent committees with reference to inventions by servants of the Crown.

8. Desirability of considering the interests of members of the public as well as the interests of the inventor and the Government Department.

DISCUSSION on *The Rationalisation of Distribution.* (Chairman: Sir R. WALEY COHEN. Mr. EDWARD B. GORDON; Major L. URWICK.)

AFTERNOON.

DISCUSSION on *Bridging the Gap between the Birth of an Idea and its Industrial Application.* (Chairman: The Rt. Hon. Sir JOHN ANDERSON, P.C., K.C.B. Mr. A. P. M. FLEMING, C.B.E.)

Mr. A. P. M. FLEMING, C.B.E.

A review of present industrial conditions reveals the fact that the advantage which the long-established manufacturing countries have over those newly-established is minimised by two important factors, namely, the ease and rapidity with which new manufacturing facilities can be established and the fact that increasingly the bulk of human wants can be met by products that can be produced by mass-production methods, which require very little inherent skill on the part of the workers employed.

In a country so dependent on its manufacturing industries as Great Britain, this means that the long-established industrial facilities now possess a diminishing margin of supremacy over those of more newly-established industrial competitors. Its chief asset is a body of workers possessing highly-developed individual skill. To employ this asset to the greatest advantage, however, necessitates its application to new industrial developments which require this individual skill. The development of new industrial activity is therefore a matter of great importance to this country, and one which is made even more important by the present great excess of unemployment.

New industrial activity always arises from an idea, which, if developed through certain definite stages, may become of commercial importance. There is at present no lack of ideas, but there is no nationally-organised means of completely bridging the gap between the birth of the idea containing the germ of a new industrial development and the manufacturable entity. The organisation necessary to provide the means for analysing the possibilities which new ideas afford of producing new lines of manufacture would involve :—

The establishment of laboratory facilities.

The employment of very highly-trained scientific staff.

Facilities for semi-scale manufacture, which is the only means of enabling the commercial practicability of new projects to be determined.

Association with finance houses to secure the financial arrangements for flotation.

The establishment of manufacturing facilities for the new products thus developed.

The means for selecting and training the various types of personnel required to 'man' the new industrial organisation.

There are at present organisations which deal with parts of this sequence, but there is urgent need for the establishment of an organisation capable of carrying out the entire sequence of operations.

Discussion on *The Utility of Trade Barometers.* (Chairman: Prof. A. L. Bowley. Mr. J. A. Crabtree; Mr. H. Quigley; Sir Josiah Stamp, G.B.E.)

Mr. J. A. Crabtree.

A business man's view of trade forecasting services.—The problems of the business man—his need for concentrated economic information.—Whom do the forecasting services seek to serve?—Price and Trade indices and their use in relation to the individual business.—Stock Market indices.—Hindrance of constant changes in structure and base of indices.—Why not have an agreed international base year?—The limitations of current methods of weighting.—Some faults in charting methods and suggested improvements, and need for standardisation.—Failure of forecasting services during last three years.—Why the failure and what alterations seem to be necessary.—Difference in outlook of economist and business man.—The gap to be bridged between the general economic situation and the individual business.—What the forecasting services can do.

Mr. H. Quigley.

Trade barometers depend for their effective functioning on a number of factors which cannot all be accurately defined. Chief among them are:—

1. Relative stability of purely economic values as against political and social in the modern state.
2. Accuracy of statistical registration and the continuity of adequate records over a period of time sufficient to allow some degree of historical perspective.
3. Correct definition of the industrial and economic activities covered by the statistics used in the formation of the barometer, so that duplication will be avoided and excessive over-weighting of certain factors be eliminated.
4. Accurate adjustment of the time and production factors.

Those four conditions are, in themselves, almost impossible of realisation, with the result that, in its most complete form, the trade barometer is still a very defective method of assessing trade tendencies.

The limitations of trade statistics used for study of movements over a considerable period are, primarily, mechanical. No satisfactory method has yet been evolved of linking up the elements which should comprise an adequate barometric curve. Production, trade, employment, finance, shipping and transport represent essential activities which hitherto have not been brought together within a satisfactory statistical framework, with the result that present trade barometers are based on a selective process which is dangerous in itself, through lack of adequate representation.

The trade cycle is not a natural, scientifically determinable phenomenon. It is subject to psychological, political and even ethical influences which cannot be assessed. It lends itself, therefore, to no set principle or law which could cover all the factors and influences involved. There is the additional weakness, that large-scale industrial and commercial activity, as understood now, is less than a century old, and one cannot, in default of scientific deduction, obtain principles through observation spread over a long period of time. The greatest weakness of all is to be found in the fact that the trade cycle cannot establish a clear distinction between the static and dynamic elements composing it, and statistical records are not yet full enough to allow the investigator to forecast, very much in advance, the main lines governing technical, scientific and social progress, which ultimately have an effect on trade activity.

The value of the trade barometer is decidedly limited, in view of the factors already described, but it is quite real. Through the statistics collected in its compilation it gives an illustration of the state of production and of trade at a definite time, and renders it possible for the executive to establish a basis of assessment in the study of non-statistical, ancillary or auxiliary economic movements.

The trade barometer, when it is properly executed, permits of some form of budgetary control in industry on a short-term basis, and it may be used to establish control or to guide marketing and production policies within very narrow time limits. It illustrates upper and lower limits to manufacturing enterprise, largely because it is concerned with statistics governing volumes and values. It does not serve as a

basis of individual or national trade activity, but it creates certain values which can, viewed in perspective, guide the action of the executive in determining what may be safely undertaken in the immediate future.

Trade barometers do not weaken or render unnecessary administrative capacity. The responsibility of management is not lessened by the fact that the executive has at his disposal statistical charts or curves. He must still depend for his success on the capacity to size up the situation and act wisely and promptly. In the largely unconscious training which goes to form this capacity the statistical records and movements given by trade barometers are of very great value; but it would be dangerous to use such barometers to determine policy exclusively.

Examination of the results obtained by American forecasting agencies with reference to the trade crises which have taken place since the conclusion of the war serves to show that reliance on the trade barometer would be productive of almost as much error as a complete lack of confidence in it. By the test of results trade forecasting has not been particularly successful; by the test of education and of economic knowledge trade forecasting and trade barometers have justified themselves. It is dangerous, however, to believe that a mere accumulation of curves and statistics can take the place of executive vision or acute intuition.

Tuesday, September 29.

DISCUSSION on *The Physiology and Psychology of Work: Physique, Posture and Environment in relation to Work.* (Chairman: Air Vice-Marshal Sir DAVID MUNRO, K.C.B. Mr. T. C. ANGUS; Herr EDGAR ATZLER; MM. R. BONWARDEL and HENRI LAUGIER; Prof. E. P. CATHCART, C.B.E., F.R.S.; Dr. C. S. MYERS, C.B.E., F.R.S.; Dr. H. M VERNON; Mr. D. R. WILSON.)

Mr. T. C. ANGUS.

What proof have we that poor ventilation reduces industrial efficiency? Two analogies from the senses of sight and hearing.

Examples.

Examples of Kata-measurements in industry; suggested and existing means to rectify bad conditions.

Recent advances in Kata-thermometry.

The new high-temperature Kata and its uses. The reduction of its readings into the terms generally understood by means of a nomograph.

How Factory Ventilation differs from the Ventilation of Crowded Buildings.

The heat and moisture given off by persons.

American example.

Herr EDGAR ATZLER.

Physical efficiency is dependent to a considerable extent upon the condition of the circulation, and particularly of the heart. In order to test the human heart, use is made of the fact that the capacity of a condenser, in addition to the surface and the distance between the plates, depends upon the dielectric constant of the intermediate medium. The capacity varies proportionately to the volume of a substance of a different dielectric constant brought into the condenser field. As the heart expands and contracts in a space of divergent dielectric constant, *i.e.* the air-containing lung, the possibility is provided of constantly recording the volume of the heart and its changes. With the aid of an ultra-short wave transmitter *Atzler* and *Lehmann* induced an oscillating circuit between the condenser plates, on which is placed the thorax of the person being tested. The intensity of the co-vibrations is measured by rectification, amplification and induction on a recording instrument (Dielectrograph). The curves obtained, and which should also be of interest to clinics, contain a number of details regarding the action of the heart. A particularly important factor in deciding the efficiency of the heart appears to be the altered filling mechanism in transition from the horizontal to the vertical.

MM. R. Bonnardel and H. Laugier.

Summary of the work of Langlois, Boussaguet, Desbouis, Garrelon, Marcou and Routhier concerning Human Labour in an Artificial Mine.—This artificial mine constructed by the Mining Committee, is 8 metres in length with a cross-section of 2½ square metres. It contains devices for ventilation (fan delivering 10 cubic metres per second), air heating (gas boiler generating 18,000 calories per hour), humidification (helicoidal fluid jet atomisers), which thus permit of varying the ventilation, the temperature, the hygrometric degree, and of obtaining temperatures of 40° Centigrade in an atmosphere saturated with moisture, with an air velocity reaching 6·40 metres in a gallery 1 metre in height.

The essential results are as follows :—

1. *Ventilation and Circulation.*—The ventilation, with the subject in repose, brings about a reduction in the maximum arterial pressure, particularly at high temperatures ; during work the increase in pressure is diminished by the ventilation.

2. *Ventilation and Respiration.*—Several cases (Marcou) : '(*a*) When the effect of the wind becomes manifest in symptoms of cold, no matter how slight (goose-flesh, &c.), the amplitude of the respiration, the production of CO_2, the consumption of O_2, and also the emission of vapour by the respiration are much higher than in a calm.

'(*b*) Under average conditions in which the effects of the wind or calm on the sensation of warmth are deemed to make no difference, the amplitude of the respiration and of the production of CO_2 are not affected by the wind, but the emission of vapour decreases considerably in the wind.

'(*c*) In cases (high temperatures of over 30°) in which the wind produces an agreeable sensation, the amplitude of the respiration increases under the action of the wind, the production of CO_2 diminishes slightly, and the emission of vapour decreases considerably.

'(*d*) At extremely high temperatures, *i.e.* air warmer than the body, the amplitudes of respiration and also the production of CO_2 are greater in the wind than in a calm, the emission of vapour being considerably greater in the wind than in a calm.'

3. *Study of the loss of Water.*—In repose the ratio between the water lost by respiration and the water lost through the skin (evaporatory quotient of Boussaguet) is about 40 per cent. During work it falls to 12–16 per cent. In a dry atmosphere a ventilation of 2 to 3 metres per second may cause this quotient to fall below 2 per cent. On the other hand, ' any deviation in temperature between the dry thermometer and the wet thermometer of less than 4°, when the wet thermometer is above 24°, indicates a surrounding atmosphere incompatible with the physiological working conditions.'

4. *Action on the thermal regulation.*—The ventilation distinctly assists the thermal regulation.

5. *Effect on the efficiency.*—In comparing the work done with the quantity of CO_2 evolved, Langlois and Routhier conclude ' that, in an atmosphere having a temperature adjacent 25° on the wet thermometer, an air stream striking the worker at a velocity of 1 metre per second considerably increases his efficiency.'

6. *Effect on pathological conditions.*—Investigations on animals ; comparison of working conditions on normal guinea-pigs and tuberculous guinea-pigs. The hypothermia, which is a function of the hygrometric state, is more pronounced in tuberculous animals than in healthy animals, and the consequences, which are mild in the case of the healthy animal, are serious in the sick animal.

Prof. E. P. Cathcart, C.B.E., F.R.S.

The appreciation by those responsible for the organisation of labour of the physiological limitations of man as a working unit would do much to alleviate the conditions of labour.

The adjustment of the machinery and conditions of labour to man's capacity will determine the optimal result both as regards the health of the worker and his output. The design of the machine, the environmental conditions such as the temperature, the humidity, lighting, as well as the actual workshop organisation with its rest pauses, speed of work, &c., and the physique of the personnel are all involved.

As man spends but a portion of his time within the workshop, it is evident that the mode of employment of the non-working hours will largely determine his working efficiency.

Dr. C. S. Myers, C.B.E., F.R.S.

Recent Researches in Great Britain on the Psychology of Work.—The existence of the 'end spurt' was first demonstrated in laboratory investigations made by experimental psychologists into the factors influencing the work curve. Recognition of the end spurt has proved of practical importance in occupational work, both output and contentment being increased by diminishing the size of the units in which the raw material is given out to the worker. But the unit of work must not be too small.

Change of work proves almost as valuable as a rest pause. But here again, research has shown that the changes in work must not be too frequent. The effects of changes of work and of rest pauses appear to vary according to the type both of the work and of the worker.

In certain kinds of machine work boredom has been alleviated, and output has been increased, without increase of fatigue, by speeding up the machine. But here, once more, an optimal limit of speed is reached depending both on the work and on the worker.

A distinction must be drawn between mere repetitive 'practice' and systematic 'training,' in which the general principles determining the best movements are explained to the worker. Little evidence is obtainable of a general ability common to the various simpler skilled movements. But the general attitude acquired by the worker allows of its transfer from one motor ability to another. Valuable results may be expected from the increasing application of notions of general, group and special abilities to vocational selection and guidance.

Recent research into the estimation of temperamental and character qualities and their relation to output, spoiled work and accidents is summarised. Attention is drawn to the high appreciation by the workers of the application of industrial psychological research to their occupational life.

Dr. H. M. Vernon.

The Influence of the Humidity of the Air on Working Capacity at High Temperatures.—It is stated by J. S. Haldane that the wet-bulb temperature alone has to be considered in measuring the maximum temperature which can be borne by men (*e.g.* miners) performing mechanical work at high temperatures, and that the dry-bulb temperature can be ignored. On the other hand, C. P. Yaglou and his colleagues maintain that the dry-bulb temperature of the air exerts a considerable influence, as well as the wet bulb, and they have combined the three factors of dry-bulb temperature, wet-bulb temperature, and air velocity into a single measure, which they term the *effective temperature*. For instance, still air with both wet- and dry-bulb temperatures at 70° F. would have an effective temperature of 70°, but if the wet bulb was 70° and the dry bulb 100°, the effective temperature, according to their charts, would amount to 80·5°, or 10·5° higher. Yaglou accordingly suggests that the air in hot mines ought to be conditioned by *increasing* its humidity by means of a water spray.

In order to obtain further information, two subjects have performed a number of three-hour experiments in air at a wet-bulb temperature of 70°, 75°, 80°, and 85°, and at a velocity of 93 feet per minute. One subject performed mechanical work roughly corresponding to the work of a coalminer (viz., 14,400 kgm. m. per hour). His pulse rate increased with rise of wet-bulb temperature, but in dry air 40 per cent. saturated with moisture the mean pulse rate, at a given wet-bulb temperature, was about six beats per minute higher than in air 60 per cent. saturated with moisture, and ten beats higher than in moist air 95 per cent. saturated with moisture. On the other hand, the pulse increased steadily as the effective temperature increased, whether the air was dry or moist. The pulse of the other subject, who performed very little mechanical work, also corresponded with the effective temperature scale, and not with the wet-bulb temperature. The effects were smaller in summer than in winter, owing to acclimatisation.

The body temperature of both subjects rose slightly with rise of effective temperature. The skin temperature, which was taken by means of a Moll thermopile, rose steadily with rise of *dry*-bulb temperature. The oxygen consumption of the subject was fairly steady at all temperatures, but it reached a minimum at 76°, and rose 3 per cent. at higher effective temperatures. Both subjects felt considerably more fatigued after remaining for three hours in dry air than in moist air of the same wet-bulb temperature.

The subjects made a number of preliminary experiments in order to get themselves acclimatised, but acclimatisation effects can never be entirely avoided. For instance, when four consecutive experiments were made in dry air at 100° (d.b.) and 80° (w.b.), the mean pulse rate of the subject doing mechanical work fell gradually from 121·2 to 117·2, whilst the loss of sweat increased from 16·8 oz. to 20·2 oz. per hour. The next three experiments were made in moist air at 81° (d.b.) and 80° (w.b.), and the sweat amounted to 17·7, 12·7 and 10·8 oz. respectively (*i.e.* it fell gradually as the effect of the preceding dry air experiments wore off).

Afternoon.

Discussion on *Some Contributions of the Biological Sciences to Economy and Safety in Transport.* (Chairman: Mr. Herbert S. Morrison, M.P.):—

Demonstration of *Psycho-physiological Tests for Selection of Pilots,* by Squadron-Leader H. L. Burton, arranged by Director of Medical Services, Royal Air Force.

The aim of the tests employed by the Royal Air Force in the selection of candidates for flying duties is the differentiation not only of those who are free from any physical defect, but, further, of those who will be capable of continuing to function at a high pitch of efficiency under conditions of exceptional physical and nervous stress.

Amongst the attributes a Service pilot must possess is the capability of flying in time of war, in any climate, at high speeds and altitudes for prolonged periods.

The tests, therefore, are largely concerned with the investigation of the circulatory and respiratory systems and of the general stability of nervous control, since upon these particularly fall the stresses involved in Service flying.

Records of the original, and periodic similar examinations subsequently carried out, afford a valuable means of determining any deterioration in efficiency. The index of each individual's efficiency can be expressed as a simple figure calculated from a table in use.

The methods of examination were outlined and certain of the tests demonstrated.

M. J. M. Lahy.—*Influence de la sélection dans les transports sur l'amélioration de l'apprentissage et la diminution des accidents.*

SECTION G.—ENGINEERING.

Thursday, September 24.

Sir Robert Hadfield, Bart., F.R.S.—*Notes on a Research on Faraday's 'Steels and Alloys.'*

For the first time, the 'Steel and Alloys' made by Michael Faraday during the period 1819–1824 have now been subjected by the author of this paper to complete examination including chemical analysis, metallographic, physical, mechanical and other tests. Hitherto, the only information available concerning Faraday's metallurgical researches has been that contained in papers which he presented jointly with James Stodart, F.R.S., to the Royal Institution in 1820 and the Royal Society in 1822. Not much definite information from the modern point of view is to be gleaned from those papers, as they contain very little in the way of chemical analyses or quantitative tests.

By kind permission of the Managers of the Royal Institution, the author has been able to investigate the nature and properties of seventy-nine specimens of Faraday's steel and alloys by the aid of the resources of a modern research laboratory. From the information thus obtained, which could have been secured by no other means,

it is evident that Faraday's work constituted an important and valuable research, specially considering the state of metallurgical knowledge and practice at the time. In fact, it was not only his first research but also the first systematic research in the field of steel alloys on a comprehensive scale.

Faraday made and examined alloys of steel with at least sixteen different elements as well as four special compounds, and though the specimens examined by the author represent only a portion of the total number made by Faraday, it is clear that these prove his work by no means ended in failure, as has been stated by some who were not metallurgists and were only able to inspect the specimens, if, indeed, they saw them at all. Some of the most interesting alloys made by Faraday, including those containing very high percentages of platinum and rhodium, were not to be found in the small deal box, labelled ' Steel and Alloys ' in his own handwriting, which lay in the storeroom of the Royal Institution for more than a century. A recent most careful search there failed to reveal any trace of these high platinum and rhodium alloys which Faraday described. Neither have the most painstaking enquiries in Sheffield and London brought to light any of the comparatively heavy ingots of Faraday's alloys, weighing 10 to 20 lbs. each, which were made to his instructions by the firm of Sandersons, in Sheffield.

All that has survived is the deal box, containing seventy-nine specimens, of which only thirteen weighed 100 grams (3½ ozs.) or over, the heaviest 140·10 grams (5 ozs.) and the other sixty-six averaged only 31 grams (1·1 ozs.) each. The total weight of the seventy-nine specimens was 7 lbs. 14 ozs. Information given in Stodart and Faraday's papers of 1820 and 1822 provided some useful clues of a general nature, but there was nothing to indicate the actual composition of the individual specimens. Nevertheless, with so small a total weight of material, with so many alloys of different and quite unknown composition, and with the further handicap of having to work with a small portion of each specimen, so that none of these valuable relics might be completely destroyed, the author has succeeded in determining the composition, nature and properties of the greater portion of them, and from them he has discovered much information useful to-day.

In the paper the author not only describes his own research on Faraday's specimens and presents the results obtained, but he also relates the circumstances which led up to Faraday's research, and describes the methods employed by that great experimental philosopher in this, the first of his important researches. To understand the situation, the historical part of the paper is in its way as necessary as the account of the modern research on the specimens themselves, for it represents the results of a careful study of a large number of contemporary writings and other sources of information, and it enables the conditions of Faraday's time to be appreciated. It is only by these means that a true judgment can be formed of the value of his work and the genius which inspired it.

The author explains the important influence of wootz or Indian steel in the early years of last century, and makes clear the relation between James Stodart, the maker of surgical instruments and cutlery, and Michael Faraday, who at that time regarded himself primarily as a chemist. The primitive state of scientific and metallurgical knowledge in those days is indicated, and the hand-blown ' blast-furnace ' and crucibles in which Faraday prepared his alloys are described. With these resources Faraday was able to melt pure iron, and to make alloys of platinum and steel. He could, in fact, reach such temperatures as often caused his best crucibles to soften and break down. The author relates how the long series of small-scale experiments conducted by Faraday himself in the Royal Institution were followed by the manufacture of alloys on a comparatively large scale at Sheffield. Some of the alloys were used to make cutlery, razors, fenders and other commercial articles. Faraday continued his researches for nearly a year after Stodart's death, but then, influenced no doubt by the difficulties attending metallurgical operations and want of precise chemical knowledge in those days, the absence of any urgent demand for alloy steels in industry, and the pressing claims of other researches, he did not further pursue metallurgical work, although he was always talking about his researches, and turned to the electrical investigations which led ultimately to the electrical industry as we know it to-day.

Although Faraday is, and will always, remain, most famous for his electrical and chemical researches, the facts disclosed, it is believed for the first time by the present research, entitle him to be regarded as a metallurgical investigator of the highest ability and the pioneer of alloy steels.

Dr. W. Rosenhain, F.R.S.—*Metals and Alloys in relation to Engineering Progress.*

Prof. F. C. Lea.—*The Effect of Temperature on some of the Physical Properties of Metals.*

The creep or flow of metals and other materials under stress at ordinary and high temperatures is a well-known phenomenon. The arts of drawing and forging metals have been long practised. Lead flows easily at ordinary temperatures, but at the temperature of liquid air resists considerable stresses. The quantitative determination of creep under stress and its importance in industry is a modern development. Some methods of determining creep under direct, torsional and repeated stresses. Positive and negative creep. The 'Limiting Creep Stress.' Inter-crystalline effects of stress and temperature and the embrittling effects of stress and temperature.

Prof. E. Wilson.—*On the Origin of Iron in Corrosion Products due to London Atmosphere.*

The corrosion products on metal exposed to the atmosphere of London are known to contain iron which is wind-borne. Analysis of the soot collected from chimneys in the neighbourhood of certain conductors exposed on the roof of King's College showed an appreciable percentage of ferrous sulphide but no metallic iron. Determinations of the magnetic susceptibility of the soot and corrosion products have led to the conclusion that the iron present in the corrosion is due to the mineral iron pyrites originally in the coal.

Friday, September 25.

Mr. Noel Ashbridge.—*Acoustical Problems of Broadcasting Studios.*

The first section of the paper deals with the development of studio acoustics, beginning with the conditions existing when broadcasting first became a public service in 1922. At first the study of acoustical conditions in studios was hampered by severe distortion in both the transmitting and receiving apparatus. The microphone and the loud-speaker were probably the most imperfect units, and introduced distortion to such an extent that it is not remarkable that the scientific treatment of studios did not receive immediate serious consideration. The earliest microphones were originally intended for the reproduction of commercial speech, with the result that little or no response was given to frequencies below 250 p.p.s. or above about 3,000 p.p.s. However, it was at once realised that when a broadcast was carried out from an ordinary room where a large proportion of the wall and ceiling surfaces consisted of hard plaster, the reverberation period was too great. The imperfections of the electrical apparatus accentuated this effect, owing to the existence of electrical resonances, and in the case of the microphones and loud-speakers, mechanical resonances in addition. For these reasons there was a tendency to go to extremes, and all early studios were very heavily 'damped' with sound absorbing material. With the rapid development of microphones and other apparatus, the lack of normal reverberation became obvious and objectionable.

Since it was considered that the amount of reverberation allowable for different types of musical programme must be variable, and because in many cases the same studio was of necessity used for widely different purposes, the difficulty was at first overcome by the introduction of 'artificial echo'; a device by which the apparent reverberation time of a studio can be increased to an adjustable degree by a combination of mechanical and electrical means. It became evident, however, some years ago, that this method did not give results comparable to those obtained when using a studio which possessed good natural acoustic properties. Consequently, the problem of designing studios which themselves possessed such properties had to be faced.

The next section of the paper deals with the conditions which obtain both from the scientific and practical points of view. There is the difficulty that it is impossible to forecast accurately the conditions under which the music will be reproduced in the

listener's home. In most cases, however, the reverberation time of a living-room is almost negligible, but unfortunately there are exceptions. Apart from the acoustical properties of the room, the loud-speaker and associated apparatus are by no means standardised. This fact in itself prevents a precise treatment of the problem. Again, normally music is reproduced at a much lower strength than the original, but in the case of speech the opposite is very frequently the case, a fact which often accounts for unnatural speech.

Considering studio conditions, microphone design is not finalised, particularly in connection with the maximum volume of sound which can be handled. Again, account has to be taken of the fact that the number of persons present in the studio cannot always be fixed definitely, such variations appreciably affecting the acoustical conditions in the case of the larger studios.

From the economic point of view it is difficult to arrange that a studio is always of the correct size in relation to the number of performers taking part. Moreover, for the same reason it is not always possible to design a studio with the correct relationship between height, width and length.

The third section describes the principles on which studio design is based in the B.B.C. Curves are given showing the number of players which should normally be allowed in relation to studio volume, also the correct reverberation period in relation to studio volume. Curves are also given showing the reverberation period plotted against frequency for two or three existing studios. Comments are made on the aural effect of a non-linear reverberation time-frequency curve and the question of the design of studios for special purposes such as the production of dramatic effects is briefly considered. Reference is also made to Continental practice.

Col. The Master of Sempill.—*Motorless Flight.*

Mr. E. F. Relf.—*The Compressed Air Wind Tunnel at the National Physical Laboratory.*

After a brief outline of the history of the development of this type of wind tunnel, a detailed exposition is given of the theoretical reasoning which leads to the conclusion that a wind tunnel using a compressed gas as the working fluid is the only feasible way of conducting model experiments in the laboratory under conditions corresponding with those of full-scale flight. A description of the construction of the compressed air tunnel at the Laboratory is next given, together with an account of the auxiliary plant provided to charge it with compressed air and to circulate the air current in the tunnel itself.

The maximum working pressure is twenty-five atmospheres, at which pressure an expenditure of 500 H.P. in the tunnel motor will produce a wind speed of some 90 ft. sec. in the 6 ft. jet of the tunnel. The Reynolds number obtained on a model of an average aeroplane corresponds with full-scale flight at 150 to 200 miles per hour.

There follows a brief reference to the nature of the various classes of test that will be made in the tunnel, leading to a more detailed description of the special balance and other measuring equipment that has been designed to meet the requirements of these tests. The main feature that differentiates the compressed air tunnel from all other types is that the observer cannot operate the measuring apparatus directly, so that all balances and other apparatus must be designed for indirect operation from outside the tunnel shell. In the present design the aerodynamic forces are measured by balancing them against the electro-magnetic attraction between coils of wire and observing the current necessary to effect such balance. Finally, the methods adopted to measure wind speed and Reynolds number, involving the determination of pressure, temperature, and the value of ρv^2 in the air jet are described.

Mr. R. McKinnon Wood.—*The New Wind Tunnels of the Royal Aircraft Establishment.*

The paper opens with a brief discussion of the validity and scope of small-scale experiments in aerodynamics and of the principles upon which the use of a wind tunnel rests. Small-scale experiments in the wind tunnel have been valuable in guiding the designers of aircraft; but the method is sufficiently defective in some respects to

warrant recourse to more costly apparatus, the compressed air tunnel nearing completion at Teddington, and the large wind tunnel which will be erected at Farnborough.

Between 1915 and 1919 three wind tunnels were erected at the Royal Aircraft Establishment. These take the form of tubes of square cross-section, through which air is drawn by a fan at the outlet. The model is thus tested in a stream bounded by four walls. The smallest of these tunnels has recently been removed and replaced by a 5 ft. open-jet tunnel closely modelled on that built by Prandtl at Göttingen. In this tunnel the model is tested in a free stream of air flowing across the test chamber. The tunnel requires only one-fifth of the power required by a corresponding tunnel of the other type. The absence of walls around the model facilitates experiment. The maximum wind speed is 115 m.p.h. It is intended to increase it to 250 m.p.h. by installing a larger motor. The diameter of the jet is 5 feet.

A jet of circular section is suitable for many purposes, but an elliptic section would be best suited for tests of complete models. The major axis should be about 1·5 times the minor axis. A jet of 10 ft. by 15 ft. section could be obtained in the building housing one of the present larger tunnels, which are 7 ft. square.

The large tunnel which is to be erected at the Royal Aircraft Establishment will have an air circuit identical in form with that of this 5 ft. open-jet tunnel. The wind stream will have a diameter of 24 ft., and attain a speed of 120 m.p.h. Owing to the accessibility of the air stream it can be arranged that two of the actual aircraft which will be tested in this tunnel can be prepared for test at the same time in an adjoining hall and moved quickly by cranes into the stream. The tunnel will also be used for tests of large-scale models, and similar arrangements are being incorporated overhead for the preparation of the models and their rapid movement to and from the stream. A special ventilating system will remove poisonous exhaust gases when the engine of an aircraft under test is running in the stream, and this will also serve to keep down the rise of temperature during tests.

When a full-size aircraft is under test in this tunnel the wings will extend beyond the stream. It will not be possible to obtain the lift and resistance of the whole aircraft, but the effect of variations in the central parts will be obtainable on the actual aircraft, and this information should prove of great value.

There is also in course of construction a wind tunnel in which the air flows vertically upwards, and the model will not be fixed to balances but will be in free flight. This tunnel is for investigating the motion known as a spin, from which the pilot is sometimes unable to effect recovery to normal flight. The model will be mounted on a vertical-axis bearing upon which the wind will cause it to rotate and ultimately to rise into a free spin. The upcurrent annuls the descent of the spinning aircraft and a spin of indefinite duration can be observed at close-hand.

Afternoon.

Presidential Address and Bramwell Trust Lecture by Sir Alfred Ewing, K.C.B., F.R.S., on *Power* (see page 122.)

Monday, September 28.

Brigadier-General C. H. Mitchell, C.B., C.M.G.—*Engineers' Contributions to Canada's Development.*

Canada is but a new country and its engineering progress is measured only by decades. It cannot be said that its engineering dates back for a hundred years; rather is it more nearly fifty or seventy-five.

Canada's first people, coming originally from France three hundred years ago and from Britain a hundred and fifty years later, occupied only the regions around the Atlantic coasts and the Great Lakes. It was but seventy-five years ago that the expansion began, and it is not much more than fifty or sixty years since the definite movement of population commenced to the prairie regions westward of the Lakes and to the Pacific coast.

The population to-day, now about ten millions, is confined mainly to the Atlantic Provinces, to the regions about the St. Lawrence and the Great Lakes, and to a belt not more than three or four hundred miles wide stretching across the western prairie

and the Rocky Mountains to the Pacific, a total transcontinental distance of about three thousand five hundred miles. Economically, however, the Dominion presents a much larger figure. Its developable area, now being disclosed as containing the most valuable of natural resources and largely capable of sustaining population, may be viewed as nearly three times this size. In other words, the belt can, and undoubtedly will in time, be expanded to a region of from eight hundred to a thousand miles wide.

It is but fifty years ago that the great transcontinental railway, the Canadian Pacific, was undertaken as a private enterprise, but it rapidly became the link to bind the new Western provinces to the Eastern. It, with the Intercolonial Railway built shortly before and serving the Maritime Provinces, made practical the political confederation of the provinces, which, in 1867, created the Dominion of Canada.

If the movement in the past decades was westward, Canada's development to-day is, by contrast, definitely moving northward. The New North of Canada is now beginning to be realized, and there is a distinct northward trend of population and development.

Engineering, wherever carried on, demands design, construction, and operation of engineering works in conformity with the characteristics of the country. These governing characteristics are both physical and human ; they depend upon geography, climate, and natural resources, and, above all, the habits and customs of the people in their tendencies and requirements of life and business.

Canadian conditions, demanding initiative and resourcefulness, have stimulated this independence and have developed the types of practice in the country. Canadian engineering is consequently recognised as having had great influence upon different phases of the country's activities and business. It has been a valuable factor alike upon the economic, the social, and the political life of the nation, for no young country can grow so rapidly without demanding and receiving the highest form of scientific direction in its development. This is recognised by the attention paid in Canada to high standards of education to provide the human resources with which to develop the material resources.

This influence on the country's development through the last thirty or forty years can be indicated by certain prominent fields in which special engineering progress has been made, and in which Canada has attracted attention from other nations. In all of these, engineers' contributions have been notable.

Agriculture is, and will continue to be, Canada's premier industry. In recent years its rapid mechanisation has quite changed its character, and the engineer has taken a more prominent part in farm equipment, in storage and elevators, in the milling of foodstuffs, and in transportation by rail and water.

The development of Canada's forest resources has brought about the ' miracle of paper,' whereby the paper industry has taken second place in her industries. Canadian paper is shipped to the remotest parts of the Empire. Engineers have largely contributed to this.

Mining, also, is now attracting attention, especially as it is an industry less than forty years old. This is the more remarkable because Canada now stands first in the production of nickel, second in gold, and fourth in copper.

It is, however, in water-power that Canada is attracting the greatest attention. To it can be traced not only marked industrial and economic development, but in a large measure social and economic advancement as well, for, with cheap electric power available to every home in the city or on the farm, social life promptly reacts for its betterment. The total water-power installed in Canada to-day is at a rate of over six-tenths of a horse power per inhabitant. Notable in existing developments is the very successful publicly-owned Ontario system, nearly one-half of its capacity being in one Niagara plant of 550,000 h.p., the largest yet constructed. Transmission lines in this system are in operation in lengths upwards of 220 miles, carrying power at voltages as high as 200,000 volts to most of the industries of the province, situated in nearly 700 municipalities and rural districts. Still more notable, however, will be the forthcoming power developments on the St. Lawrence River. Canada's interest in this river assures new power, when developed, of upwards of three and a half million horse power. This project is now engaging the attention of the Government for both power and deep navigation from the Lakes to the sea.

It is difficult to select a measure for the future. It would be easier if the extent of the country's resources were known, but they are not known. For instance, the mineral possibilities can, as yet, only be guessed at, as the contents of the pre-Cambrian

Shield have been ascertained at only a few places. The water-power already developed is but fifteen per cent. of the potentialities. The outstanding feature in this is likely to be a greatly increased development on the St. Lawrence River, where the combination of power with deep ocean connection will be a large factor in stimulating the development of contiguous resources.

Mr. H. H. Dalrymple-Hay.—*London Tunnelling Problems.*

In and around London there are forty-five miles of Tube railway and fifty Tube stations. In the construction of a large proportion of this work, for which the author has been responsible, tunnelling operations of varying degrees of difficulty have been accomplished. The paper gives illustrated descriptions of several of these, such as :—

Cutting through a tunnel under the Thames.

Constructing the escalators under Waterloo Station, and details of the construction of parts of Piccadilly Circus Station.

The paper is prefaced by a brief general description of the construction of a Tube railway.

Mr. A. L. Egan.—*Methods of Improving the Kata Conditions of Atmospheric Air in Deep-level Mines.*

The paper is a digest of the available relevant information concerning the engineering aspects of air-conditioning in hot and humid gold mines on the Witwatersrand. The author indicates that the problem is bound up with the siliceous nature of the dust produced by mining operations and the high humidity of the atmospheric air in these mines, the latter factor being accentuated by the large quantities of water used for mitigating the evil effects of the dust.

The paper deals mainly with the best methods for treating the ' hot spots ' of a mine. After reviewing previous work done, the author outlines further investigations which have more recently been carried out in connection with these questions. The law governing the power required for shaft ventilation is discussed in the light of tests carried out at two mines, and a description with working results is given for a dehumidification plant recently installed for experimental purposes in one of the mines. The position at present date is summarised, and future lines of investigation are suggested.

Prof. Elihu Thomson.—*Pioneering in Electrical Engineering Half a Century Ago.*

Following the Centennial Exposition in 1876 at Philadelphia, Pennsylvania, U.S.A., and the Exposition at Paris in 1878, the writer's attention was strongly drawn to the possibilities for new work in the field of electrical engineering—dynamo construction, regulation, &c. The present paper relates some of the developments undertaken by him in association with his colleague, Prof. E. J. Houston, work leading to the well-known Thomson-Houston arc lighting system, afterwards extensively used in the United States and later in Europe. This early work formed the basis of the British Thomson-Houston Company and the Compagnie-Française Thomson-Houston and similar developments in other countries. It was the combination in 1892 of the Edison General Electric Company and the Thomson-Houston Electric Company which gave rise to the present world-renowned General Electric Company of America.

The present paper first briefly describes a dynamo built by Elihu Thomson in 1878, which may be called a pioneer structure. It was a self-exciting machine with a commutator for continuous or direct currents, and it was also a two-phase alternator with collecting rings. It is notable that early in 1879 it was used at the Franklin Institute in Philadelphia to operate a pair of transformers connected to this A.C. machine in parallel by the fine wire primaries of these induction coils, later designated as of the ' shell ' type, because the iron of the magnetic core was extended as a shell

over and enclosing the copper wire coils, which constituted the primary and secondary parts of the circuit. The coarse wire secondaries of these transformers operated the load at reduced potential, and as this work ante-dated any production of incandescent lamps, a special form of small vibrating arc lamp was used, as also separate coils of resistance wire rendered incandescent. This, as is evident, was a pioneer realisation of the essential features of the transformer system of to-day. This feeble beginning, however, was followed in a few years by a widely expanding application of the principles involved.

The paper also relates the inception of, or invention of, the 'three-phase winding.' The first dynamo, the armature of which bore such winding, was devised in 1878–9, and forms the subject of patent ♯223,557, dated January 13, 1880. The original machine is preserved at the U.S. National Museum in Washington, D.C. The description in the specification of a patent relating to this, the first dynamo of its type, at first included the provision of three collector rings, allowing the output to be A.C. when desired, thus agreeing in that particular with the preceding two-phase machine above referred to. The bearing of this construction on modern power practice is evidently fundamental.

Interesting circumstances concerning the construction of this first 'three-coil' machine are noted in the present paper.

The peculiar arrangement for regulation in conjunction with these three-coil early dynamos in maintaining a constant current in a circuit of arc lamps is briefly described, and matters which may be of general interest even at this day are alluded to in this connection.

The writer closes his narrative by correcting a widely spread misconception as to the function of the unique air-blast commutator mechanism used in the machines of the old Thomson-Houston type. This function was always to secure steadiness in the lamps, as will be seen, and not, as often stated, merely a 'bold expedient' to blow out the sparks at the commutator.

Tuesday, September 29.

Sir DAVID MILNE-WATSON.—*The New Gas Industry.*

The gas industry commenced in London as a public utility in 1812, and was thus in being when the British Association was formed.

The centenary of the Association coincides with the Faraday celebrations, and some account of the great scientist's interest in and work for the gas industry over a period of nearly fifty years is given. The commencement and development of the industry are sketched with a view to showing the difficulties of the pioneers, the progress made and the gradual development and extension of parliamentary control.

The effects upon the industry resulting from the change in the requirements of gas consumers for heat instead of light are considered. The output of an average gas undertaking can be allocated in varying percentages between (*a*) domestic heating, cooking and hot water supply; (*b*) industrial heating; (*c*) lighting, and (*d*) power. Other statistics of the industry are given, and the gas industry is shown to be treating over 18 million tons of coal annually, whilst some 300,000 million cubic feet of gas are sold. The gas consumption per consumer in Great Britain averages 153 therms, or 30,547 cubic feet per consumer, equal to 30 therms per head of population. Comparison is made with the sale of electricity, and it is shown that on a heat basis five times as much gas is sold as electricity, and that in the domestic field the proportion is as 18 to 1.

The development of the gas-manufacturing process has been aided by various research organisations; the various carbonising processes are considered. The thermal efficiency of the modern continuous vertical retort is shown to be 86 per cent.

Cheap power is now produced in gasworks from the heat previously wasted in chimney gases. New extensions have been made to the purifying processes, notably the elimination of naphthalene, moisture and the extraction of benzole by activated carbon.

The requirements to be satisfied by fuels to-day are severe, and a high standard of precision of performance is expected. The development of gas-using appliances to give such precision in the home and in industry is explained.

In addition to gas, the industry produces smokeless fuel—coke. Lately much attention has been given to the scientific production of this fuel, and special apparatus evolved for its use.

The production and distribution of fuels, however desirable, must be accompanied by a selling organisation giving expert advice, demonstrations and maintenance facilities ; the new gas industry has developed such services.

The increasing demand for gas and coke finds parallels in the experiences of other highly-organised countries. In America the sale of gas is seven-and-a-half times that in this country, and has increased 110 per cent. in ten years. Similar results are noted in Germany, Sweden and Switzerland.

In Germany much interest has been aroused in the Ruhr Gas Grid, extending to 1,800 miles of main and designed to dispose of 300,000 million cubic feet of gas from the coke ovens. Parliament has recently passed such a scheme for the neighbourhood of Sheffield, following upon the recommendation of a Government committee to consider a gas grid in the north of England.

The gas industry endeavours to treat its employees as thoroughly as its consumers, and mention is made of copartnership schemes and welfare work.

The situation concerning low-temperature carbonisation, in the light of smokeless fuel and home-produced oils, is reviewed. The suggestion is made that various hydrogenation processes may render oils from high-temperature carbonisation suitable substitutes for imported oils, and that coal blending and pre-treatment can result in improved smokeless solid fuels.

It is claimed that the gas industry of to-day is virile, scientific and progressive.

Prof. E. G. Coker, F.R.S.—*Force Fits and Shrinkage Fits.*

A number of important cases arise in engineering practice, in which two parts of a machine or structure are connected together by forcing one part into a hollow of slightly less size in the other part, or alternatively cooling the first part in liquid air or other refrigerant, so that when it is set in position it is, temporarily, slightly smaller than the hollow, but ultimately regains its original size, and then exerts considerable pressure on the part in which it is placed.

Other familiar cases are the heating of enveloping bodies, such as tyres for wheels and straps for crank webs, so that the envelope can be readily placed in position, and then shrunk by cooling to give a firm grip of the interior member. In such cases of engineering practice the stress distributions produced in the assembled parts are usually difficult, if not impossible to solve by analysis.

Experimental measurements have been made of the strains experienced in wheels and other bodies, from which the stress distributions were inferred.

The present paper deals with experimental observations on crank webs, locomotive wheels, boiler tube plates and the like, in which the processes mentioned above are carried out with transparent models, and the stress distributions are measured by photo-elastic means.

A number of such cases are described in some detail, and the applications to actual practice of the measured stress distributions are discussed in connection with the theory of stress distributions in multiply connected plates.

Prof. C. F. Jenkin, C.B.E., F.R.S.—*Earth Pressure Investigations.*

Report of Committee on *Earth Pressures.*

Wednesday, September 30.

Prof. Julius Hartmann.—*Spark-neglecting Commutation : a New Principle in Large Power D.C. Production.*

At the Leeds Meeting in 1927 a new mechanical rectifier for large powers, *the el-wave rectifier*, was described to the members of Section G. This rectifier introduced a new principle, *the principle of spark-neglecting commutation*, into practical electrotechnics. The present paper is a plea for the adoption of this principle, which

is thought extremely well suited for all kinds of d.c. production and for wider purposes, too.

In the first part of the paper the new principle is compared with the principle of *valve commutation* as known from the mercury vapour rectifier, and especially with the principle of *sparkless commutation* as developed for use in the old rotary equipment for d.c. production. The wearless devices upon which the spark-neglecting commutation is based are reviewed, and results of recent investigations with regard to the size of the load and the voltage which can be dealt with by a simple jet-wave commutator are reported. The most striking of these results is perhaps that, while the largest voltage difference between two adjacent bars of the collector in a d.c. generator is, say, 30–50 volts, the corresponding voltage difference between the two sides of a jet-wave commutator, operated in hydrogen, can at least be raised to 1,800 volts or probably to still higher values. At the same time the load of such a commutator can be made as large as 100 kw.—although the jet-piece utilised in the commutator and forming the movable member of the same has a weight of 25 g. only.

These extraordinary qualities of a commutator according to the principle of spark-neglecting commutation rendered a discussion of the various possible forms of spark-neglecting commutators a matter of great interest. Such a discussion has been carried out, and a brief review of the main results is given in the second part of the paper, where three new commutator types—the jet-chain commutator, the spiral wave commutator and the plate-jet commutator are described. All types are based on the wearless commutator devices developed in connection with the jet-wave commutator, and differ only by the way in which the moving member of the commutator is produced.

In a third part of the paper the scheme of a future transmission of electric energy in the form of high-tension direct current is touched upon. The principle of spark-neglecting commutation permits the production of rectifiers for practically all powers and voltages. At the same time an inverter, *i.e.* an apparatus for the conversion of a direct voltage into an alternating voltage, can be based on the principle, since a spark-neglecting rectifier plant forms a reversible system. Finally, it follows that a step-up and step-down d.c. transformer can be built as a combination of an inverter and a rectifier. In a future scheme of high-tension d.c. transmission all three types of apparatus will be required. It was thought likely that the principle of spark-neglecting would lend itself better to the production of the necessary apparatus than perhaps any other principle of commutation. This point of view gave rise to the erection of a special plant for the study of the systems in question. Some results obtained with this plant are reported in the paper.

Report of Committee on *Electrical Units and Definitions.*

Report of Committee on *Stresses in Overstrained Materials* (see page 269).

The following papers were presented by this Committee :—

(1) *Indentation Hardness of Test-pieces resulting from Plastic Flow.*—Sir Henry Fowler.

(2) *The Hardness of a Steel Tube along certain ' Lüders ' or ' Piobert ' Lines.* —Sir Henry Fowler.

(3) *The Upper and Lower Yield Point in Steel exposed to non-uniform Distribution of Stress.*—Prof. Gilbert Cook.

(4) *Plastic Strain in relation to Fatigue in Mild Steel.*—B. P. Haigh, D.Sc., and T. S. Robertson, B.Sc.

(5) *The Phenomenon of Tensile Yield in Mild Steel and Iron.*—Dr. J. G. Docherty and F. W. Thorne.

SECTION H.—ANTHROPOLOGY.

Thursday, September 24.

Presidential Address by Prof. A. R. Radcliffe-Brown on *The Present Position of Anthropological Studies* (see page 141).

Mr. C. W. M. Hart.—*Tribal Government in Australia.*

Miss C. Wedgwood.—*A Critical Analysis of the so-called Avoidance and Joking Relationships, with special reference to Melanesia.*

Miss B. Blackwood.—*Puberty Rites and Initiation Ceremonies in the Northern Solomons.*

The rites which form the subject of this paper are practised by the natives of the northern part of the island of Bougainville. The description given is based partly on the personal observation of the writer, who has herself taken part in some of them, and partly on accounts given by natives well known to her.

The ceremonies for boys are connected with the wearing of the *upi*, an elongated oval structure made of palm leaves, on the heads of the adolescents. It is put on at the first ceremony (*wapi*), when the boys are about seven or eight years of age, and worn for ten or twelve years. During this time the boys are not allowed to cut their hair, which is pushed up inside the *upi*.

The *wapi* ceremony takes place in the heart of the forest, all women being rigorously excluded. The bull-roarer is sounded and mock fighting takes place. Huge wooden figures, carried on the shoulders of men, and intended to represent the spirits of the dead, appear out of the bush and go through the actions of killing the novices, and subsequently bringing them back to life again. At the close of the ceremony the *upis* are put on the heads of the youths. From this time onwards they must never be seen without their *upis* except in the seclusion of the *tobar* (men's house). They are subjected to many restrictions, the most severe being those connected with avoidance of women. They may not enter any hut in which a woman lives, not even their own mother's, but must hold themselves aloof from all but the most distant converse with women.

At a subsequent time, determined by the chief, and dependent largely on the food supply, the second ceremony (*watawutch*) takes place. This is held in the village, and is not secret. Each novice in turn climbs a tree set up for the purpose in the centre of the village, having first removed his *upi* to show his hair, which often reaches far below his waist. A procession is formed of the novices seated on the shoulders of their sponsors, and preceded by wooden figures of men, women and birds. Much feasting and dancing ensues.

After undergoing this ceremony some of the more rigorous restrictions are removed, but the novice still wears the *upi* and is not entirely free from prohibitions until it is officially removed, and his hair cut, at a third ceremony (*wasipsip*), which is often delayed till he is seventeen or eighteen years of age. It is less elaborate than the other two, and consists chiefly of the distribution of food. The youth is then regarded as a man, with the right to live with the wife to whom he has been betrothed from childhood.

There is also a ceremony marking the first menstruation of a girl, but while every boy wears the *upi*, the rites for a girl are performed only in the case of a person of rank.

The girl is secluded in her mother's hut for five days. During the first two of these she must fast, and the whole village fasts in sympathy. After this she is given small quantities of taro ceremonially prepared. Two smaller girls are appointed as her attendants and remain in the house with her. Other women of the village keep her company by turns.

At the end of the five days she climbs a banana palm planted in the village for the purpose. This is then cut down, and she is received into the arms of the village women, who must take great care that she does not touch the ground. She then retires into the house, and the women indulge in horseplay among themselves.

These ceremonies have now been given up everywhere except over a small area. They will soon disappear altogether, as the younger generation is beginning to chafe at the restrictions involved.

Mr. Gregory Bateson.—*A Form of Shamanism on the Sepik River, New Guinea.*

The Iatmul people of the Sepik River (New Guinea) practise a form of Shamanism which is comparable with Asiatic Shamanism. We may study it from three points of view: (*a*) Morphological, (*b*) Functional, and (*c*) Evolutionary.

According to our point of view different facts become relevant, and we require different axioms and different criteria of equivalence.

In conclusion, it is suggested that Sepik Shamanism is probably not morphologically equivalent (homologous) to that of Asia, although the two are to some extent functionally equivalent (analogous). From an evolutionary point of view it is probable that similar processes and laws of change have been at work in the two areas, and that therefore the two systems are to some extent genetically equivalent (homonomous).

Sir Hubert Murray, K.C.M.G.—*The Scientific Method as applied to Native Labour Problems in Papua.*

In dealing with native labour in Papua, the Administration has as its principles: (1) the well-being and development of the natives; (2) the development of the country in which they live.

The first is a 'sacred trust of civilisation,' accepted for all native races in the British Empire; their interests are to be 'paramount,' that is, never to be sacrificed to those of the European settlers.

Development of resources is necessary (*a*) that Papua may contribute what it can to the general wealth of mankind; (*b*) in the direct general interest of the natives themselves, as well as of white people attracted thither. Without development, there can be no local revenue, and local revenue is necessary because the subsidy from the Commonwealth of Australia is not sufficient to maintain order, suppress head-hunting and similar customs, and maintain schools and hospitals and other utilities.

Development may be carried out by natives, either working for themselves, in their own interests, or working for European employers, or by both methods combined. In Papua, unlike West Africa and Tanganyika, the natives could not have developed plantations or mines without European direction and capital. But the demand for labour under European direction must be ratified either by natives or by imported labourers. Australian opinion precludes the latter, and rightly insists that both on principle and in practice Papuan labour must be voluntary. Such labour may be either free or indentured. The Indenture system implies a legal penalty for breach of indenture, which is defensible until the native develops the sense of responsibility for contracts; but in various ways the transition from indentured to free labour is facilitated. Meanwhile, the indenture system serves to regulate the labour supply. It is, however, applied only to men, because indentured households lose their status in their own village, and tend to become a landless proletariat.

Development by native enterprise is slow. The native-tax (applied exclusively to the benefit of natives) encourages produce and manufacture; and a native community may work out its liability to tax by establishing a plantation in co-partnership with government; individual natives are encouraged in development work on their own account.

Admittedly we are imposing on a primitive people a form of activity involving a conception of duty which is really strange to them. The administration, while making them face the new conditions, must see that they suffer no harm from this absolutely novel and very dangerous adventure.

Afternoon.

The Rt. Hon. Lord Lugard, P.C., G.C.M.G.—*Lecture on Africa in Transition.*

(Joint Meeting with Section E, *q.v.*)

Friday, September 25.

Miss E. W. Gardner and Miss G. Caton-Thompson.—*Preliminary Work on the Geology and Archæology of the Kharga Depression, Egypt.*

The oasis depression which lies in the Libyan Desert about 120 miles west of the Nile covers an area of about 11,000 square miles, the excavatory concession for which is now held by the Royal Anthropological Institute. Although the oasis was certainly under the domination of Egypt as far back as the Middle Kingdom, and probably at a still earlier date, its dynastic record has so far yielded no archæological data prior to the twenty-seventh—Persian—dynasty. Herodotus, echoing a popular and doubtless primeval belief in the oasis as the abode of departed spirits, called it 'The Island of the Blessed.' The part played by the Libyan Oases in the origination or development of certain very early elements found in predynastic Egypt requires investigation. The season's work has shown that a people in a 'neolithic' stage, but perhaps without pottery, was centred there: their culture has certain affinities with the Faiyum 'B' phase, on the one hand, and with the Badarian on the other, but may be older than either. Huge flint mines of this, and older periods have been discovered. In Palæolithic times a Levalloisean industry not yet found *in situ*, collected in quantity on the desert plateau; this industry overlies the travertine formations, and certain plateau and wadi gravels magnificently developed on the cliffs.

On the floor of the depression itself the deposits of Pleistocene fossil springs have been discovered. An *Aterien* industry lies *in situ* in these, whose characters suggest culture-contact between the Levalloisean of the Plateau and neanthropic elements. At no time did a lake exist in the depression, as has been thought, and prehistoric man must have depended for his water upon the activity of the now fossil springs, whose alternating deposits of clays, loams and sands may be expected to yield information concerning the physiographical régime of Pleistocene times.

Mr. L. A. Cammiade and Mr. F. J. Richards.—*Climatic Changes in Palæolithic India.*

A study of the laterite capping of Manjan-Kerauai Hill, near Madras, and of the palæolithic tools and flakes found in and on it suggests the following climatic sequence:

a. A period of heavy 'monsoon' rains, during which the laterite was formed.

b. A relatively dry period unfavourable to the formation of laterite and associated with a prolific culture of early palæolithic type.

c. A period of violent rains and heavy erosion, and a recurrence of laterite formation.

d. A reversion to relatively dry conditions, similar to those which now prevail, and associated with a change in type of the palæolithic artifacts.

Can these variations be correlated with the isolation of temperate flora and fauna in tropical hill-tops and with the retreat of glaciers in E. Tibet?

Miss W. Lamb.—*Excavations in Thermi.*

Prehistoric Thermi, on the east coast of Lesbos, is the first Troadic settlement discovered in Greek lands; though possessing certain individual features, its chief contribution to archæology is the light it throws on the hitherto unsolved problems of Troy and on the spread of the Anatolian culture in the Early Bronze Age.

Thermi was colonised from Asia Minor some time before 3000 B.C. Architecturally, we can distinguish five towns: ceramically, three periods: towns and periods can be equated as follows:—

Thermi I, Thermi II	=First Ceramic Period. Resembling Troy I.
Thermi III	=Second Ceramic Period. Not represented at Troy.
Thermi IV, Thermi V	=Third Ceramic Period. Equivalent to Troy II.

Architectural evidence, and the evidence of most of the pottery suggest that the site was abandoned (not destroyed) about the time of the sack of Troy II, and that our town V flourished during that stage of prehistory when the influence of Troy was strongly felt in Macedonia and the western Aegean. This town was surrounded by a

wall and entered through two gateways. The houses, divided by streets and alleys, are long and narrow, and one example shews the *Megaron type* already developed. Semi-apsidal houses are not uncommon.

Whereas town V has been completely uncovered, towns I–IV have been, so far, only partially explored. No. III is characterised by *bothroi*, allied to those found in an Early Helladic context in mainland Greece: Nos. II and I, differently oriented to their successors, show slight traces of Cycladic influence and produce a few Cycladic imports. There is, however, no change of culture within the settlement.

The finds include pottery, terra-cotta figurines, bone, stone and copper implements but no trace of bronze.

Mr. W. A. Heurtley.—*An Early Bronze Age Site in Western Macedonia.*

This year's excavations in Macedonia by the British School at Athens has just been concluded. A site in Western Macedonia was chosen, at the village of Armenochóri, near Flórina, some twenty kilometres south of the Greek-Jugo-Slav frontier. The narrow valley (2,000 ft. above sea-level) is here watered by numerous tributaries running northward to the Tserna River, on one of which the site lies.

The excavations revealed a deposit of an average depth of two metres, containing two occupation levels. Both belong to the Early Macedonian Bronze Age culture, which is known from previous excavations by the School in other parts of Macedonia, to have flourished about 2500–2000 B.C., and to have been the counterpart of the Early Helladic culture further South, both being probably of Anatolian origin and developing on roughly parallel lines.

At Armenochóri, the two levels represent the two phases of this culture, the earlier being imposed upon an indigenous Neolithic culture (of which numerous elements were found associated with it), the latter being a development of the earlier.

In the upper level were found some thirty complete vases, each with two high-swung, ribbon-shaped handles, a form which was to have great vogue in the succeeding period in the South, and throughout the subsequent history of Greek pottery. Its discovery in such quantity in this early context at Armenochóri is thus of unusual interest.

Besides these vases, a large quantity of coarse cooking-vessels was found, some bored stone celts, small stone saws, and other stone objects, all proper to this culture, as well as a remarkable clay figurine. Except for the Neolithic sherds, and one incised sherd, no obvious contacts with more northern cultures were observed.

After the conclusion of the excavation a short excursion was made into Jugo-Slavia to the region between Bitolj and Prilep, where similar mounds had been reported. About twenty kilometres north of Bitolj, a settlement belonging to the same culture was identified.

It will be the object of future excavation to define the limits of this culture in this direction, and its relation to the Gradač-Vinča culture.

Dr. Vassitz.—*The Vinča Site.*

Miss D. A. E. Garrod.—*Excavations at the Wady al-Mughara in* 1931.

Excavations in the caves of the Wady al-Mughara, Mt. Carmel, are being undertaken by the British School of Archæology in Jerusalem in collaboration with the American School of Prehistoric Research. In 1931 work was carried out on three sites:—

1. *Mugharet al-Wad* (Cave of the Valley). This is the largest of the group, and the interior was excavated in 1929 and 1930. This year work was confined to the Mesolithic deposit on the platform of the cave. Two phases of the Mesolithic industry of Palestine were identified, and these have been named Upper and Lower Natufian. In the Lower Natufian was found a group of burials with head-dresses and other ornaments made up of dentalium shells and bone pendants, in place on the skeletons. At the base of the deposit was a wall of rough blocks of limestone resting on the bedrock; this curved outward from the levelled area and rock-cut basins found last year, and evidently formed part of the same group of 'works.'

2. *Mugharet as-School* (Cave of the Kids).—A small rock-shelter containing a single archæological deposit of Mousterian age. Towards the base of this deposit the skeleton of a young child was found in an intensely hard breccia accompanied by typical Mousterian implements.

3. *Mugharet at-Tabon* (Cave of the Oven).—This cave contains a very deep deposit, of which the upper part has yielded a Mousterian industry identical with that of the Mugharet as-School associated with a well-preserved fauna. The lower levels are still unexplored, but have been reached in a sounding.

Afternoon.

Mr. H. St. George Gray.—*Results of Excavations Conducted at Avebury.*

In 1899 a Committee of the British Association was formed to deal with the subject of the 'Age of Stone Circles,' and in 1901 and 1902 excavations were conducted at Arbor Low, in Derbyshire. In 1905 the Stripple Stones Circle in Cornwall was examined, and in that year and the following that circle and four others, forming a group of five on Bodmin Moors, were surveyed. In 1908 some changes were made in the Committee, and excavations began at Avebury, the work being continued in 1909, 1911 and 1914, and resumed finally in 1922. Accounts, containing considerable detail, appeared in the *Reports*, *Brit. Assoc.*, after each period of excavation, and Arbor Low and the Bodmin circles have been dealt with more fully in *Archæologia*, vols. LVIII and LXI. It is hoped, also, to publish the Avebury work on a larger scale than is possible in the pages of the *B.A. Reports*. In 1915, a plan of Avebury was prepared at a scale of 40 feet to 1 inch; also sectional diagrams of the great fosse during the periods of excavation. (These, together with a large number of lantern slides of photographs of the excavations and of the 'finds,' were exhibited at the meeting.)

In point of size and grandeur Avebury stands out pre-eminently among the prehistoric stone monuments of Great Britain; at the same time it is decidedly difficult to realise fully what Avebury and its appendages were when in the height of their glory. The monument has been terribly mutilated, and vandalism must have reigned supreme for many years to effect the complete destruction of some 95 per cent. of the great sarsen monoliths.

The Avebury of to-day consists of a few enormous standing-stones and similar prostrate stones, scattered over an area which is usually recorded as 28½ acres, and is surrounded by a stupendous fosse; this again is bounded by a vallum of imposing height. The destruction of parts of the bank and ditch was caused chiefly by the building of the village of Avebury and the construction of roads which approach the ancient monument from four directions. Walls and houses, obviously built of the venerable stones cracked up for the purpose, meet one's gaze at every turn.

Avebury has a circumference of about 4,442 feet, whereas the vallum and fosse at Stonehenge measure only 1,107 feet (about one-quarter of Avebury). The diameter of the area at Avebury within the encircling vallum measures 1,400 feet. Within the fosse are the remains of the great outer circle, and within that the northern and southern inner circles and other megalithic remains with which they are associated.

The excavations were confined chiefly to the fosse on the southern side of the monument, but one cutting was made into the vallum and one into the socket-hole of one of the prostrate stones. A total length of 134 feet of the fosse was re-excavated; the average depth where there was only a normal accumulation of silting was about 18 feet, but in one place against the S. entrance causeway (the position of which was proved) the depth was 30½ feet. The average width of the fosse at the top was 30 feet, at the bottom 15 feet.

There can be little doubt that the Avebury fosse and vallum are referable to the late Neolithic period. The total absence of metals in the lower parts of the silting of the fosse and in the vallum cutting affords strong negative evidence. The persistence of tools of stone, antler and bone, including flint implements, antler picks, hammers and rakes, bone shovels and other worked bones, at least strongly suggests Neolithic date. The evidence, too, of this date is greatly strengthened by the discovery of fragments of pottery; some of the types compare with similar prehistoric pottery found more recently in excavations in England.

Mr. A. Leslie Armstrong.—*A late Upper Aurignacian Station in North Lincolnshire.*

Excavations at Mother Grundy's Parlour, Creswell Crags, Derbyshire, carried out by the writer in 1924, revealed the gradual development of Upper Aurignacian

culture in England and established the fact that this development was of a distinctive character and practically free from Magdalenian influences.

Excavations in the Mendip caves and elsewhere have since confirmed these conclusions, and it is now recognised that the latest phase of this developed Aurignacian culture is essentially an English one.

Hitherto, definite occupation sites have been confined to caves, but four years ago what appeared to be an open station was located on the North Lincolnshire Cliff Range, in the Scunthorpe district. This has since been consistently examined each year, and the original conclusion proves to be correct.

The site, though confined to a limited area, has yielded a typical series of Developed Aurignacian tools, including long batter-backed blades, keeled scrapers and burins of various types ; also microlithic blades. The Scunthorpe area has long been known as one of the type stations of the Azilio-Tardenoisian culture in England, and the Upper Aurignacian site forming the subject of this communication is situated within a few miles of one of the areas where pygmy tools are most abundant. The industry represented, however, is free from Tardenoisian influences. Pygmy tools are extremely scarce and it is believed to be a representative site of the native culture upon which the Azilian and the Tardenoisian was engrafted. Tools which appear to belong to the same cultural epoch have been collected on the Ridges southwards towards Lincoln, occurring upon the surface, together with tools of later industries, and what is likely to prove a second definite occupation area has recently been located in the vicinity of Willoughton and is under examination.

Mr. V. E. Nash-Williams.—*The Early Iron Age Hill Settlement at Llanmelin, Monmouthshire.*

During the summer of 1930 the National Museum of Wales carried out tentative excavations on the site of a fortified hill-settlement at Llanmelin in Monmouthshire, two miles to the north of Caerwent, the site of the Romano-British town of *Venta Silurum*. The settlement crowns the flattened summit of one of the southern spurs of a range of low, wooded hills. The Llanmelin spur is defined on the west by the valley of a small mountain stream, called the Troggy, on the east by a shallow re-entrant into the hills ; it rises to a maximum height of 335 feet above sea-level. The defences of the camp in their circuit of the end of the spur cling steadily to the 300-foot contour-line.

Structurally, the earthwork comprises two main parts—the camp proper, a roughly elliptical enclosure comprising an area of nearly 5½ acres, and a narrow oblong 'annexe' with an area of about 2¼ acres, tacked on to one of the two longer sides of the ellipse. There is no direct means of intercommunication between the two parts. The camp is defended by a multiple series of banks and ditches, with a single narrow inturned entrance situated in one of the angles at the junction of the camp and the annexe. The annexe is similarly defended, with multiple banks and ditches on one (longer) side, but by a single steep bank and a ditch on the other. Entrance to the interior was apparently gained through an inturned entrance in the narrow side farthest from the camp. A remarkable feature of the annexe is a series of cross-banks and ditches, of the same massive build as those of the main defences, *traversing the interior* and dividing it into three separate bays or enclosures.

The work carried out on the site during the summer of 1930 was limited to a general survey of the ground and the cutting of a single section across the earthwork on the line of its major axis. The section showed the earthwork to have been constructed directly on the natural rock. The defensive ditches were cut in the rock. They were U- or V-shaped in section, ranging from 17 feet to 34 feet in width and from 8 feet to 12 feet in depth. The banks were composed of earth and rock-fragments representing the upcast from the ditches. They varied from 15 feet to 25 feet in (basal) width, and from 4 feet to 8 feet in present height. Originally, however, they must have been at least twice as high. Where drawn around the steeper slopes of the hill, the banks were revetted on the outside with rough stone walling, dry-built. On the north-west, where the slope is steepest, the builders had made the ascent still more precipitous by cutting back the rock just below the defences to a vertical face nearly 6 feet high.

Traces of occupation within the earthwork were found at four separate points—immediately inside the entrance to the main camp and, in three places, inside the annexe. At the first of these points the cutting revealed a slight occupation-layer,

which yielded a small quantity of pottery, a sling-stone, and a bronze penannular brooch with spiral terminals of a type represented elsewhere, *e.g.* at Glastonbury. The pottery included fragments of bead-rim jars, hand-made, of coarse wares varying from light-brown to grey-black in colour, a single sherd of a native wheel-made vessel of orange-buff ware, and part of the base of a Roman *olla*. The evidence from the annexe comprised the remains of two cooking-hearths found bedded on the rock *underneath* the cross-banks, and, in addition, the foundations of a mediæval stone hut inserted in the innermost bay. The cooking-hearths found underlying the cross-banks were in each case covered with charcoal, in which occurred numerous pot-boilers, burnt animal-bones and a number of fragments of pottery. The latter were all hand-made, for the most part of coarse grey or greyish-brown ware, fairly well-baked, with a hard smooth surface. Among the vessels represented were straight- and oblique-sided pots ornamented with lightly incised wave-patterns, also globular jars with constricted neck and everted rim, one of them having the exterior neck ornamented with a double row of incised chevrons.

The evidence from the site generally is too indeterminate for precise dating, but the pottery, save for the olla-fragment and some mediæval sherds found in the hut, may all be assigned to the La Tène epoch. On present evidence, therefore, it would appear that the Llanmelin settlement was first founded in this epoch, probably not later than *c.* 200 B.C. The occurrence of the cooking-hearths beneath the cross-banks of the annexe suggests that the annexe may be of later date than the main camp. The settlement continued in occupation down to the opening of the Roman period, but perhaps not beyond. Probably its place was then usurped by the Romano-British town established at Venta Silurum (Caerwent) two miles away. In mediæval times the earthwork was reused by occasional settlers.

Sir ARTHUR J. EVANS, F.R.S.—*Discovery of the Temple Tomb of the House of Minos.*

Following up a clue supplied by the discovery of a massive gold signet ring, the author had brought to light beneath a bluff of the limestone hillside south of Knossos a sepulchral monument of a new character, combining a mortuary chamber excavated in the rock with a sanctuary building. The basement part of this, which forms the approach to the tomb, was constructed in a cutting in the slope and culminated in a columnar shrine above ground. A curious confirmation was thus obtained of a very ancient Cretan tradition, preserved by Diodorus, that, on the death by treachery of the last king of the name of Minos during his Sicilian expedition, his Cretan followers had raised him a magnificent monument of a dual character—a tomb hidden in the earth and a temple, dedicated to the goddess, above. Apart from this striking confirmation of folk-memory the whole plan of the building and the relics found within proved to be of the highest religious and anthropological interest.

The lower entrance led through a pavilion, seemingly designed for memorial feasts, to a small paved area adapted for funeral sports, and overlooked by roof terraces. A doorway between two pylons gave access thence to a hall opening on a pillar crypt, a portal in the inner wall of which led into the rock-cut chamber. From the hall a staircase ran up to the roof terrace that gave access to an upper bi-columnar shrine or temple proper. This seems to have been partly ruined by an earthquake that also did much damage to the Palace about 1520 B.C., and it was probably on this occasion that the plundering of the original interments took place which led to the loss of the gold ring.

The signet illustrates the advent of the Minoan Goddess to a new sanctuary, conveying her little pillar-shrine in a bark across an arm of sea. A separate scene shows her seated on an altar base and offered the juice of a sacred tree, which, like the Somaq of the Vedas, produced an ecstatic state.

The Sepulchral Chamber itself as its sunken pavement and central pillar indicate, was also a scene of worship, and a characteristic stone block for libations, with five tubular cavities, represents in an almost unchanged form an early Nilotic cult object. An incense burner of later date was also remarkable as having been painted inside as well as out with bright-coloured decoration for the use of the dead. In the last age of the Palace (*c.* 1400 B.C.) the vault was again opened—for the interment probably of some last scion of the House of Minos in a corner pit. This, though it had been rifled for precious objects, contained many interesting relics, and there was also

evidence of a renewed funereal cult illustrated by a series of offertory bowls and goblets, amongst which were miniature jugs otherwise associated with domestic snake worship. Some of the bones had drifted outside the entrance of the tomb, and the skull with which they are associated, according to Dr. L. H. Dudley Buxton's report, is intermediate between the old Mediterranean type of Crete and the intrusive Armenoid.

The discovery of this temple tomb had a sequel of almost equal importance. A short section of paved way led to what was clearly the residence of the priestly warden of this Minoan 'Holy Sepulchre.' This contained a private chapel, with choir-stalls, chancel screens and, in the inner sanctum, an altar and sacred symbols of the cult.

Saturday, September 26.

His Honour Judge Dowdall.—*The Psychological Origins of Law.*

1. *James Ward.*—A lawyer dealing with psychology must choose his psychological expert, and I follow Ward. His articles in the 9th, 10th and 11th editions of *Encyclopaedia Britannica* made him, in the words of the *Times*, 'the father of modern English Psychology'; and his *Psychological Principles*, 1918, is, in the words of Professor Dawes Hicks, 'beyond all question the greatest and most original work on the science in the English language.' Forty years of active and accurate reading in British, Continental and American literature enabled him to state, with references, how far he agreed with other writers and, when he differed, to state why. The book is comprehensive and consistent, and free from 'subreptions,' so that every word will stand the pressure of exacting analysis.

It is a long way from the law of the jungle to the law of a civilised state, but the progress is continuous, and the path is throughout illuminated by Ward's principle of subjective selection, which, far more than Darwin's correlative principle of natural selection, is destined to be the guide of the social sciences and a hope for individual men. A meal fit for a king implies a king for whom it is fit as well as a meal; and he becomes a king by means of subjective selection—a selection made by him or his ancestors, and giving him a psychological environment which makes him what he is and which articulates with that of his fellows into a social whole.

2. *Subjective Selection.*—*Intellegere* means to pick out or select, and intelligence means selection by the individual subject of ends which will give him satisfaction and of means conducive thereto. But subjective selection is more comprehensive than intelligence; for subjective selection includes selection determined by hereditary disposition or acquired habit, whereas intelligence refers to special choice. The selection of cabbage by a caterpillar or of honey by a bee (or by a butterfly transformed from a caterpillar) is subjective selection, for the subject selects the object; but it is not called intelligence, though the hereditary disposition has, in Ward's view, been developed by the protracted operation of something like a humble kind of intelligence. And, furthermore, though the bee is hereditarily disposed to gather honey, he makes something in the nature of an individual choice between this rose and that.

The great difference between man and lower animals, and between civilised and uncivilised men, lies in subjective selection and its range, complication, and flexibility. In the process of this elaboration the discovery and development of language is of critical importance. For by language man identifies, and relates, and subjects to intellectual control, those recurrent elements of the changing scene which he (rightly or wrongly) conceives to be of importance to him. The history of legal institutions is the history of legal ideas and the history of legal ideas is sometimes best extracted from the implications of language.

3. *Community.*—Totemite language shows that early man conceives his corporate unity as somehow resembling that of a biological species. The psychology of a biological species presents many still unsolved problems; but the unity appears to be that of a community (*Gemeinschaft*) rather than a society (*Gesellschaft*), *i.e.*, it is based on disposition rather than device, or, to use Tarde's terms, on 'imitation' rather than 'invention.' Not that these are strictly separable, for dispositions develop, and are the result of experience arising from experiment, and require something in the nature of device for their realisation. But in a community the individual accepts the law of the community as he finds it; his task is indicated to him by circumstances with which he is familiar, and he is prepared to do it. In such circum-

stances Austinian sanction (which comes later, and itself depends on a free cooperation for its enforcement) has no place. Each finds his own living in the communal life and contributes his service in accordance with the scheme into which he is born and which he takes for granted. The scheme itself is, like language, discovered rather than made, and is not understood . essentials and unessentials are confused and the whole regarded as a gift of the gods, acts and forbearances being regarded as religious obligations. *Dharma* derives from *dhri*, to maintain : and *νομός* was a pasture and a portion long before *νόμοι* were deliberately made. Delinquency involves a bad *karma* or *nemesis* falling sometimes on the community, which may expiate it by killing the delinquent. The conception of law appropriate to such life is that underlying the Anglo-Saxon *æ* (which is the earliest English word of law, and cognate with aye, for ever), and the English word *law*, from Danish *lagu* and cognate words, *i.e.*, that which is laid down or settled like a lake or sedimentary rock.

4. *Society.*—Even communal life demands a certain amount of specially-concerted initiative, and religion assigns recognised duties to the divine chief. But routine is essential to communal life, and dislocation of it elicits a more thoroughly intellective process. This is most easily effected by submission to a dictator, who assigns duties to members of the society in accordance with a scheme which he devises as likely to be effective to the special end. The predatory life is a conspicuous illustration, but the same applies to all new adventure and progress. *Societas* derives from the same root as *sequi*, to follow, and *ἀρχή* means initiative. Later comes the deliberately selected *lex*, which is rather the creator than the creature of settled dispositions and which, at least until these are set, requires sanction to supply an incentive to those who are not sufficiently interested in the social end as to act without collateral inducement. Intermediate or concurrent, but starting earlier than the legislative period, is the deliberative assembly which decides or witnesses the decision of what is indicated as the right thing to do, either in respect of future plans or in dealing with someone who has done amiss. Hence we get the idea of *δίκη*, what is, or what ought to be, *indicated* as the *right*, *i.e.* straight, road to where you want to get. Decisions, unless conspicuously unfortunate, are apt to become precedents in similar circumstances. Habit grows in the body politic as in the individual. Man's rational nature looks to find some presiding genius or logical principle behind, and giving consistency to, these decisions—a god of justice, a law of nature, etc. But such is not easily found even in these days, and the discovery is fragmentary. 'The English common law consists of half a dozen obvious propositions, but unfortunately no one knows what they are.'

5. *The Group Mind.*—A group mind is constituted by an articulation of individual interests causing individuals to act as a group. Groups of all kinds are forming and dissolving incessantly, but special interest attaches to groups which maintain their identity although the individuals change. The fact that this can happen shows that the integration must be at the objective, and not at the subjective, end of experience. As a shareholder in the G.W.R., I may have the same objective interest as an unknown allottee long dead; but, apart from the fact that both he and I value dividends and consider the G.W.R. a good investment, we are distinct individuals. So, too, directors, engine drivers, etc., succeed one another in office, and by reason of their articulate interests keep the company going although they are separate individual subjects and have countless different interests which may articulate into all kinds of other groups, ephemeral or persistent, according to circumstances. Thus, the psychological implications of a persistent group is that a number of interests shall be organically interdependent and that there shall be a supply of individuals having those interests. Such a supply is secured by the operation of heredity and education and by the vesting of the relevant interests in individuals thus disposed to value them. In the case of primitive man, speaking perhaps a holophrastic language, without free ideas with which to manipulate and elaborate thought, in hourly touch with his fellows and with no one else, it is easy to see how irresistible a long and universally held tradition must be. For whatever else the tradition may include, it clearly must include a scheme of cooperation which supplies men, women and children with a livelihood.

Prof. V. Suk.—*Physical Anthropology and Ethnic Pathology.*

My present paper brings a very urgent proposition and suggestion to all physical anthropologists to collect data on ethnic pathology and on nutrition of people living

under primitive conditions, and to collect data as to the pathology of the different racial types amongst white people.

The total gain of the scientific efforts of Physical Anthropology is not altogether satisfactory; during the last decades an amazing number of figures, measurements, indices, angles, etc., has been collected all over the world, yet the most important and interesting part, the pathology of primitive people and the pathology with regard to the different anthropologic types, has been almost entirely neglected, and the same holds true with regard to the study of nutrition of primitive people and comparison with the nutrition of highly-civilised peoples. Yet this is a branch of Anthropology which may be of utmost importance and really practical utility with regard to the health status of the civilised peoples.

The many splendid experiments and studies on diet and disease, on the different deficiency diseases, on the occurrence of several diseases amongst primitive people living under special conditions and, further, the investigations on decay of teeth and food, the occurrence of some diseases amongst highly-civilised peoples, show us that the environmental factors seem to have a much greater importance and that in all probability the really racial factors in ethnic pathology, *i.e.* factors determined by heredity, are very few, if any at all, and that some of the apparent differences in this respect amongst the various peoples and races of our globe are due in the main to environmental factors.

Some work in this line has been done already (India), yet much more could be done if the travelling anthropologists instead of focussing their whole attention on measurements, many times of doubtful value, would collect data on the following items: climatic conditions of the respective place—the standard requirements of normal adult persons with regard to the composition of food, cereals, fat, meat, sugar, salt, etc.—the amount of food consumed fresh and the amount of food prepared by boiling, baking, etc., *i.e.* with due regard to the accessory food substances—the intake of stimulants, spices, etc.—the same with regard to growing children and data on breast feeding—the average height and weight of adult persons—the respective racial type—the average height and weight of children according to age and sex—the routine medical examination, with special reference to acute infectious diseases and immunity—nutritive and deficiency diseases with regard to native food—occurrence of rickets with regard to nutrition and seasonal changes, and of osteomalacy with special regard to environment—diseases of joints, bones and muscles, diseases of alimentary canal and of the respiratory system—diseases of blood and vascular organs and the question of blood pressure in primitive people—diseases of the nervous system and forms of insanity—diseases of generative organs and midwifery—diseases of skin, abnormalities in pigmentation and scar formation—malignant neoplasmata—diseases of the eye, nose and ear—diseases of teeth and gums, especially decay of teeth, involvement of the different teeth, occurrence of alveolar pyorrhoea, wearing down of crowns under physiologic conditions, etc., etc.

With regard to tuberculosis, the simple cutaneous tuberculin test in small children should be always made, for it gives a very good idea as to the extent of frequency of tuberculous infection in a community, and the same holds true for the simple tests for syphilis, the flocculation tests (although they are, of course, not as exact as the routine complement fixation tests, yet for field work they suffice).

Amongst civilised peoples there is a great need for collecting exact data as to the pathology of the different types of human communities to determine exactly the occurrence of diseases amongst nordic types, alpine, mediterranean types, etc. To this end especially, all the post-mortem examinations, which render the most exact diagnoses should always bring data on the racial type, and this same should be done in examining for instance, drafted men, schoolchildren, students, etc. We have already enormous numbers of anthropologic observations amongst the classes mentioned, yet unfortunately, with few exceptions, they bring us, in addition to some very interesting data on the racial composition of large populations, also a mass of almost useless figures. Opportunities for studying the health status of large communities, with due regard to their social status and, together with it, with regard to their racial characters, have, unfortunately, with a few exceptions, been omitted.

Prof. C. Daryll Forde.—*Hopi Agriculture and Land Ownership.*

Hunting and gathering have played little part in Hopi economy, while the herds of sheep they have reared since Spanish times are insignificant compared with those

of the Navajo. Dependence on agriculture, despite the desert conditions, is possible on account of the peculiar character of the flood water run-off from the mesas. Flash floods following the storms descend the canyons and scarps, fan out over the lower land forming washes, without cutting a definite channel. Apart from the gardens at springs which *may* be due to Spanish influence, there is no irrigation, merely the planting of patches of ground likely to be flooded. In recent years, however, definite channels or arroyas have been cut across some of the washes, seriously reducing the flooded area and destroying much valuable land. This may in part be due to over-grazing of the mesas by the Navajo and Hopi flocks promoting more rapid run-off, but other factors may be operating, since cycles of arroya cutting have occurred in the past. One, indeed, appears to coincide with the abandonment of many Pueblo sites in the south west at the end of the Great Pueblo period.

Arable land is therefore rather rigidly restricted, and there are definite, although disputed, boundaries between the lands of each village. The village lands are parcelled out into major areas nominally owned by the clans, and marked by boundary stones roughly incised with clan symbols. The clan lands are not concentrated in one place but subdivided into several lots in different parts of the village territory. Each family has field patches within several of its clan's lands. This produces a superficial resemblance to open-field systems and distributes the risk of crop failure as all fields are subject to a double hazard, *i.e.* of being washed out by too fierce a flood or of receiving no water in a particular year.

The matrilineal clan organisation is reflected in the control of land. The family plots are theoretically controlled by women, the older women disposing of fields to their daughters as they marry and have need of them. Since, however, all field work except at harvest time is done by men, the husbands and unmarried sons of the clans-women, the actual cultivation is undertaken by men who have no personal lien on the fields they cultivate, and indeed in the case of husbands, who are not themselves members of the clan. The clan leaders reapportion fields as need arises, handing over some of those of dwindling families to others which are growing in numbers and have more labour to cultivate them. Certain fields are also reserved for the chiefs of certain societies; these are cultivated by the members of the society in question.

This scheme is, however, modified in practice by the acquisition by men of fields outside the traditional clan lands. This tendency has increased since the introduction of the horse and wagon, followed by the cultivation of fields at considerable distance from the village. In some instances men obtain control of fields within the clan lands. These are usually inherited from their mother or maternal relatives and are sometimes passed on to the man's sons and daughters. In this way fields have been alienated from the clan. This type of inheritance is recognised as inconsistent with the theory of clan ownership, but is rarely interfered with at the present time. It may be a recent development connected with the decline of population and the diversion of much male labour into new pursuits consequent on Americanisation.

Dr. R. E. Mortimer Wheeler.—*Summary of the Current Excavations on the Prehistoric and Roman Sites of Verulamium (St. Albans).*

Monday, September 28.

Prof. Henry Fairfield Osborn.—*The Geologic Age of* Pithecanthropus, Eoanthropus, *and other Fossil Men determined by the Enamel-Ridge-Plate-Grinding-Tooth-Measurement of the Proboscidea with which they were geologically contemporaneous.*

The association of man with the mammoth in closing Pleistocene times was one of the earliest archæological discoveries; it has become a tradition. One of the most striking generalisations from recent palæontological exploration and research is that man has been the travelling companion of members of the Elephant super-family, Elephantoidea, for a very long period of time, as it now appears from the close of Pliocene time, roughly estimated as 1,250,000 years before our era.

First, the natural inference is that Tertiary man inhabited the kind of partly forested, partly open upland country bordering streams and rivers that was also attractive to primitive elephants. The second inference is that only in the sands and

gravels of these river and stream borders, subject to occasional floods and cloudbursts, are we likely to find the fossil remains of primitive man and of primitive elephants intermingled in the same fluviatile deposits. We recall that the Piltdown gravels of Sussex, obviously of low gradient and fluviatile origin, yielded fossil remains of *Eoanthropus dawsoni* intermingled with those of a supposed 'Stegodon,' which now proves to be a true primitive Pliocene elephant, also with tooth fragments of a 'Mastodon,' which now proves to be a highly characteristic Pliocene mastodon of the genus and species *Anancus arvernensis.* Again, in far-eastern Java, cranial and dental fragments of *Pithecanthropus erectus* were found in river-deposited sands and gravels associated with the remains of a primitive elephantoid known as *Stegodon airawana*, also with the more progressive elephantoid related to the *Elephas namadicus* of the Siwaliks of India. Still more recently the Vaal River gravels of South Africa have yielded on lower and higher levels three elephantoids of the genus *Archidiskodon*, as recently described by the present author in *Science*, but no fossil human remains have thus far been found of equal antiquity with these primitive elephants of the Vaal River terraces.

It follows that along elephant migration routes we may confidently look for human migration routes. There is also evidence that, prior to the Piltdown period, man hunted the elephant or the mastodon and used the bones in the fashioning of tools, but this disputed archæological problem need not be discussed in the present paper.

Our attention must now be devoted to the bearing which the association of fossil remains of man with those of primitive elephants or mastodons has upon the all-important question of the geologic antiquity of man.

From closing Pliocene time onwards man and the elephants evolved side by side. While in man the grinding teeth were relatively stationary, in the elephants the elevation of the distinctive ridge plates composed of dentine, sheathed in enamel and bathed in cement, *evolved with extraordinary chronologic precision within members of each generic phylum, at precisely the same rate whether in the same region or in widely separated regions.* For example, we observe, as to the wide geographic distribution of the ancestral 'southern' or 'imperial' mammoth such as *Archidiskodon planifrons* and *Archidiskodon subplanifrons*, that the ridge plates of a given geologic period correspond exactly in height whether measured in South Africa, in southern England, or in the Siwalik hills of India. Thus the evolution of the grinding teeth of primitive elephants afford an absolutely reliable chronometer whereby the antiquity of the successive stages of human evolution even in widely separated geographic regions can be precisely determined and correlated.

The bearing on anthropology of these priceless enamel chronometers is nowhere more important than in helping us to determine the respective geologic ages of the *Pithecanthropus* of Java and the *Eoanthropus* of Piltdown. When *Pithecanthropus* was discovered it was almost unanimously regarded as of Pliocene or lower Pleistocene age. When *Eoanthropus* was discovered it was regarded as of undetermined lower to middle Pleistocene age. It now appears that both of these determinations are probably wrong. From the observations of Dubois, Dietrich, Freudenberg, Hopwood, Bather, and Osborn in his forthcoming Proboscidea Memoir, it would seem that *Pithecanthropus* was the companion of *Stegodon airawana* and *Palæoloxodon namadicus*, hence of middle Pleistocene age; *Eoanthropus dawsoni* was the companion of *Archidiskodon planifrons* and *Anancus arvernensis*, hence of upper Pliocene or lower Pleistocene age.

The new preliminary ganometric estimates of the age of the known fossil men and their contemporary flint industries or cultures are as follows :—

Eoanthropus dawsoni	1,000,000	Pre-Chellean.
Palæanthropus heidelbergensis . . .	920,000	Pre-Chellean.
Sinanthropus pekingensis, of China . .	920,000	Unknown.
Pithecanthropus erectus, of Java . . .	500,000	Unknown.
Palæanthropus neanderthalensis, of Ehringsdorf	135,000	Pre-Mousterian.
Palæanthropus neanderthalensis, of Neanderthal	70,000	Mousterian.
Homo sapiens cromagnonensis . . .	30,000	Magdalenian.
Homo sapiens nordicus, of S. Scandinavia .	12,000	Campignian.

The above estimates are based upon the most recent calculations by geologists that the whole Pleistocene or age of man, including four glacial and three interglacial epochs, extended over not less than 1,000,000 years prior to the 15,000-year period when the Scandinavian ice-sheet retreated so far northward as to leave southern Scandinavia, known as Scania, free for human habitation.

Prof. Sergio Sergi.—*Neanderthal Man in Italy: the Saccopastore Skull.*

The Neanderthaloid skull of Saccopastore (Rome) found in 1929 has been the subject of successive communications as it was gradually freed from the gravel which covered it. Though somewhat damaged, it is one of the best preserved neanderthaloids, especially at its base, which is entire. It appears to belong to a woman about thirty years old, as may be inferred from the condition of the teeth and the sutures. The cranial capacity does not exceed 1,200 c.c., and, along with that of the Gibraltar skull, the lowest of its type. The development of the face in comparison with that of the brain case is still more marked than in the La Chapelle skull. The foramen magnum is set rather forward, as happens in the living races of mankind. The plane of the aperture of the foramen slopes forward with a very marked degree of negative inclination. For these reasons the individual must certainly have held her head erect, and walked like modern men. The different position and inclination of the foramen of the La Chapelle skull must be attributed to artificial causes, especially to a defect in the reconstruction of the base. The horizontal profile of the skull coincides in a surprising way with that of La Chapelle. The maximum horizontal circumference is 520 mm., very near that of La Quina (515). The approximate cephalic index is 78·4, very near to that of Gibraltar (77·9). The curve of the profile is unsymmetrical. The supraorbital ridges have been destroyed, but they seem to have been widely extended sideways. The region of the lambda is occupied by a complex arrangement of supernumerary ossicles, a common occurrence in Neanderthaloids, which shows the morphological instability of the super-inial part of the occiput, which is in course of evolution.

The vault is flattish, with a low angle of frontal slope (74°) and also of occipital (70·5°), with values very close to Gibraltar. The platycephaly is well marked, with height-length index 60·2, which equals that of La Quina (60·09?), and is very close to Gibraltar, and with height-breadth index of 76·76, the lowest hitherto encountered.

The calvarial index determined on the glabella-inion line is 44·30, which indicates a less marked platycephaly than that determined on the glabella-lambda, which has the value of 26·50, very near that of the La Quina skull (25·3). This results from the prominence of the part of occiput included between the lambda and the inion, in which the parieto-occipital sagittal curve resembles that of the Gibraltar skull and differs from that of the skulls of La Quina and La Chapelle, which project wedge-shaped backwards. The prominence of the inferior frontal gyrus is well developed about the pterion, a sign of special development of the pars triangularis and of the foot of the third frontal convolution. In norma occipitalis the outline is cycloid and unsymmetrical; its lower limit is marked by the strong relief of the occipital protuberance.

The face is very large and shows a notable morphological height (85), very large orbits (left breadth 45, height 39? orbital index 86, area of the aperture 1755), piriform aperture wide and low (naso-spinal distance 59, greatest width 31, nasal index 52·54); bridge of nose extraordinarily prominent in front by the very marked forward development of the maxilla. The nasal bones are wide and meet at an obtuse angle. Viewed from the side the nose is rather concave below the fronto-nasal suture. The anterior surface of the maxilla is swollen and projects forwards. The alveolar arcade is very broad, short and rounded. The remaining teeth—the three molars and the second premolar on the left, the second and third molar on the right—are large; the first and second molars of equal size, the third smaller, all with a considerable amount of wear on the masticating surface, more marked on the lingual side. Among neanderthaloid finds that of Gibraltar most closely resembles that of Saccopastore in its dimensions generally and in its general and special morphology.

The Saccopastore skull was found with the remains of large fossil mammals, *Elephas antiquus*, *Rhinoceros Mercki*, *Hippopotamus major*, in river or lake deposits rich in volcanic materials which belong to the period at which the lower valley of the Tiber was taking shape to assume its actual topographical appearance. These

beds, according to the observations of the geologists of Rome, precede the final stages of volcanic activity in Latium, and mark the close of a well-defined stage of the local volcanic outbreak. They can be synchronised with the Riss-Wurm inter-glacial interval.

With the discovery of this Roman skull an anthropological void is filled in the pleistocene of Italy. Certainly it is to Neanderthal man that belong the most certain vestiges of Mousterian industry, found in the alluvial districts of the Roman Campagna. He was contemporary with the great extinct mammals which characterised the so-called warm-climate fauna, and witnessed, if only for part of the time, the volcanic occurrences which assailed the district which he inhabited.

The Neanderthaloids, the first discoveries of which seemed to locate them in a limited region of Europe, are now shown by the subsequent and more recent traces of the last few years, culminating in this Roman find, to consist of different varieties more or less related, which, originating perhaps in the African Continent, spread over all the Mediterranean basin. Italy, plunged in the midst of that sea, began to exercise then, from the first apparition of the most ancient stock, that function as a centre of attraction to peoples, to which it was fated by its geographical position and surrounding conditions, which continue to develop in the ages which follow, and culminate in the historic periods. Together with that function, it served as a bridge and a station for fresh morphological and cultural evolution of the men who were advancing to the conquest of European lands.

Dr. R. Broom.—*Early Man in South Africa.*

Prof. C. G. Seligman, F.R.S.—*Human Hybrids.*

Dr. C. van Riet Lowe.—*Early Palæolithic Cultures in South Africa.*

Prof. Mrs. A. C. L. Donohugh.—*A Luba Tribe in Katanga: Customs and Folk-lore.*

Afternoon.

Prof. G. Elliot Smith, F.R.S.—*Peking Man.*

Tuesday, September 29.

Dr. Campbell Thompson.—*Three Seasons' Excavations at Nineveh.**

These three seasons were undertaken on behalf of the British Museum at Nineveh, initially with funds provided by that institution, Miss Gertrude Bell's bequest, the Percy Sladen Memorial Fund, Merton College, Oxford, the Society of Antiquaries and others, and finally by Sir Charles Hyde, Bart., who has borne all the charges of more than two seasons, thus making possible yet another season. I was accompanied by Mr. Hutchinson for the first two, and by Mr. Hamilton for the third, and unofficially by my wife, Miss Campbell Shaw, and Miss Marion Hallett for various periods.

It will be remembered that the Palaces of Sennacherib and of Ashurbanipal had been discovered in the last century. In 1904 Dr. L. W. King and I had found a second small palace of Sennacherib, and after he had gone home the Temple of Nabu came to light. In 1927 this latter was cleared, the work showing that it had been built on a large and solid platform of unburnt brick round a rectangular courtyard. The 1929 season excavated the site of the Palace of Ashurnasirpal, which had been found during the last few days of the 1927 season, a building so much restored by successive builders, who had re-used the valuable burnt bricks of which it was composed, that

* With Mr. R. W. Hutchinson (1927, 1929) and Mr. R. W. Hamilton (1930). A film on this subject was shown at the Imperial Institute.

it was almost impossible to trace the original building. The 1930 season (last winter) has almost finished the clearing of the great Temple of Ishtar.

During these seasons a house built by Sennacherib for his son, and some very interesting early arched tombs, were also discovered. Stone cuneiform inscriptions are numerous, and, among other finds, are a practically perfect prism of Esarhaddon, pieces of a cuneiform tablet giving in epic form the events at the end of the Kassite wars, a life-size bronze head of 3000 B.C., and an almost complete stone cylinder of about 1830 B.C., giving the account of the rebuilding of the Temple of Ishtar. The painted pottery of the third millennium has appeared in fair profusion, and there have been found numerous pieces of cylinders or tablets, quantities of beads, coins (four hoards) and several cylinder seals.

Mr. H. P. Vowles.—*The Early Evolution of Power Engineering.*

Apart from the use of wind for ship propulsion, the earliest method of utilising the forces of nature appears to have been the river-rotated water-raising wheel. This may have been known in Sumerian times. Vitruvius, however, gives the first unmistakable reference to wheels of this type, also describing the water-wheel driving millstones through toothed gearing.

There are at the present day several primitive forms of water-mill, none of which involves gearing. These may have preceded the geared mill of Roman times. One of them, operating pestles through a trip-hammer mechanism, appears to be referred to by Pliny.

Heron of Alexandria describes what has every appearance of being a wind-mill, used to move a piston by a trip-hammer arrangement similar to the pestle and mortar water-mill. The wind-mill is not heard of again until the tenth century A.D., when authentic references to such mills are made by three Persian writers in Arabic. There are large numbers of wind-mills of very primitive design in eastern Persia to-day. Yet another form possibly of early origin is used in China. The earliest authenticated date for a wind-mill in Britain is A.D. 1191.

A method of obtaining rotary motion by steam is described by Heron. This is an adaptation of the stationary eolipile already in common use in his time. There were no further developments of note in connection with steam until Heron's book was translated into Latin and Italian in the sixteenth century.

Some brief notes are added relating to early knowledge of magnetism and electricity, from Thales in Europe and Susruta in Asia in the sixth century B.C. down to the work of Faraday 100 years ago.

Sir W. M. Flinders Petrie, F.R.S.—*Excavations at Old Gaza.*

The site of Tell Ajjul appears to be the old city of Gaza, on the side of the Wady Ghuzzeh; it was deserted about 2000 B.C., when the present Gaza was established, five miles to the north. It is thirty-three acres in area, all of the Middle and Early Bronze Ages. The latest period is that of the Hyksos, who were buried with their horses. They had no separate civilisation, but used the products of each country which they overran. Beneath them were the Canaanites of the twelfth dynasty, with a high civic life, having a regularly planned city, with shrines and baths. They used skilfully made gold jewellery, and their weights showed trade with Egypt, North Syria and Babylonia.

Before them there was a desolation of the region, due to the passage of a people from North Syria down into Egypt, forming the seventh and eighth dynasties. These invaders destroyed the old Copper Age civilisation, which is marked by immense fortifications (like those of Homs), a long tunnel, pottery inheriting from the neolithic, and the beginning of copper weapons. This was about the earlier pyramid period, in the fifth and sixth dynasties. The civilisations of Palestine are now linked with those of Egypt at each stage.

Prof. and Mrs. C. G. Seligman.—*The Social Organisation of the Nilotes.*

By Nilotes we mean the tall, very dark-skinned, dolichocephalic negroids of the White Nile and its tributaries, whose life and interest centre round the keeping of cattle. Of these tribes the best known are the Shilluk, Dinka and Nuer, our

knowledge of the Anuak being very restricted. The Bari, though tall and dolichocephalic, differ not only in appearance and speech, but in disposition as well as in many cultural characters.

The Shilluk form a nation under 'a divine king,' in whom the spirit of Nyakang, the founder of the nation and its first king, is immanent. No such political organisation is found among the other Nilotic tribes. The large Dinka units, which may fairly be called tribes, act independently of one another and differ considerably both in dialect and customs. The Nuer probably differ no more from the Dinka than do some tribes of the Dinka from each other.

Among the Shilluk the strength of the central political and religious organisation dominates the social organisation. Rain ceremonies are performed by the king, who is not allowed to die or to become senile lest his weakness should affect the fertility of his people and their flocks. He is therefore killed ceremonially while still in his prime. The rain-makers of the Dinka are likewise divine chiefs and killed ceremonially, but much later in life and usually at their own request. The Nuer and Anuak—as far as our present knowledge goes—do not kill their rain-makers, about whose position we know relatively little, though among the Anuak there are regalia corresponding to the Shilluk insignia of kingship which go back to Nyakang, the founder of the nation.

All these tribes are divided into exogamous clans. The Dinka and Nuer are definitely totemistic, the common legend being that the totem animal (when the totem is an animal) and clan ancestor were born as twins. Plants are also totems, and among the Dinka such phenomena as fire and thunder. Totemism, if it really exists among the Shilluk (for it clearly is not universal), may be due to old Dinka influence or modern Dinka contact, for intermarriage and even Dinka shrines are not uncommon in the southern part of the Shilluk country.

Among all these tribes there is a cult of sacred spears, and, it is probably true to say, a high god—commonly otiose, but relatively active among the Dinka—associated with the sky.

The kinship system of the Nilotes is peculiar ; it is descriptive in the true sense of the word.

The Nilotes are resistant to foreign influence and culture to a degree that seems to constitute a definite psychic character, so marked, indeed, that it can scarcely be accounted for by the inhospitable nature of their country.

In contradistinction to the Nilotes, their southern neighbours the Bari have chiefs who, though rain-makers, are not divine kings but rather medicine men, who, if they fail to produce rain, are threatened and even killed. Here the rain-making technique is associated with stones, usually of quartz. Totemism is well developed, and the kinship system has not the true descriptive character of the Nilotes. The Bari, though they have suffered considerably from foreign aggression, are quick to adopt foreign objects and culture, and psychically seem to stand entirely apart from the aloof Nilotes.

Dr. E. E. Evans Pritchard.—*The Nuer of the Nilotic Sudan.*

Evening.

Huxley Memorial Lecture of the Royal Anthropological Institute, by Dr. G. Thilenius, on *Some Biological View-points in Ethnology.*

(By the courtesy of the Institute, members of the Association were admitted to this lecture, which was purposely arranged to take place during the Association's Meeting.)

Wednesday, September 30.

Miss M. A. Murray.—*An Ossuary of the Bronze Age in Minorca* (with preliminary note by Sir Arthur Keith, F.R.S.).

Miss W. Blackman.—*Egyptian Tattoo Designs, their Magical and Decorative Significance.*

Capt. G. Pitt-Rivers.—*Anthropological Approach to Eugenics.*

While Anthropology as a background of all sociological education has, from the inception of that science, been neglected, there is a growing appreciation of the positive contributions to national health and well-being that anthropological research can make. The applications of a science must remain indeterminate, and the systematic extension in its scope is likely to remain undeveloped, until its methodology has been satisfactorily formulated. For this reason alone Anthropology has remained for so long a very young science. It is significant that some of the discussion on the methodology of the science in 1875, and some of the criticism, is as relevant and valid to-day as it was then.

Evolution, regarded in terms of growth forces in a changing milieu, is a process of adaptation to modifications in environmental conditions.

Inbreeding and outbreeding as factors in adaptation open out a field so far very little explored. These factors affecting Constitution clearly are related to problems of Culture and the social organisation of exogamic and endogamic customs. In these processes the rates of elimination and selection are measurable in demographic terms.

Variation in the survival rates of distinguishable groups touches on all the anthropological problems concerned with the influences, cultural, physical and environmental, that affect the extinction or the survival of ethnic types or variations.

Quantitative changes bring about qualitative changes. In so far as these influences are capable of social control or direction they are also the problems of eugenics. Eugenics is, therefore, seen as the study of those forces amenable to social control, which can influence the fertility and survival rate of variations of type in a population. The significance of demographic data. A consideration of group variations in maternal mortality, and in infant mortality. Examples drawn from variations in sex-ratio. The usefulness of anthropological researches is exemplified in its eugenical bearings.

Miss L. Mair.—*Economic Man in Primitive Society.*

Modern Economics the science of human acquisitiveness ; its method to isolate the acquisitive side of human activities and study this on the assumption that in all his economic relationships man is predominantly actuated by calculations of material gain. In modern society individualistic acquisitiveness plays such an important part that this is approximately true; economists of course admit that the abstraction 'economic man' never existed.

But primitive economics cannot be described or reconstructed by reducing modern economic laws to their simplest terms, because among primitive peoples—largely owing to the limited possibilities of acquiring wealth—other than economic considerations play a much more important part in their economic relationships. This point can be illustrated from various primitive societies in East Africa.

Three main non-economic elements emerge :

1. The religious attitude towards possessions—land or cattle—(this is coupled with the impossibility of obtaining, in primitive conditions, any economic equivalent for the fundamental means of livelihood).

2. The principle of the kinship group as economic co-operative (especially land-owning) unit ; hence various purely non-economic relationships have to be created on the admission of strangers to cultivate land, or herd cattle. Adoption ceremonies ; duties towards owner ; return made not in any sense an economic equivalent, or so regarded ; moral obligation on owner not to dispossess tenant.

3. Most important ways in which wealth circulates not primarily economic ; payment of bride-price, payment of compensation, gifts to chief from which he is expected to support the poor and needy ; general principle of responsibility of a group towards its poorer members.

Dr. Cipriani.—*The Origin of the Bantu.*

SECTION I.—PHYSIOLOGY.

Thursday, September 24.

DISCUSSION on *The Physiological Basis of Sensation.* (Prof. E. D. ADRIAN, F.R.S.; Prof. FRANK ALLEN; Dr. C. S. MYERS, C.B.E., F.R.S.; Prof. D. W. BRONK; Dr. R. S. CREED; Sir J. HERBERT PARSONS, C.B.E., F.R.S.; Prof. H. HARTRIDGE; Prof. H. H. WOOLLARD; Prof. H. E. ROAF; Dr. E. P. POULTON.)

Prof. E. D. ADRIAN, F.R.S.

What sort of change takes place in the sense organs and their nerves when the skin is touched and what happens in the brain to make us feel? The events in the brain which cause sensation are still uncertain, but we know what kind of messages are transcribed by the sensory nerves, for they can be detected electrically. They consist of a succession of impulses in the nerve fibres, each impulse being a brief surface reaction which spreads down the fibre accompanied by a change of potential. Their frequency varies with the intensity and abruptness of the stimulus, but in any one fibre they are all alike, and they are of the same general type in all nerve fibres. Then in its main outline the working of the sensory apparatus seems to depend on physical and chemical changes of fairly simple character, but there are many outstanding questions when we consider all the different kinds of sense organ and different qualities of sensation. Problems concerning the eye and ear, the different sense organs in the skin, the nervous mechanism of pain, &c., are in urgent need of solution, and their discussion may show the complexity of the sensory apparatus, and may help to explain the variety of sensation which we experience in spite of the simple nature of the messages which are sent to the brain.

Dr. C. S. MYERS, C.B.E., F.R.S.

Relatively little light seems likely to be thrown on the physiological basis of sensation by the study of sensory nerve fibres of sensory end-organs. We are, at present, almost wholly ignorant of what occurs in the more important cerebral sensory 'centres.' Although these centres are *essential* for sensation, they are not to be regarded as the *seats* of sensation. For sensation depends on the relation of their activity to that of that vast integration of central nervous activity on which 'self-activity' is dependent: where no self-activity is involved there can be no consciousness of sensation. The biological function served by the consciousness of sensation is the control of its specific efferent responses by the organism. Sensations have become differentiated from one another because the response is different for each. When the patterns of response for two different stimuli are too nearly identical, the respective sensations are indistinguishable: just indistinguishable (or just distinguishable) differences in sensation do not necessarily imply the activity of the same (or different) end-organs. It is conceivable that, just as muscular responses may be regarded as clonic or tonic, or as combinations of these, so certain sensations may be fundamentally classified as 'clonic' (*i.e.*, as explosive, all-or-none, punctately distributed, uni-phasic, and easily fatigable), and as 'tonic' (*i.e.*, as enduring, graded, diffusely distributed, bi-phasic, and subject to adaptation and contrast). On the future study (*a*) of the integration of these two and perhaps other elementary classes of sensation, and (*b*) of the central responses involved, immediate additions to our knowledge of the physiological basis of sensation seem likely to depend.

Friday, September 25.

Dr. MARIE STOPES.—*Some Physiological Facts from Ten Thousand Clinical Cases dealing with the Control of Conception.*

Ten years ago the vaginal use of Quinine was widespread but without scientific investigation or supervision: clinical observation thereon began in 1921: the absorption of quinine by the vaginal walls, as distinct from the uterine, established in the human female: deleterious effects in approximately 5 per cent. of subjects: comments

on Prof. A. Thomson's theory of uterine absorption : technique of procedure establishing vaginal absorptive capacity.

The nature of vaginal secretions in the human female : chemical compounds formed by vaginal secretions : bearing of their nature on contraceptive technique : research desiderata.

Muscular reactions of the non-pregnant uterus : discovery of and clinical verification of previously unsuspected cervical mobility in the human female : comments and criticisms of laboratory work on animals from which others have drawn deductions without taking into account certain conditions essential to a successful experiment : some confirmatory data from medical practice.

Dr. C. P. Blacker.—*The Rôle of Genetics in the Future of Preventive Medicine.*

Profound differences of opinion exist among experienced no less than among lay people as to the relative influences of heredity and environment in determining human type. These differences are attributable to prejudice no less than to ignorance. Philosophic, religious, familial and political prejudices are considered.

The general scope of our ignorance is defined. It is argued that this can be much reduced by disseminating an interest in pedigree study. If everybody carefully studied and impartially compiled their family histories to-day, there would be available for future generations an abundance of material utilisable for scientific purposes that would enlighten us as to the rôle of genetics in preventive medicine.

Prof. E. P. Cathcart, F.R.S.—*Man Values or Family Coefficients.*

Prof. A. V. Hill, F.R.S.—*The Recent Revolution in Muscular Physiology.*

Up to the end of 1926 current theories of muscular contraction were expressed in terms of lactic acid and of the energy provided by its formation from glycogen. The discovery of creatine-phosphoric acid at the beginning of 1927 brought the subject to a new stage which reached its climax in the discovery by Lundsgaard in 1930 that muscles may be so treated with iodo-acetic acid that they contract apparently normally but with no lactic acid formation at all. The present view is that the immediate chemical change in an excited muscle is the breakdown of creatine-phosphoric acid, this being followed immediately by the reformation of the latter at the expense of energy liberated by lactic acid formation, and later by the oxidative restoration of the lactic acid to glycogen.

The new facts require a readjustment and re-statement of the conclusions from older work. They do not, however, as is sometimes asserted, upset it all. Most of the experimental facts and of the conclusions from them, particularly in relation to the recovery process, are independent of the precise mechanism involved. The present position is critically examined, particularly in relation to the case of muscular exercise in man.

Prof. C. Heymans.—*The Aortic and Carotid Sinus Nerves in the Regulation of Circulation and Respiration.*

Prof. J. Barcroft, F.R.S.—*The Limits placed by Altitude to Physical Exercise.*

The work of Italian researchers has shown that man can live at an atmospheric pressure of about 110 mm. Hg if he breathes, not air, but oxygen. At that pressure he can do little or no work. Recent researches by Barcroft, Douglas Kendal and Margaria have, however, shown that at 170 mm. pressure breathing oxygen, man can step up 1,000 feet in an hour. As the work consisted of stepping on to a box, it was incidental that he stepped down as often as he stepped up. One hundred and seventy millimetres is a much lower pressure than that of the top of Everest. Therefore it follows that, given a supply of oxygen into the respiratory passages, the feat of climbing at the highest altitudes on the earth's surface is not impossible, and, apart from unknown difficulties in the terrain, the problem of climbing Everest is less one for the mountaineer than for the engineer. Not the engineer in general, but a

particular sort of engineer, one who specialises in the apparatus of respiration, such, for instance, as the diving engineer or the technical chemical defence departments of the National Armée. The amount of oxygen necessary is about a litre and a half per minute. If an ascent of 5,000 cubic feet were made in five hours the oxygen actually absorbed by the climber would be 450 litres. Suppose he breathed half that amount on the descent :—

$$\begin{array}{l} 450 \\ 225 \\ \hline 675 : \text{ say 700 litres, say 30 cubic feet.} \end{array}$$

The problem then may be subdivided thus :—

1. How is a man to carry 30 cubic feet of oxygen ?
2. If he ' rebreathes ' it, how is he to get rid of the carbonic acid ?
3. How is he to cope with the incidental difficulties proper to the use of apparatus such as the occurrence of water vapour, which freezes in inconvenient places ?
4. An estimate of the margin necessary over and above the theoretical quantity.

Seven hundred litres weigh approximately a kilogram, so that the weight of the oxygen itself is trifling, but the weight of an ordinary cylinder which would carry 30 cubic feet of compressed gas is too great. What, therefore, are the lightest cylinders into which it could be compressed ? Were the problem one of providing a supply of air for a diver or a respirator for a soldier, a competent authority would sit down to face it on a basis of exhaustive experiment and extended drill. In the end I have little doubt that he would solve this problem and with less expenditure than is entailed in the equipment and expenses of successive expeditions to the Himalayas—expeditions which cost not only money but a toll of valuable lives.

Mr. N. E. Odell, member of the Everest Expedition, 1924.

Dr. Raymond Greene, member of the Kamet Expedition, 1931.

Monday, September 28.

Presidential Address by Dr. H. H. Dale, C.B.E., Sec.R.S., on *The Biological Nature of Filtrable Viruses* (see page 172).

Discussion on *The Biological Nature of Filtrable Viruses* :—

Dr. T. M. Rivers.—*The Nature of Animal Viruses.*

For discussion Dr. Dale has chosen to characterise viruses by three negative properties, namely, invisibility by ordinary microscopic methods, failure to be retained by filters impervious to ordinary bacteria, and failure to propagate themselves in the absence of susceptible cells. I prefer a positive characterisation of the viruses, one emphasising the intimate relation between them and their host cells. Nevertheless, I have followed the cue given by Dr. Dale, who, incidentally, has posed many questions that cannot be answered at the present time.

Data concerning the filterability, visibility and cultivation of viruses are sufficiently adequate in quantity, but in many instances distinctly lacking in quality. Frequently, when observations are accurate, conclusions are unwarranted. According to reports, many of which have come from eminent investigators, most of the viruses have been seen and have been cultivated on lifeless media. If these reports are correct it is obvious that such viruses are autonomous living agents, and that further discussion of their biological nature should deal with their place in the scale of life or with their relation to other forms of life. Evidently most workers do not consider the reliable data already acquired sufficient proof of the nature of the viruses. Therefore, if much of the information in the past has been incorrect or misleading, there is no reason to suppose that all of it garnered in the future will lack these faults. Consequently, investigators working in the virus field should demand experimental accuracy and intellectual integrity.

A point in question is the increasing demand, made by certain workers, that viruses are merely filterable stages in the life-cycle of ordinary bacteria. It is

becoming more and more evident that a clinical entity may not be caused by a single agent. For instance, some virus maladies are induced not by one virus but by two, while others appear to be produced by the concerted action of viruses and bacteria. Therefore the fact that visible bacteria are consistently found in certain virus diseases does not mean that they are always harmless secondary invaders, nor does it necessarily indicate that they are the visible form of the viruses. At present there is no conclusive proof that viruses are filterable forms of bacteria.

The pathological pictures observed in virus maladies have an indirect bearing upon the nature of the agents inciting them. Cells injured by many viruses frequently exhibit inclusion bodies, concerning the nature of which numerous conflicting opinions exist. In some diseases, such as wilt of caterpillars, there is evidence that the infective agents are not associated with their characteristic inclusions, while in others, such as fowl-pox, large amounts of the viruses are undoubtedly present within these structures. In spite of definite proof that viruses are present in certain types of inclusions, there is still doubt regarding the organismal nature of the small coccoid bodies of uniform size found within them.

Other features, hyperplasia and necrosis, observed in the pathological pictures induced by viruses are fully as important as are the inclusion bodies, and it is because of them that such phenomena as lysis of bacteria, fever blisters, smallpox, warts and tumors can be discussed logically at the same time. Such a statement naturally opens up the question of the etiology of malignant neoplasms. Undoubtedly a number of fowl tumors are caused by agents separable from cells, and, although there is as yet no proof that human cancer arises in this way, the possibility is one that must be seriously considered, and offers a fertile field for work. The fact, however, that some tumors are produced by filterable agents is by no means conclusive evidence that all neoplasms arise through the activity of such agents.

Dr. J. Henderson Smith.—*Plant Viruses.*

The viruses which occur in plants have essentially the same characters as the animal viruses. They have the small size, the capacity for indefinite multiplication and spread, the infectivity, the inability to increase in the absence of living cells, the production of intracellular inclusions, the differences among themselves in specificity and resistance, and so on. There are differences in effects, *e.g.* plants are unable to develop an active immunity; but there is no doubt that in their nature they are similar to the animal viruses. What that nature is remains equally uncertain. The relevant evidence is of the same kind, *e.g.* for the small size the microscopic invisibility, and the results of differential filtration. The latter method, however, can give no precise data at present as to the exact dimensions, since ability to pass an orifice is dependent as much on capacity for distortion as on actual size, and we have no information as to the rigidity of the virus particle. In at least one case it seems definitely established that the intracellular inclusions are formed by the aggregation of particles of altered cytoplasm.

Exact quantitative data are deficient on the plant side; and it is only recently that it has become possible satisfactorily to free the viruses from the other ingredients of the plant juices and isolate them in a relatively high degree of purity, which is an essential preliminary step.

There is no fact certainly established which is incompatible with the organismal hypothesis; and some which seem more easily explained on such a theory than on any other. It is certain, for example, that some of the plant viruses multiply in their insect vectors, and that there is in some cases an interval during which the insect after infection is not capable of transmitting the disease, the so-called incubation period. Again, in some commercial crops, *e.g.* sugar-cane, millions of plants are grown annually, in which unlimited opportunity is given for abnormal cellular constituents to originate, but virus disease does not appear, unless it is introduced from without; and its rapid spread, when so introduced, shows that, if an infective cellular abnormality did arise, it would reveal itself. The analogies for the parasitic view are very strong, and while it must be recognised that there is as yet no definite and conclusive evidence one way or the other, it seems more reasonable to adopt provisionally the position that the plant viruses are parasitic organisms, not necessarily bacterial or protozoal or like them, but with special characters imposed by the small size.

Dr. R. Alexander.

Dr. C. H. Andrewes.—*Extrinsic Origin of Viruses.*

Since it is hard to define a living thing it is well to leave aside for the present the question of whether viruses are alive or dead. It is more profitable to discuss whether they have an intrinsic origin from the affected cell or whether they reach it from outside. This question is discussed from three points of view with reference to the viruses of herpes simplex and the filterable fowl tumours.

(i) The epidemiological argument that these viruses must have an intrinsic origin because they appear to arise in no relation with other diseased individuals is shown to be fallacious; such a state of affairs is well known to occur in the field of bacteriology.

(ii) The viruses discussed are not confined to the limits of one species. Herpes is transmissible to rabbits and other animals; the virus of Fujinami's fowl sarcoma to ducks; the virus of Rous' No. 1 fowl sarcoma to pheasants. These viruses are antigenic and therefore presumably proteins. It is hard to believe that a human protein can be multiplied by rabbits or a fowl protein by ducks. Yet this is what would have to be believed if the viruses were of intrinsic origin.

(iii) Human beings with herpes and birds with fowl tumours develop antibodies to the infecting viruses. It is contrary to all our knowledge of immunology to believe that the body can make antibodies to products of its own abnormal metabolism, such as viruses must be if they originate within the body.

Mr. J. E. Barnard, F.R.S.—*Microscopic Methods and Appearances.*

Dr. S. P. Bedson.

In so short a time as ten minutes it would be impossible to discuss all the evidence bearing on the nature of filterable viruses, so that two points only have been selected for consideration:—

1. The fact that, though the size of virus particles varies from one virus to another, each species maintains a constant order of size of particle irrespective of the environment in which it multiplies.

2. That each species maintains its antigenic individuality irrespective of its environment.

Experiments with vaccinia, herpes and foot and mouth viruses have shown that the centrifuge enables one to gain a good idea of the size of virus particles, provided that the virus suspensions are free from tissue *debris* and the results so obtained are in keeping with estimates obtained from filtration and ultra-filtration experiments. When the above three viruses, grown in the skin of the guinea-pig's foot and suspended in saline, are centrifuged at 5,000 r.p.m. for 2 hours, the viruses of herpes and vaccinia are thrown down to a considerable extent, while the foot and mouth virus is not thrown down at all. The virus of psittacosis which is more readily spun out of a suspension than either herpes or vaccinia, should be represented by particles of a considerable size and almost certainly visible under the microscope. Examination of stained preparations shows that this is so. If, then, one is to assume that these filterable viruses are unorganised agents—some sort of self-reproducing disease catalyst—their apparent organisation in particles would have to be explained on the assumption that they are adsorbed on the particles resulting from the breakdown of cells. And the constant size with which each virus species reproduces itself would require the further assumption that each virus was selectively adsorbed by tissue particles of a constant size. If, however, filterable viruses are looked on as independent living agents, this phenomenon of constant size becomes readily understandable.

The maintenance of antigenic individuality irrespective of the tissue or animal species in which a virus is grown is a point of even greater importance. If a human strain of herpes is adapted to the guinea-pig and an anti-herpes serum prepared therewith, this serum will neutralise specifically not only guinea-pig strains of herpes virus but human and rabbit strains also. The same phenomenon can be demonstrated by the reaction of complement fixation, and applies equally to other viruses. Thus, an anti-vaccinial serum made with a guinea-pig strain will react specifically with vaccinia virus of guinea-pig, rabbit and human origin. Other examples might be cited. If filterable viruses are unorganised non-living things, then their multiplication in living tissues must result from the affected cells producing more of the agent which caused the initial change. But an agent of this sort would assuredly bear the antigenic

imprint of the animal species from which it arose. We have seen that this is not so, and to account for this one would have to assume that a filterable virus was something in the nature of a haptene, which, uniting with the cell degeneration products, gives them a new antigenic value—a virus specificity. This would imply an agent of simple chemical structure—non-protein and yet capable of multiplication. The only reasonable explanation of these facts is that filterable viruses are independent living things.

Dr. W. F. Bewley.—*Plant Viruses.*

Dr. G. H. Eagles.—*Artificial Cultivation.*

Dr. W. J. Elford.—*Filters and Filtration.*

Filtration experiments with several typical viruses and proteins employing a refined method of technique with a new series of graded collodion membranes have yielded very consistent and well-defined results. These have enabled the relative sizes of the infective agents to be determined under comparable conditions as well as estimated values for their sizes to be given. The following size relationships have been deduced :—

B. Prodigiosus	0·5–1·0μ
Bovine Pleuro-Pneumonia	0·1–0·5μ
Vaccinia Virus (testicular strain)	0·125–0·175μ
Infectious Ectromelia	0·1–0·15μ
Bacteriophage (Coli)	20–30$\mu\mu$
Foot and Mouth disease virus	8–12$\mu\mu$
Oxyhæmoglobin	3–5$\mu\mu$

Filtration evidence, therefore, indicates that the viruses cannot as a class be differentiated sharply from ordinary bacteria by any marked gulf existing between their relative sizes. On the contrary the viruses have sizes ranging from those of the smallest recognised bacteria down to within two or three times the size of serum proteins and oxyhæmoglobin.

Dr. W. E. Gye.—*Relation of Viruses to Malignant Tumours.*

Prof. J. C. G. Ledingham.

Tuesday, September 29.

Discussion on *Problems of Resuscitation, including Asphyxia, Electrocution, Drowning, &c.* (Sir Edward Sharpey-Schafer, F.R.S ; Prof. Yandell Henderson ; Dr. C. K. Drinker ; Sir Francis Shipway ; Sir Bernard Spilsbury ; Prof. J. A. Gunn ; Prof. J. S. Haldane, F.R.S.)

Sir E. Sharpey-Schafer, F.R.S.

Resuscitation in asphyxia. Circumstances producing asphyxia and requiring resuscitation :—

1. Mechanical causes. Obstruction of air passages, as by drowning, strangulation, hanging, foreign bodies.
2. Inhalation of air deprived of oxygen, such as carbon monoxide.
3. Paralysis of respiratory centre by electric shock.
4. Paralysis of respiratory centre by drugs, such as morphine, cocaine or volatile anæsthetics.

Physiology of respiration. Its dependence on (i) chemical stimulation of respiratory centre by CO_2, (ii) reflex stimulation of afferent nerves.

Excess of CO_2 causes paralysis of centre; small amount stimulates it. Afferent nerves for reflex stimulation of centre: pulmonary branches of vagi; superior laryngeal; nerves to carotid sinus; afferent nerves of respiratory muscles; cutaneous nerves.

Methods of artificial respiration. History of the subject. Importance of selecting a method independent of apparatus and simple of application. Measurement of efficiency. Description of prone pressure method. Cinematograph illustration. Advantages of the prone pressure method. It is (1) efficient, (2) simple, (3) involves no fatigue, (4) only one operator necessary, (5) no obstructions possible from tongue or soft palate or mucus. In supine position all of these tend to block the air passages and prevent air from entering the lungs; this especially in conditions of unconsciousness such as accompanies drowning, seen to a less extent during the unconsciousness of sleep, snoring being caused by obstruction of the tongue and soft palate to the passage of air into the lungs when a person is in the supine position. It immediately stops on his turning to one side.

Instances of fatal results in cases of drowning from turning patient from prone to supine position. Danger of calling in an uninstructed medical man. Artificial respiration not taught in most medical schools. Most medical students and doctors have never practised it. It could and should be included as part of physical drill in all schools. Already well taught in army, navy, police force, and to boy scouts and girl guides.

Prone pressure method rests on physiological basis; when the centre in the bulb is no longer completely paralysed by excess of CO_2 owing to renewal of air in the lungs from the movements of the chest it responds to each pressure with an inspiration; in other words, each expiration causes a respiratory movement. This is the well-known Hering-Breuer phenomenon. In this way the method assists recovery of natural respiration, and may be regarded as physiological, although the direct action of the operator is in the first instance to produce expiration.

Adrenaline should be given if possible, and as soon as possible, to aid recovery of heart and of arterial tone. This is the natural function of adrenaline, which is constantly being secreted into the blood under normal circumstances by reflex stimulation of a centre in the bulb. Like the other centres there (respiratory, cardiac, vasomotor) this centre is stimulated at the beginning of asphyxia, but is paralysed later by excess of CO_2, so that adrenaline is no longer secreted. To combat this loss it should be administered in asphyxia; best by injecting it directly into the heart, in a dose of about 20 minims of 1 in 1000 solution, which may be repeated if necessary.

Asphyxia neonatorum usually caused by obstruction of vessels in umbilical cord by compression of the contracted uterus if the cord is round the neck of the fœtus. It must be treated on a different principle because the lungs are empty of air and need to be filled. This is accomplished, if possible, by inducing natural respiration by stimulation of skin by cold or otherwise, but if the respiratory centre is paralysed by excess of CO_2 it will not respond. Failing this, inflation can be effected through a cannula passed into the trachea; mouth to mouth inflation is difficult, since air driven into the mouth is apt to pass into the œsophagus and stomach. If the lungs are inflated respiration can be carried on by intermittently blowing air in and allowing it to escape or alternatively by employment of pressure on the chest or abdomen, preferably in the prone position. In cases of *asphyxia neonatorum* adrenaline should always be injected at once into the heart.

Prof. Yandell Henderson.

It is not enough to resuscitate a man from drowning or asphyxia or other accident, or from surgical anæsthesia, if he is to die a few days later from secondary pneumonia.

Inhalation of carbon dioxide is the most effective means of stimulating respiration, when depressed or stopped, to renewed and increased activity. It has also proved to be a highly effective preventive—indeed it is essentially the specific preventive—of the collapse of the lungs which leads to pneumonia. These two advantages have now been demonstrated by experience in large numbers of cases of carbon monoxide asphyxia, drowning and other accidents. For the asphyxia of the new-born this inhalation is replacing the old brutal and often ineffective methods of swinging, spanking and dipping. In surgical clinics this treatment is already widely used to counteract failure of breathing under anæsthesia to restore a normal circulation, and especially to prevent post-operative pneumonia.

Respiration is closely associated with the tonus of the muscles, especially of the thorax. Depressed breathing and low tonus permit collapse of parts or all of a lung. From this collapse, if infection is present, pneumonia develops. If respiration and muscle tonus are stimulated by inhalation of carbon dioxide, the lungs are kept open and the development of pneumonia is prevented.

Dr. C. K. Drinker.

Since 1925 the author has had part charge of organising and conducting the emergency and educational work of the New York Consolidated Gas Company against carbon monoxide poisoning. The number of calls for treatment of asphyxia responded to by the emergency crews of the company averages 1,722 a year. Of this number 78 per cent. are due to carbon monoxide. The author outlines the training given all the employees of the gas company and the especial training of the emergency crews. The paper describes practical experience with the treatment of poisoning by inhalation of carbon dioxide and oxygen mixtures, the outcome of cases, and finally discusses the absence of sequelæ representing neurological damage.

Sir Francis E. Shipway.

Many deaths are preventable ; accidents arise from lack of knowledge of methods of resuscitation, from delay in their application.

Resuscitation may be defined briefly as the restoration of the nervous and muscular activities of the body. These activities depend largely upon an adequate supply of oxygen and the removal of excess of carbon dioxide. The exchange of gases may be interrupted in the following ways : (1) temporary paralysis of the nervous system owing to lack of oxygen or the action of anæsthetics ; (2) paralysis of the muscles of respiration, generally secondary to paralysis of the nervous centres ; (3) failure of the circulation of the blood.

Treatment is based upon the foregoing conditions.

Artificial respiration combined with oxygen and a small percentage of carbon dioxide will supply the needs of the nervous system and reduce the amount of the anæsthetic which is depressing the nervous centres ; further, the alternate expansion and contraction of the lungs produce a mechanical effect by pumping the blood, and a respiratory reflex, the Hering-Breuer phenomenon, if the medulla is excitable.

The pumping action of the heart can be improved by the supply of oxygen and of carbon dioxide, and also by the mechanical effect of the artificial respiration. In case of failure of the heart to beat, puncture of auricle to be employed without delay ; intracardiac injection of drugs unnecessary and may be harmful. Massage of heart may be attempted if abdomen is already open. Artificial respiration must be started simultaneously with cardiac resuscitation. Time of survival of cortical cells discussed.

In the case of a child apparently still-born there are the following points to remember : its nervous system and its heart are more resistant to lack of oxygen than is the case in the adult ; its lungs resemble a solid organ, and require inflating with air. Mouth to mouth inflation supplies sufficient oxygen and carbon dioxide, but its use is to be confined to emergencies as the air is more likely to inflate the stomach. Warmth increases the excitability of the nervous system and the metabolism. In more serious cases (white asphyxia) artificial respiration by the method of Schafer, administration of carbon dioxide and oxygen, cardiac puncture may be necessary. Respiratory centre of child may be depressed by drug given to mother to alleviate pains of labour.

In considering the methods of treatment the advantages and disadvantages of the Sylvester and Schafer methods of artificial respiration must be weighed in relation to the nature of operation. The value of carbon dioxide as a stimulant to the respiratory centre, prevention of necessity for resuscitation rather than cure, are considered.

Prof. J. A. Gunn.

The survival-time of tissues, especially of the central nervous system, after stoppage of the circulation.

Problems of resuscitation after arrest of the heart; massage of the heart—aim, methods and results; cardiac and respiratory stimulants; the duration of action, and methods of administration, of adrenaline.

The treatment of *asphyxia neonatorum*; a description of a modification of Buist's method of performing artificial respiration in the new-born child.

SECTION J.—PSYCHOLOGY.

Thursday, September 24.

(Section meeting in two divisions.)

Division 1.

Prof. C. W. Valentine.—*The Methods of Experiment and Observation in the Psychology of Early Childhood, with some Results.*

1. Simple experiments can be used more frequently than supposed in the course of everyday observation of infant development. Occasional observations as to sensory-motor development, reflexes, &c., are often unreliable.

2. Experiments may supply conditions never found in the course of everyday observation. Examples:—

(i) Experiments on the learning of names of geometric forms apprehended (*a*) by sight; (*b*) by touch, blindfolded.

(ii) Experiments on colour discrimination and colour preference.

3. Observation is essential on the other hand because some aspects of development (especially instinctive and emotional) can hardly be experimented upon. Thus, though experiments on fear have been devised, the richest material for the understanding of sympathy, self-assertion, anger, pugnacity, self-display, comes from careful everyday observation.

4. Examples of results of such observations, showing:—

(*a*) The distinction between anger and pugnacity at two or three years.

(*b*) The clear distinction between self-assertion and self-display.

(*c*) Sympathetic response to genuine pain cry at one or two years, but detection of pretended cry.

(*d*) Active sympathy at two years.

(*e*) Jealousy and affection in reference to other children and to parents.

Though generally favourable conditions may be arranged for such observations, an attempt at exact experiment is apt to lead to artificiality which defeats the purpose of the experiment. But for such observations impartiality and psychological training are essential.

5. Observation is also essential because some mental capacities at their earliest dawn seem to depend on the energy supplied by spontaneous momentary interest. Hence abilities appear at first sporadically and may fail to appear in a series of experimental tests. Examples: The beginnings of language, the use of numbers, the grasping of relations.

Prof. Beatrice Edgell.—*A Qualitative Study of the Memory Reports given at different intervals about the same Originals.*

The original objects to be remembered were coloured reproductions, postcard size, of illuminations from a British Museum MS. Each picture represented people engaged in some occupation proper to a given season of the year, *e.g.* hay-making, falconry.

These pictures were shown for thirty seconds each. The observer was asked to write out an 'immediate' memory report of the picture, giving all the detail he could recall. He was allowed to draw any sketch or plan that would aid his descriptive report and to name colours by reference to a graded selection of hues, tints and shades available for his inspection. The pictures were not shown again, but memory reports were asked for after an interval of one month and again after three months. Sixteen persons took part in the experiment, and eight of them wrote additional reports

twelve months after the original exhibition of the pictures. In no case was the request for such a late recall expected. The reports have been analysed with a view to studying the tendencies characteristic of the subjects taken collectively and also those characteristic of individual observers. Such analysis reveals: (*a*) The picture which is best recalled as an individual picture; the picture which is most often forgotten as an individual picture; the features most likely to be recalled; the features most likely to be forgotten; the parts of the pictures most liable to confusion; the displacements and interpolations characteristic of later recalls; the progress of forgetting; the influence of earlier on later recalls; (*b*) the individual differences in the mode of recall and in the facts and items remembered; the progress of forgetting.

Dr. Mary Collins.—*Some Observations on Immediate Colour-memory.*

The ability to reproduce a colour fifteen seconds after seeing it was found to vary considerably with the wave-length chosen. The blue (4609) and yellow (5880) selected were reproduced fairly easily by all subjects, and could be reproduced with a strong feeling of certainty after a fair number of trials had been given. A red of wave-length 6700 offered greater difficulty, and even after a lengthy number of observations was not reproduced with too great certainty, although the amount of error undoubtedly decreased. The most difficult wave-length to reproduce, however, was that of thallium green (5350). After over 100 observations there was still great uncertainty felt by each subject in reproducing it, and little learning showed itself.

The thresholds of the subjects for these colours were also determined. While these seemed to have some bearing on the results obtained with the yellow, blue and red used, they showed little correlation with the green, for the thresholds in the green were decidedly lower than the reproductions would lead one to expect.

Dr. Ll. Wynn Jones.—*A Study of Speed in Associative Reproduction.*

The study of reaction time with respect to the mental factors which condition the reaction has not received adequate attention. Three typical reactions may be cited:—

1. Those of an eductive nature which serve as tests of ' g.' Here it is possible to arrange conditions so that speed becomes a measure of ' g ' or else to alter them so that speed is irrelevant.

2. ' Free ' association tests. The Binet speed of sixty words in three minutes may be a better measure of ' fluency ' than of ' g.'

3. Reactions which may serve as tests of ' retentivity ' in one of its forms. When a pupil begins a modern language, *e.g.* French, his speed of giving the English equivalents of a series of French words would be expected to correlate positively with ' g.' After years of study, however, the speed would not be expected to remain a function of ' g ' but rather of ' retentivity,' if the commonest French words were chosen. Well-known French words were therefore selected which, according to the results of Henmon, would be known by more than 90 per cent. of pupils who had studied French for only one year.

Pupils from forms 3a and 5a in a secondary school were tested by means of a self-recording chronoscope. Similarly the reaction times for ' free ' association were obtained, using a series of English words. There were also available for each pupil the class mark in French, the Easter examination mark in French, his mark in the Cattell test of intelligence, and his position with all school subjects pooled. The speed of translating well-known French words into English showed appreciable correlation with the class marks in French and also with the Easter examination marks in French.

There was no evidence of correlation between the Cattell test and the speed test, or the class marks in French, or the Easter examination marks in French.

The ensuing tetrad differences would be accounted for by assuming that the class mark in French, the examination mark in French and the speed of translating well-known French words into English are influenced by a group factor. It has already been ascertained that ' retentivity ' cannot be regarded as a factor of wide functional unity, but if the results of the present study are confirmed, it appears possible for many pupils who are suitably endowed to distinguish themselves in modern languages at the secondary school with only a modest amount of ' g.'

Division 2.

Dr. W. Stephenson.—*The Incidence of Spearman Factors in Psychiatrical Material.*

In a mental hospital psychologists and psychiatrists co-operated in a survey of psychiatrical material—patients with delusions, dementia præcox, melancholia, manic-depressive psychosis, &c.—with, on the psychological side, the Spearman Factors as the experimental approach to determination of the objective mental conditions of the patients.

The factors measured were g-factor (' general intelligence '), p-factor (general mental inertia, ' perseveration,' the continuance of the effect of past experience), o-factor (fluctuations of attention), memory, and w-factor (' purposiveness,' a ' will '-factor). The purpose of this paper is to afford illustration of Spearman mental tests that could be used profitably in mental clinics, particularly for research purposes, to show the functioning of certain of the tests, and to give a short summary of the contact made by the tests with psychiatrical classification, and with dementia.

New non-verbal g-tests were employed ; their value was shown in cases of senile psychoses. Interesting general results were reached in measurements of the o-factor, where, even with the simple 5-minute o-tests used, high oscillation was shown to be highly associated with impulsive, restless behaviour in general.

Our particular interest is in the p-factor. The p-tests showed high correlations and showed contact with psychotic conditions, especially in (*a*) the manic, manic-depressive and melancholic reaction types, and (*b*) the præcox psychosis. There were indications of a second p-factor, especially in the præcox psychosis, associated with perseveration (continued action) of excitement.

In routine clinical work the measurement of the various factors appears to be possible ; the p-factor especially seems to be of very great importance, and is as readily observable as the g-factor (intelligence).

Dr. Evelyn Lawrence.—*The Social Distribution of Intelligence.*

The study to be described provides data on variations in intelligence test results with the social class of the subjects tested, with special reference to the parts played by heredity and environment.

The average I.Q. of children in certain Poor Law and other institutions is compared with that of elementary school, and preparatory school, children. Some figures for adolescents and adults are furnished by the scores obtained by public school boys and girls, training college students, and Workers' Educational Association students in the National Institution of Industrial Psychology's Group Test 33.

The large differences among these groups, however, throw little light on the problem of inherited class differences, as the effects of environment and training cannot be weighed. A possible means of examining the question is provided by the study of children removed from their homes into institutions in earliest infancy, or when progressive changes of intelligence can be looked for where environment has changed in some definite way. Groups of such children are here examined. Stanford-Binet tests and Simplex group tests were given to 384 children of ages from 5 to 15, who left their mothers at an average age of six months, and who had never come in contact with their fathers. The intelligence test results of these children have been correlated with the social class of the parents.

In another group of institution children it has been possible to compare the correlation between intelligence and parents' class for children who left home very young, and those who left later in life. Increase in intelligence was also looked for among children who were taken from very bad homes into improved surroundings. A further comparison useful for the estimates of the effect of environment is that between the variability of groups of children brought up in very homogeneous conditions and that of those brought up in their own diverse homes. The groups described furnish such a comparison.

It has been argued that most of the studies of social differentiation of intelligence have been based on Binet and other tests mainly verbal in their nature, and that smaller differences might be found if more practical manifestations of intelligence were examined. Correlations between social class and performance tests results among groups of London elementary school children are here available for examination.

It has also been possible to relate intelligence to legitimacy and to such measurements of general health as the height-weight index, and a doctor's estimate of general physical fitness.

The general conclusion suggested by this work is that, while a slight inherent class-difference in intelligence seems to have been demonstrated, at least in one group, the statement needs most careful qualifications and limitations.

Prof. F. C. Bartlett.—*Psychological Problems in the Government of Native Races.*

Dr. Shepherd Dawson.—*Intelligence and Fertility.*

The application of individual mental tests to a large number of children of approximately the same social status, and an inquiry into the size of the family to which each belonged, showed a small but significant negative correlation between the intelligence of the child and the size of the family. The birth-rate was higher in the families represented by the dull children, and, although the fatalities were also higher, the number of survivors was greater. The difference would probably be greater if a representative sample of the whole population were examined. Although intelligence is affected by pathological conditions, yet, if it be an inherited character, as is commonly believed, this suggests a possible slow but serious dilution of intellect in the population as a whole and the need for a study of the inheritance of mental aptitudes.

Friday, September 25.

Division 1.

Dr. R. H. Thouless.—*Individual Differences in Phenomenal Regression.*

The amount of 'phenomenal regression' of different subjects, *i.e.* the degree of their tendencies to see the 'real' characters of objects rather than the characters given by peripheral stimulation, was measured by three tests. These were: (A) a test of 'tendency to constancy of shape'; (B) a test of 'tendency to constancy of size'; and (C) a test of 'tendency to constancy of brightness.' The principal objects of the experiment were, first, to discover how universal was the effect and how large were the quantitative differences between different individuals; and, secondly, to find out the nature of the quantitative relationship between the different tests.

Of 129 subjects (including ten artists and teachers of art) none were found who did not show phenomenal regression in tests A and B, although a few showed zero or negative results in test C. The median tendency was large with a large scatter, showing that, in general, perception is largely determined by 'real' characters, but that the extent to which it is differs very much from individual to individual.

There was a large correlation (about ·7) between the results of test A and test B for groups homogeneous in age and sex. This remained considerable after the effect of partial correlation with intelligence had been eliminated. This shows that there is a group factor determining the tendency to phenomenal regression, the strength of which in an individual determines how far he sees things in their 'real' shape when he is looking at them at different angles, and how far he sees them in their 'real' sizes when he is looking at them from different distances.

Evidence as to whether the tendency to constancy of brightness was dependent on the same factor was inconclusive. The correlation of test C with the other tests was low and of doubtful significance. Quantitative treatment of this test was complicated by the discovery that two variables contributed to the result; the tendency to see the more reflective paper as the brighter even when reflecting less light, and the tendency to a difference between the phenomena of 'whiteness' and 'brightness.' This difference was but absent in about one in five subjects. Phenomenal regression for 'whiteness' showed fairly high correlation with tests A and B.

Correlation of phenomenal regression with intelligence was found to be small and negative, *i.e.* the more intelligent subject tends slightly to perceive more nearly to the stimulus character of the object. The relationship with age was in the opposite direction, the older subjects tending to see more nearly the 'real' character of the object. There was a significant difference between men and women students, the latter having the greater tendency to phenomenal regression. A small group of

artists showed significantly less phenomenal regression than a control group of the same sex and average age. An attempt was made to discover whether there was an association between amount of phenomenal regression and temperamental type, but with the numbers available no significant difference could be established.

Dr. F. Aveling.—*The Influence of Volition upon Thinking.*

The present communication embodies the results of a series of experiments, continuing work previously carried out in the laboratory of King's College, which were intended to investigate the nature and amount of influence that will-acts have upon processes of thought; in particular, upon the three fundamental processes of original knowing, *i.e.* the cognising of experience, the awareness of relations between items of experience, and the production of ideal correlates.

The last-named problem was attacked by the method of Free and Relationally Predetermined Reactions (so-called 'Controlled *Associations*'). Results show that the relation determined by the acceptance of the instruction (*i.e. resolution* to react in a particular way) works, even though it may no longer be clearly in consciousness, but that consciousness of it tends to reappear in certain subjective conditions; that the different relations investigated function in a similar way through notional awareness; and that abstract concepts tend to arise as mediating between the more particular meanings of stimulus and reaction words. Temporal values of the several kinds of reaction are considered, together with the order of difficulty of the relevant processes. The conclusion is drawn that volition affects the process of correlate eduction by way of a conative attitude which may be wholly subconscious. Such conative attitudes may be described as volitional dispositions or tendencies, and may be compared with connate instinctive dispositions.

Original cognising of experience was investigated by means of the tachistoscopic exposition of monochrome letters and monochrome and coloured symbols, under the influence of two different instructions: (1) 'Observe what you can and record all that you can see'; (2) 'Look for a given letter (or symbol) if it should be shown, and record all that you can see.' The results in both cases were marked in such a way that the gain, if any, for the required items could be shown, together with any loss in the amount of items seen as a whole. Working with the first instruction the percentage marks could be taken as an indication of the expectation that any given letter or symbol would be apprehended; working with the second the actual percentage for letter or symbol was directly calculable.

A similar procedure was followed in respect of relations of similarity of shape and of colour obtaining between the items exhibited.

Results indicate a very considerable gain for the letter, symbol or relation which subjects had determined to apprehend if it were there; and a loss (though variable) in the number of items or relations seen when the experiment was carried out with the second instruction.

The indication is that the will-act to apprehend a given item or relation sets up a conative tendency to observe it if it is actually presented; while, at the same time, in general there is inhibition of other items and relations. (*Caveat.* It is possible and, according to introspection probable, that items and relations are in fact apprehended, but fail to be retained between the time of exposition and the act of recording.) The gains over the calculated expectation of any given letter, symbol or relation being apprehended range from 69 per cent. to nil (or even, in one or two instances, to minus quantities). The inference is that, while individual differences come into play, the antecedent will-act very considerably influences the subsequent process of perception of items and relations by setting up a conative disposition to apprehend them if they should be in the presented field, but that the inhibition bears no ascertained relation to the disposition. These volitionally initiated conative dispositions or tendencies may, again, be compared with instinctive dispositions, in virtue of which conscious organisms 'pay attention to objects of a certain class or kind'; in regard to which they have also a tendency to behave in a more or less definite manner.

There is a further observation which makes the analogy between instinct and will even more complete. The experiments show that one may be aware of a definite relation in the absence of awareness—certainly in the absence of clear awareness—of one (or even of both) of its fundaments. One need not suppose that instinctive behaviour requires clear cognitive guidance.

It is with respect to ends (natively or volitionally determined goals) and means to ends that volitional and conative processes are directed by cognition; but items and relations are only cognised as ends and means to ends in so far as they have teleological significance. And that significance may be largely subconscious, as is particularly shown in the results of the first and last experiment treated.

Dr. William Brown.—*The Mathematical and Experimental Evidence for the Existence of a General Factor* (g).

If a number of sufficiently dissimilar mental tests be applied to a group of individuals and correlation coefficients calculated, it is found that these correlation coefficients are related to one another in such a way that for any four (or *tetrad*) of them the following relation holds good, within the limits of random sampling, viz. :—

$$r_{13}r_{24} - r_{14}r_{23} = 0.$$

We owe both the discovery of fact and the devising of the criterion to Prof. C. Spearman. The inference drawn from this is that the abilities measured by the mental tests are divisible into two factors each, the one being common to all (the general factor, g), while the other is in each case specific and independent (s).

The tetrad criterion is free from the effects of 'attenuation' upon coefficients, and therefore escapes criticism of mine to which previous criteria were susceptible. But its applicability involves certain mathematical and statistical presuppositions to which the present paper addresses itself.

The argument is illustrated by detailed statistical analysis of three groups of correlation coefficients, including one of thirty-six coefficients (giving 378 tetrads) resulting from nine tests of mathematical ability applied to eighty-three public school boys.

A mathematically adequate proof (or disproof) of Spearman's theory would involve the application of at least twenty suitable non-overlapping mental tests to a group of at least 300 suitable individuals (*i.e.* forming an adequately homogeneous 'random sample'). As Prof. Karl Pearson has pointed out, 'short series involve such large probable errors that a mere statement that theory and observation are in accordance within the limits of the probable errors can carry no conviction with it' (*Biometrika*, vol. xix, 1927, p. 261).

I am organising an extensive research on these lines at Oxford, with a psychopathological counterpart at Bethlem Royal Hospital, which should ultimately give definitive results. The 14,535 tetrads will give a reliable frequency-distribution from which all the necessary statistical constants can be calculated with adequately small probable errors. The result should also settle the exact mathematical relationship between g and s, whether additive as at present assumed, or obeying some more complicated formula.

Mr. A. W. Wolters.—*Some Considerations with regard to Conceptual Thinking.*

The psychological investigation of thought processes would have fared better had the traditional terminology of logic been avoided. It seems useful now to stress the conative aspect of thinking, and, while accepting the current views as to the character of the observable contents of the process, to concentrate more upon the nature of the process as such. It is suggested that if thought be thus taken as a form of mental behaviour, instructive analogies can be found in so-called practical behaviour; it may even be held that the distinction between 'practical' and 'intellectual' behaviour is relatively unimportant to the psychologist. If this can be established the concept may be regarded as a method of thinking, rather than 'a thought.' As in the practical sphere we find generalised reaction tendencies which become specialised to meet particular situations, so there exists a schematic readiness to deal with thought situations of a given type. Whether this readiness shows itself as an observable conscious content depends upon the circumstances of the moment. If this view of conceptual thinking be accepted, the question may be raised whether there is any fundamental distinction between volitional and intellectual decisions. And is animal behaviour conceptual? Probably it would be better to banish the terms concept and conceptual from psychology.

Division 2.

Dr. E. O. Lewis.—*The Social Aspects of Mental Deficiency.*

The significance of the social problem which mental deficiency presents cannot be appreciated unless we have a clear conception of the nature of this abnormal condition. Whether we approach the subject from a biological, ætiological, clinical or social standpoint, mental deficiency comprises a complexity of conditions. Its composite character is recognised in some of the generally accepted classifications of defectives such as those into primary and secondary cases of amentia, familial and non-familial, inherited and sporadic cases. Each of these classifications has its special merits, but in a discussion of the social aspects of the problem the simpler classification into the pathological and the sub-cultural types seems more helpful. The pathological cases are those in which there is a definite organic lesion or abnormality—cerebral hæmorrhage, hydrocephalus, euchorine deficiency, or some physical defect due to toxic factors that have impeded growth during childhood or at the fœtal stage, or may have impaired the germ-plasm of one or both parents. The sub-cultural type of defective is merely a variant of the normal. Intelligence is distributed in accordance with the normal curve of distribution and the sub-cultural group of defectives are represented by the lowest part of this curve. The pathological deficiency is a definitely abnormal condition, whereas sub-cultural type is merely a normal variation.

In a discussion of the social aspects of mental deficiency the sub-cultural group is all important, whereas the pathological group has comparatively little significance. The pathological group includes considerably less than half of the mentally defective; its members are mostly low-grade and need little more than custodial care and some simple form of training, and they are fairly evenly distributed amongst the various social grades of the community. The sub-cultural defectives, on the other hand, are much more numerous—not infrequently several are found in the same family. A large proportion of these families form an important group in the lowest social strata of the population, and give rise to many of the chronic social problems of our nation.

The higher the species in the biological scale the greater is the variety of form, function and behaviour amongst its individual members. Mental deficiency of the sub-cultural type may be regarded as one manifestation of this variety. This variation in the endowment of innate mental characteristics seems to increase, or at least to become more conspicuous as civilisation progresses. The complexity of industrial and social conditions prevailing in a civilised community, especially if the economic system has a competitive basis, makes manifest the wide variations in mental and physical capacities of the individuals comprising the community. Viewed from this standpoint the problem of the sub-cultural group of the mentally defective can be said to be an inevitable concomitant of a progressive civilisation.

These variations amongst the population, especially if of sub-normal character, add considerably to the difficulty of maintaining the cohesiveness of the social group. Certain economic and social factors tend to produce a social system in which the sub-normal members are segregated in certain districts; and the economic and social failure of large numbers of the men and women who belong to this group results in their becoming a financial burden upon their families or upon public funds. The assimilation of this sub-cultural group and of the still larger contiguous group—the sub-normal tenth—into the general life of the community, constitutes one of the most difficult problems that western civilisation has to solve. The process of finding a solution will probably evolve new conceptions of the social hierarchy and of the mutual obligations of the various groups in the general community.

Prof. F. A. E. Crew.—*The Genetic Background of Mental Defect.*

The geneticist is not responsible for the description of characters, the mode of inheritance of which he studies. He accepts the descriptions and definitions of others and concerns himself solely with the interpretation of the facts relating to the distribution of characters among successive generations. In the case of the human subject the geneticist does not even collect his own data; his materials are the records of the clinician. The pedigrees he examines have all too commonly been constructed by such as are unskilled in psychology or else in genetics, and who accept anecdote in place of evidence. It is not surprising, therefore, that amongst pedigrees examined mental defect in different cases demands, in explanation of its distribution, the action

of a single dominant, a single recessive, or multiple recessive hereditary factors. There is such variety in diagnosis and in interpretation that it is impossible to know whether or not mental defect, as defined, can be the expression of different genetic forces. In the absence of accuracy in diagnosis there can be no accuracy in the interpretation of the genetic basis of the character. But that there is a genetic background of mental defect is certain.

Mental defect, whatever may be its exact genetic nature, is a character in the genetic sense. In any programme of control through breeding, therefore, genetic fact and theory are to be considered. It can be eradicated by breeding only by denying parentage to all those who carry the hereditary ingredients of the character, but the success of any attempt to control the spread of these among a population will be determined largely by the nature and the number of the genetic factors involved.

Dr. R. G. Gordon and Dr. R. M. Norman.—*Some Psychological Experiments on Different Types of Mental Defectives.*

Certain experiments were carried out to discover the distribution and nature of various functions in the mental defective. Two series were used, 100 adult girls with an I.Q. of over 40 per cent., and 100 boys of the idiot and imbecile class.

(1) Head's Aphasic tests, which were devised to test the reactions of persons with damage to the cerebral cortex, were applied to discover the reactions of those possessed of imperfectly developed cortex. The tests resolve themselves into two groups, those concerned with verbal and language manipulation and those concerned with spatial discrimination and manipulation. The former group proved very much easier for the defective than the latter.

(2) Rorschach's blot tests were applied to the same two series to test the nature of visual imagery, observation of colour and tendency to descriptiveness. Imagery could be divided into rich, fair and poor, but in all cases was restricted in scope and confined to concrete objects within the limited experience of the defective. Colour was noticed by a minority of the subjects, and descriptiveness was characteristic of the responses of the higher grade rather than of the lower grade subjects.

(3) Thorndike's puzzle box test was applied to the same two series to discover the possibility of grasping a spatial and mechanical problem as a whole, of learning by experience and of applying the solution of simpler puzzles to help in the solution of more complex problems. The last two capacities were markedly present in both groups, so much so in the higher grade subjects as to make the serial tests valueless. These were presented with the most complex puzzle only, and showed variable capacity in grasping it and solving it. The lower grade subjects were presented with the series and showed a varying ability to solve the more complex puzzles in the light of the simpler ones.

(4) Yerkes' discrimination tests were modified to test the power of retaining the image of a spatial configuration. For various reasons this was found to be useless as a test for the higher grade cases. In the lower grade cases interesting results were obtained, but only indicate so far how this method might be usefully applied in a more elaborate research. All these experiments show a general but by no means exact correlation with intelligence as measured by the Binet and Porteus tests.

Dr. F. C. Shrubsall.—*Classification of the Mentally Defective and the Relative Frequency of Different Types.*

It is of interest to compare the frequency of types found in the course of a statutory ascertainment with those found by Dr. E. O. Lewis in his special investigation. Statutorily, Mental Deficiency Committees have only a duty to ascertain those who in addition to being defective are also subject to be dealt with, which means roughly that either the parents have applied for help or the persons are neglected, criminal or inebriate. Local education authorities have, however, a duty to ascertain all defective children in their area between the ages of 7 and 16, so that their findings should give a cross-section of the community. Whereas the Mental Deficiency Act requires the subjects to be grouped in four classes—idiots, imbeciles, feeble-minded persons and morally defective persons, the latter being an almost negligible number—the Education Act only requires a classification into those who are educable in special schools and those who are not, the latter being the idiots and imbeciles.

The London published data show that from 1902 to 1930, out of the total number of children examined 40,208 were deemed to be mentally defective, of whom 3,953, or roughly 9·8 per cent., were regarded straight away as ineducable. Certain others are deemed ineducable after a trial in a special school. Since 1914 data have been collected to show the relative numbers of each grade, and the following table shows the corrected figures and percentage for comparison with Dr. Lewis's figures for the urban areas he investigated.

Class.	Number.	Percentage.	Dr. Lewis's Percentage.
Mentally defective educable . .	19,713	85·05	80·3
Imbecile	3,193	13·78	16·0
Idiot	272	1·17	3·6

As a number of the lower grade London children were dealt with by the Metropolitan Asylums Board and never came to the purview of the Education Authority, the lower rates for low grade cases are not unexpected.

The percentage distribution of these children among the different clinical types of defect was :—

Type of Defect.	Percentage Frequency.	
	Educable Defectives.	Ineducable Defectives.
Simple Primary Aments	90·77	44·83
Mongols	0·97	11·54
Cretins	0·39	2·46
Microcephals	0·52	3·25
Hydrocephals.	0·33	2·03
Epileptics	2·12	17·00
Plegics	0·27	8·66
Plegics + Epilepsy	—	6·38
Miscellaneous secondary Aments . . .	4·63	3·81

The relation between the mental and chronological age in a sample of the educable mentally defective children was :—

Chronological Age.	Mental Age. 3	4	5	6	7	8	9	10	Total.	Mean Mental Age.
7 . .	159	789	1,216	521	44	1	—	—	2,730	4·8
8 . .	31	285	1,182	2,001	586	29	—	—	4,114	5·7
9 . .	7	42	285	1,068	1,227	152	1	—	2,782	6·4
10 . .	6	15	56	223	628	334	20	—	1,282	6·9
11 . .	3	1	22	94	273	309	37	7	746	7·3
12 . .	4	—	9	40	161	228	83	23	548	7·7
13 . .	—	1	5	18	72	110	84	31	321	8·0
14 . .	—	1	3	17	62	144	147	61	435	8·1
Total . .	210	1,134	2,778	3,982	3,053	1,307	372	122	12,958	

This shows that mental age was only one of the factors in deciding on suitability for a special school.

The intelligence quotients of the ineducable showed no relation to the type of defect.

A random sample of ten thousand cases of all ages dealt with under the Mental Deficiency Act gave the following distribution, which shows that there is a greater tendency or need to deal with lower grade persons who naturally are more often those with secondary types of amentia.

Type.	Class. Idiot.	Imbecile.	Feeble-minded.	Total.
Simple Primary Aments	135	1,910	5,079	7,124
Mongols	37	427	10	474
Cretins	4	86	26	116
Hydrocephals	15	58	22	95
Microcephals	29	69	21	119
Plegics	96	299	151	546
Plegics + Epilepsy	71	113	39	223
Epileptics	121	463	348	932
Miscellaneous secondary Aments	20	144	207	371
Total	528	3,569	5,903	10,000

The proportions of the grades idiot, imbecile, feeble-minded are 5, 36, 59, as compared with the 5, 20, 75 found by Dr. Lewis, and correspond more closely to the distribution in an institution.

Monday, September 28.

(Section meeting as a whole.)

Presidential Address by Dr. C. S. Myers, C.B.E., F.R.S., on *The Nature of Mind* (see page 181).

Mr. Denys W. Harding.—*Rhythmization in a Motor Task.*

Records were made of the time-intervals that occurred between successive letter-strokes when experienced typists practised unfamiliar English words until they could type them at their normal speed. It was hoped to discover in this way whether rhythmization played any part in the acquisition of certain motor skills. Rhythm is taken to be a form of activity at non-conscious levels, which, if it becomes accessible to introspection, appears as an immediate unification of a series of impressions, each of which is differentiated by reason of its position within the rhythm unit.

Twenty subjects were observed. The records show that the acquisition of skill is for many subjects accompanied by the development of a clearly-marked time pattern amongst the letter-strokes; other subjects type with more nearly uniform time intervals between successive strokes, though none achieves complete uniformity. In the one group the acquisition of speed involves the establishment of and adherence to a definite rhythm; in the other speed is acquired without the development of any fixed time pattern amongst the strokes. It would seem that the latter subjects merely *link* successive movements, each to the next, whereas the former subjects *group* successive movements into a muscular unit possessing peculiar internal time relations that are an integral part of its unity.

The particular rhythm adopted for a given word is based, not on convenience of pronunciation or of visual apprehension, but on the arrangement of the letters on the keyboard, and on the resultant differences in the speed at which various combinations of letters can be typed. Though the rhythm is suggested by these slight differences in the convenience of letter combinations, it is not absolutely enforced, and some subjects fail to adopt it. Moreover, even subjects who finally adhere to one rhythm may not achieve it until some time after practice is begun. A study of the records in graphical form suggests that small groups of letters form the nuclei of the final rhythm, and that other letters only gradually become attached to these as the total rhythm is evolved.

Rhythmization can be shown to occur not only in the rather unusual task of typing one word repeatedly, but also in the typing of normal subject-matter. It is therefore of practical significance in the training of typists. The present tendency to insist

upon uniformity of time interval between successive strokes in typing can be seen to be mistaken. Moreover, the time needed to acquire speed in typing the most frequently used words and phrases may probably be reduced by indicating to the learner the most suitable rhythm to adopt.

The same principle may well be extended to the teaching of many complex motor tasks. The best technique for applying rhythm in this way has still to be discovered, but it seems highly probable that for many beginners some indication of the rhythm to be adopted should form an important part of the preliminary instructions.

Mr. C. A. Mace.—*Influence of Indirect Incentives upon the Accuracy of Skilled Movements.*

There are grounds for supposing that rewards and penalties are not always practicable, and are sometimes detrimental incentives, particularly in the case of operations requiring delicate co-ordination. It is therefore of importance to examine the mode of operation of conditions which do not arouse complicating secondary impulses (such as the desire for reward) but affect favourably the primary intention relevant to the task in hand. (Such conditions will be called indirect incentives.) Among such conditions are those that produce an unwitting modification of the workers' standard of work, a standard which, though only implicit and inarticulate, is almost invariably present as a factor upon which his efficiency depends.

Experimental studies of two forms of aiming operations show that by modifications of the target the marksman's efficiency may be enhanced from 12 to 17 per cent. Various considerations entitle us to interpret this as due to a change in the subjective standard of efficiency imposed by the variation in the target. The principle would seem to admit of a wide range of educational and industrial application.

Mr. S. Wyatt.—*Some Personal Factors in Industrial Efficiency.*

Mr. Eric Farmer.—*The Present Tendencies in Vocational Selection.*

Vocational selection has passed its initial stage, in which it had to be proved that it was possible, by means of psychological tests, to measure certain functions involved in industrial proficiency. At present attention is mainly directed to critically examining its technique and experimenting with different types of tests. There is a tendency to give up tests involving psychological functions as far as possible in isolation for more complex tests involving several integrated functions.

This is shown by the increased use of sample and analogous tests, and also general analytic tests involving complex rather than simple functions. It cannot be expected that individual vocational tests will ever yield high correlations such as are found between intelligence tests and scholastic performance, because industrial proficiency is determined by many variables, none of which plays such a predominant part in it as intelligence does in scholastic performance. Psychological tests will none the less be valuable in so far as they select more efficient workers than any alternative methods of selection, and, judged by this criterion, they have already justified themselves, although much more research is necessary before they can be put to the fullest use.

Afternoon.

Dr. A. Macrae.—*Guidance in the Choice of an Occupation.* (Public Lecture at the *London School of Economics, Houghton Street, Aldwych.*)

Tuesday, September 29.

Prof. Frank Allen.—*The Function of Induction in Colour Vision.*

When one eye has been stimulated by any spectral hue above a certain critical intensity it has been found experimentally that, in addition to the sensation of colour, certain uniocular and binocular effects follow which are invariably confined to the reception of the three colours, red, green and violet. If the stimulating colour is red, green or violet, the corresponding receptor mechanism in the stimulated area of the retina is depressed in sensitivity, while the receptors for the remaining two colours

are enhanced. If the stimulating colour is orange, yellow or blue, two groups of receptors in each case, either those for red and green, or those for green and violet are depressed in sensitivity, while those for the remaining sensation are enhanced. In the regions adjoining the stimulated area, and in the retina of the unstimulated eye, all three receptor mechanisms are enhanced in sensitivity, though the enhancement is predominant in the sensations complementary to the stimulating colour. If the stimulating colour is of very low intensity, the effect everywhere is depression of sensitivity.

These experimental results strongly support the trichromatic theory of Young. They also show that the physiological processes of inhibition (depression) and facilitation (enhancement) are invariably associated with all visual excitations. The supposed antagonistic colour processes assumed by Hering as the basis of his theory may be identified with these physiological actions, and the two chief theories of colour vision may thus in a measure be reconciled.

The mutual inductive retinal actions, in the opinion of the writer of the paper, are to be referred to reflex actions of a sensory character by which the sensitivity of the retinal receptors is controlled.

On the basis of these actions we may explain the origin of simultaneous colour and luminosity contrast, both uniocular and binocular types. Since by the mutual inductive action of two retinal areas upon each other the receptor apparatus in each is enhanced in sensitivity but predominantly for the complementaries of the two stimulating colours, each colour perception will appear to be modified as if it were mixed with the complementary of the other.

The sensory nervous system always tends to remain in or to resume equilibrium. After stimulation the recovery of equilibrium is made by a series of oscillations of inhibition and facilitation, or of depression and enhancement. Such actions are doubtless concerned with negative after-images. Since in each half of the neural apparatus the nervous processes may oscillate in opposite phases, as well as in the same phase, we have here the possible explanation of binocular rivalry and fusion of colours.

With an excessive and permanent depression of sensitivity of the receptors for one or more of the fundamental colour sensations there occur the phenomena of 'colour blindness'; while when the enhancing processes are excessive and permanent the opposite phenomena of anomalous trichromatism are exhibited by the eye.

In the mutual inductive action of stimulated retinal areas we have the occurrence of reciprocal innervation, the character of which has been so elaborately investigated by Sir Charles Sherrington. Simultaneous contrast is reciprocal innervation operating in vision.

The conclusions suggested by the experiments in colour vision are supported by similar experiments in the senses of touch, taste and audition, as well as by experiments on the post-contraction of muscles and the secretion of glands.

Prof. J. Drever.—*The Nature of Emotion.*

Emotion, as ordinarily understood, has two aspects. On the one hand, it is a response of the organism; on the other hand, it is a modification of experience. Failure to recognise that these are two distinct aspects is a fruitful source of difficulty and controversy in modern psychology. In particular, several writers describing emotion as an experience carry over this description to the consideration of the relation of emotion to instinct with very unfortunate results. These unfortunate results could have been avoided had they started with emotion as response, and worked out a systematic account of emotion as experience on this basis. So far the starting-point of the behaviourist would appear to be justified. When emotion as experience is approached in this way it is impossible to regard it as an elementary mode of consciousness, or even to regard it as a complex affective state and nothing more.

The essential points which the psychology of emotion must keep in view are these:—

1. That emotion is fundamentally a phase of the response of the organism to a situation.

2. That emotion as an experience exhibits that polarity which is characteristic of affective experience, ranging between the two opposite poles of joy and sorrow.

3. That, nevertheless, instinct feeling or interest, as a simple elementary component of experience, cannot be identified with emotion as a complex cognitive-affective-conative 'psychosis.'

Prof. T. H. Pear.—*The Voice as an Expression of Personality.*

Personality will be defined as the effect upon others of a living being's appearance, sound, behaviour, &c., so far as they are interpreted as distinctive signs of that individual. Personality, therefore, can be expressed by physique, colouring, odours, clothes, behaviour, gestures, manners, voice and speech. Their possessor may be clearly aware, dimly aware, or unaware of any of these effects, of their causes, or of the means by which they are produced.

Character will be defined as the comparatively stable structure of a person's mind, wrought by abilities (habits, techniques, skills), sentiments, and by their integration into a relative unity. A personality-trait may produce an instantaneous effect; the judgment of character cannot be immediate.

If these definitions be accepted (*a*) voice is obviously an important mark of personality, (*b*) the study of its significance is valuable for individual and social psychology, (*c*) the problem of the connection (if any) of the voice with character must await more knowledge of the rôle of the voice in personality.

Though the voice can be considered and judged apart from speech, in practice this is seldom done. Yet in the last few years broadcasting, and to a lesser extent the increased use of the telephone and developments in the sound-film, have thrown into sharp focus the importance of personal characteristics in the voice. Since speech may now affect millions simultaneously, it may be considered as a high-grade skill of rapidly increasing importance; a skill which may have to be quickly and considerably modified to meet the new requirements of the microphone. Speech, suddenly appearing as a serious rival to the printed word, raises unexpected problems in social psychology.

From the psychologist's point of view the interest of broadcasting is not only that it facilitates vocal communication to many thousands, but that at present this is done most effectively by an intimate form of speech, evolved for the purpose of communicating to one person or to a small group. Development of the art-forms of speech suitable for the microphone will require, however, an improvement in criticism of speech-forms. The level of such criticism is at present low. It is conceivable that gifts facilitating certain kinds of literary criticism may incapacitate their possessors for sensitive discriminative listening to speech.

The development of speech in this country has produced certain vocal prototypes and stereotypes. How far these will dominate the future development of speech is an interesting question. Possibly the stimulus to experiment provided by broadcasting and the sound film may produce new deviations from accepted voice patterns.

Improvement in the analysis (physical, physiological, phonetic, musical, psychological) of the voice may make possible a comparatively rapid and deliberate modification of the voice and speech in the direction of greater effectiveness.

This inevitably provokes reflection upon the neglect in our schools of teaching speech as a means of conveying thought.

Mr. A. Rex Knight.—*The Psychology of Facial Expression.*

(i) Certain facial 'patterns' are uniformly regarded as expressing certain mental states. This fact is relied on by novelists when they seek to convey their hero's feelings and desires by describing the appearance of his face. It underlay those silent moving pictures in which the mental history of the persons involved was revealed by photographs of their faces, not by sub-titles. And the very phrase 'facial expression' shows that the face is believed to express what is happening in the mind. (ii) My experiments were not designed to confirm this obvious fact. Their purpose (like that of Feleky and Frois-Wittmann) was to determine what mental states we most constantly and consistently associate with facial expressions. Forty different facial patterns, provided by two men and two women, were photographed. The photographs were shown to 200 adults, who were asked simply to write down what mental state the face seemed to them to express in each case. (iii) Among these 200 people there was significant agreement concerning those facial patterns which were judged to express simple feelings and emotions, such as pleasure, pain, fear,

anger, surprise, disgust, complacency, coyness, &c. This accords with the evidence of Burt and others, who found, when they attempted to assess the character of the same individual in an interview, that their judgments on simple emotional qualities, like timidity and cheerfulness, were far more consistent than their judgments on other qualities. (iv) Subsidiary experiments showed how the influence exerted by the several features on the whole facial expression differs in different expressions. For example, whereas a frowning brow dominates the expression of anger, a raised upper lip dominates the expression of disgust. These experiments confirmed the detailed results obtained by Frois-Wittmann and the general observations of Bell and Darwin. (v) There is evidence that in situations where a person tends to experience feelings which he or society condemns (*e.g.* a tinge of pleasure in another's misfortune) the face sometimes betrays the fact. (vi) A problem requiring more detailed discussion than it has hitherto received is: Granted that each emotion has its own characteristic facial expression, why is it expressed by just that facial pattern and not by another?

Dr. H. Banister.—*Sentiment and Social Organisation.*

Dr. Miles and Mrs. Raphael.—*Industrial Psychology in U.S.S.R.*

The attitude towards vocational guidance and selection in Russia is influenced by the fact that due to the recent immense increases in industrialisation there is a great shortage of workers. Both young and older novices need vocational advice, and training. All jobs are open to men and to women, and strenuous attempts are made to place workers in suitable occupations so that the present heavy labour turnover can be reduced.

The Soviet has thoroughly accepted the principles of industrial psychology, and as nearly all enterprises are State-controlled this means that psycho-technical methods are applied on a large scale.

For example, there are the Railway Psycho-technical Laboratories. The Moscow branch alone employs twenty-five psychologists and ten statisticians, and tests all prospective railway employees in the Moscow district. Work is also done on rationalisation and analysis of accident causation. For these purposes the State gives a grant which in 1932 is half a million and in 1933 will be one million roubles. There are eighteen similar railway psycho-technical laboratories throughout the country. Work is conducted on a similar scale in other occupations. The Tramways and Motor Psycho-technical Laboratory tests all car and tram drivers before they are allowed a licence, and the recently formed Post Office Psycho-technical Laboratory is devising tests for all postal employees.

Vocational guidance is given to all children leaving school. There are various research centres (for example the Institute of Hygiene of Labour at Leningrad), and the methods they advise are applied throughout the country. The Institute of Hygiene of Labour is also conducting extensive research on all problems connected with the environmental conditions of labour—ventilation, lighting, glare, dust, &c., and the results are given practical application in factories.

Interesting work is also being done at the Central Institute of Labour, where a study is made of the most efficient working methods. Picked men are taught these methods, and go out to various factories to act as centres for training and improvement of technique, and as leaders of 'shock brigades.'

The value of training is fully realised. For instance, the technical school of the new ball-bearing factory started with a thousand pupils three months before the factory was due to open.

But for the psychologist visiting the U.S.S.R. the most interesting observations are the methods used by the Soviet for the propagation of their doctrine and the motivation and incentives provided.

The 'glory of labour' is everywhere emphasised. Workers are instilled with the idea that they are the owners of their work, and that they must labour fully not only for themselves but for their fellow workers. They are encouraged by posters, plays, radios, honours, and special 'shock brigades,' and the comparative effectiveness of these various stimuli are carefully observed.

It is interesting to note that enthusiasm for industrial psychology is probably stronger in the U.S.S.R. than in any other country.

SECTION K.—BOTANY.

Thursday, September 24.

Prof. A. C. Seward, F.R.S.—*A Contribution to the Early History of the Lycopodiales.*

The paper deals generally with the history of the Lycopodiales, more especially with certain little-known Palæozoic genera from South Africa and the Belgian Congo. Attention is called to the occurrence in Devonian rocks of stems with leaf-scars apparently without any indication of leaf-traces. Reference is made to the geographical distribution of extinct Lycopodiales.

Prof. J. H. Priestley, D.S.O.—*Phyllotaxis from the Standpoint of Development.*

Phyllotaxis is examined from the standpoint that the ordered succession of leaf primordia at a growing shoot apex involves the mutual adjustment of a series of successive 'growth-units,' each unit consisting of a leaf and a portion of the subtending shoot axis.

The first appearance of the primordium is to be traced to the relatively greater extension of the meristematic surface layer, as compared with the core of vacuolating, dividing tissue upon which it stands. These considerations attach importance to the procambial strands as regions of least transverse expansion, above which the superficial tissue will therefore be piled in a fold. In the Dicotyledon the folds thus produced undergo less horizontal expansion and give rise to 'sectorial' growth-units ; in the Monocotyledon they undergo extensive lateral expansion, and therefore completely surround the short apex, as peripheral or 'holocyclic' growth-units. This difference accounts for the different embryonic organisation which has provided the names for these two natural groups.

Successive primordia are competing growth centres, therefore Hofmeister's generalisation holds that each successive primordium tends to appear on the axis as far as possible from the preceding one.

If no more than two primordia are growing at the apex, therefore, they tend to be opposite each other and the phyllotaxis, alternate and two-rowed. When more than two primordia are growing at once this position is impossible, and the angle of divergence between primordia is less than half of the circumference, but always tends to be more than one-third. Examined from this standpoint, if primordia appear in succession at equal intervals of time (the plastochrone), a spiral phyllotaxis system will result, in which the mutual adjustment of the successive growth units will lead naturally to the Fibonacci series of fractions to express the angular divergence. In this series, however, the ideal angle of divergence and the genetic spiral have no real significance, the governing developmental factors are Hofmeister's rule and the need for lateral adjustments of sectorial growth units in the Dicotyledon ; peripheral or holocyclic growth-units in the Monocotyledon.

Prof. J. Doyle.—*Some New Contributions to Conifer Morphology.*

The structure of the Abietinean micropile, its manner of closure and the method of pollination in the group have been less completely studied than other aspects of Abietinean morphology. The existing rather scattered records have been brought together and an endeavour made to complete them. The micropilar structure is definite and peculiar in each of the genera ; it is never simply tubular as in other groups, and apparently no pollination drop occurs, the micropile having a stigmatic function.

The other observations deal with the gametophyte and vegetative structure of some of the less-known Conifers : *Fitzroya, Athrotaxis* and *Saxegothaea.*

The gametophytes and embryo of *Fitzroya* have been studied. It seems an interesting intermediate form between the Cupressoideae in the sense used by Saxton (Cupressineae of Seward) and the Callitroideae of Saxton (Callitrineae of Seward), its closest relation being with the latter.

Some data on the unique development in *Athrotaxis* have already been published. The study of the genus has been continued. The archegonium, septate at the first

nuclear division; the wood structure—especially young stem and root wood; even the cuticular structures when all three species are kept in view; these and other aspects of structure all point to a close relationship with Sequoia. If the relationship holds between Athrotaxis and Taiwania which has been claimed by Sorger on certain points of vegetative structure, then the group of the Sequoideae of Saxton (Sequoiineae of Seward) may be a more extensive one than at present constituted. *A. laxifolia* may possibly be a wild hybrid between the other two species.

Prof. D. Thoday and Miss A. J. Davey.—*The Mechanism of Root Contraction in Oxalis and some Monocotyledons.*

The essential features of root contraction previously described for *Oxalis incarnata* are exhibited by other species of *Oxalis* including *O. martiana, cernua,* &c.

Remarkably similar features, indicating a similar mechanism of contraction by longitudinal shrinkage, through degeneration of protoplasm, withdrawal of sap and collapse of cells in transverse zones, are also found in certain Monocotyledons, including *Brodiaea lactaea, B. capitata, B. grandiflora* and some Dactylorchids (*e.g. Orchis incarnata*).

Further work on *Oxalis incarnata* has shown that a preliminary phase of true growth contraction precedes the more extensive contraction by longitudinal shrinkage.

Prof. F. E. Lloyd.—

(*a*) *Conjugation in Mougeotia.*

Early phase. The preliminary contact of filaments prone to conjugate having been established, there is a local adhesion to the point of growth of the conjugation tube, where a stiff, water-insoluble mucilage acts as an adhesive. Follows a sharp bending away of both filaments on either side of the adherent area, sometimes accompanied by enlargement of the gametes. Both these phenomena take place without apparent change in the osmotic value of the sap.

Fusion of the gametes begins after the resorption of the canal wall. At first there is a vigorous movement of cytoplasm in the tube, streams travelling in opposite directions between the gametic masses, of which the optically most readily apprehended are the chloroplasts. The latter approach each other mutually within the tube, where they meet and where there is a coiling up and crowding in of the chloroplasts, nuclei and adherent cytoplasm. The utricular cytoplasm is left behind, this never leaving the wall (de Bary). Threads of cytoplasm are drawn out by the moving chloroplasts. These threads offer resistance to the movement as well as others aid it, and the movement appears autonomous. It proceeds until an irregular mass is formed of the chloroplasts and adherent cytoplasm, too large for the definitive space available within the limits of the zygote to be. This ends the movement of approach. Follows the period of condensation during which water is thrown off and the zygotic mass takes on the definitive lens shape. There now begins an extraordinary activity of the cytoplasm external to the zygotic mass. This consists in the extrusion of large bubbles of cytoplasm in such profusion that it suggests the boiling of a very viscous fluid. Balloon-shaped bubbles extend out and wave to and fro, sometimes fusing and all collapsing at last. This abates only with the final condensation of the zygotic mass. It would seem to be a method of rapidly throwing off water, but it is not clear that the mechanism is identical with that in Spirogyra, in which contractile vacuoles occur.

(*b*) *Vampyrella lateritia—a Regurgitating Mechanism.*

The behaviour of *Vampyrella lateritia* during and after feeding on *Mougeotia* has been recently studied and a remarkable activity in the rejection of presumably digestible material observed, by no means comparable to defecation.

The individual in question began to feed on a pair of conjugants. The procedure of withdrawing the cytoplasm and chloroplasts is quite as described for *Spirogyra* (Lloyd, Protistenkunde 67, 219–236, 1929). The contents of both gametes were withdrawn together. After retiring from the emptied cells, the animal showed disinclination to move away, yet seemed to be disquieted. Continued watching was rewarded by seeing pseudopodal activity set in. Thick, blunt, cylindrical pseudopodia were protruded five times in succession from various places. At the outer end of

each there was a mass of recently ingested material, consisting of a large starch mass with a shell of chloroplast material in normal condition, showing no change of colouring or other signs of digestion. It was evident that these were starch masses with which the chloroplasts of the *Mougeotia* gametes were loaded. When the pseudopodia had attained a length of about one-fourth the diameter of the animal the rejectamenta were discharged and the pseudopodia withdrawn. Vacuoles could be seen in the clear protoplasm of the pseudopodia. The whole process is quite distinct from the ejection of 'waste,' meaning the fæces properly meant.

AFTERNOON.

Prof. A. C. SEWARD, F.R.S.—*Some Aspects of the Bearing of Palæobotanical Evidence on the Evolution of the Plant World.* (Semi-popular Lecture *in the rooms of the Linnean Society, Burlington House, Piccadilly.*)

Friday, September 25.

Dr. E. P. PHILLIPS and Mr. R. A. DYER.—*The Genus Sutherlandia R.Br.*

The name *Sutherlandia* was first applied by Gmelin to a member of the family *Sterculiaceae,* but was later sunk under *Heritiera* Dryand. There is thus justification for the retention of the name *Sutherlandia* R.Br. The genus was regarded by E. Meyer and Harvey as monotypic, but on examination of a large series of specimens and taking into account the geographical distribution, six species are now recognised from South Africa.

Dr. E. P. PHILLIPS.—*A Conception of a Modern Herbarium.*

Prof. N. J. G. SMITH.—*Comparisons emerging from a Study of the Genus Helminthosporium in Britain and South Africa.*

The following species parasitising S. African cultivated crops are mentioned—*avenæ, gramineum, teres, sativum.* That order of writing the four names places the rarest first, and the most malignant last. That order, which holds true for S. Africa, by no means holds true for Britain. This is partly explained by the effect of different temperatures on the growth-rate of the fungi.

Some of the important cereal parasites can attack wild grasses, *e.g. H. sativum* is found on an English *Festuca* and a S. African *Urochloa.* Rather numerous fungi of this genus, belonging to species different from those which attack cereals, are found on wild grasses in the two countries.

Prof. Dame HELEN GWYNNE-VAUGHAN, G.B.E., and Mrs. H. S. WILLIAMSON.—*Sex in Ascobolus magnificus.*

Spores of *Ascobolus magnificus,* one of the few members of the Diascomycetes with male and female organs, were received from Dr. B. O. Dodge, of New York. The fungus grows well in single-spore culture, but few sexual branches are produced, and these soon die. Mycelia appear to be of two kinds, each capable of bearing both male and female organs; when they are brought into contact the sexual branches are numerous, and abundant fruits develop. The sexual branches are at first very similar, each ending in an elongated cell rich in contents. Their tips make contact and become firmly attached, so that the growth of the female branch causes it to twist once or twice round the male. The twisted portion develops six or seven septa and constitutes the trichogyne. Below it the oogonium enlarges, the nuclei divide and move to the periphery. Meantime the wall between the antheridium and trichogyne has disappeared and the male nuclei pass into the latter. They are readily distinguished by their large size and elongated nuclei, and can be traced from cell to cell till they reach and enter the oogonium. After fertilisation ascogenous hyphæ grow out and asci are formed. The haploid number of chromosomes appears to be four.

Dr. Mary J. F. Gregor.—*Heterothallism in Ceratostomella pluriannulata, Hedgcock.*

This species is characterised by the extremely long neck of the perithecium which bears, in addition to the usual terminal fringe of cilia, accessory fringes lower down the neck. A conidial stage of the *Cladosporium* type occurs in the life-cycle, but no *Graphium* form.

The fungus was isolated from elm wood and numerous series of monoascospore cultures were made; it was found to be heterothallic, the + and — strains being produced in about equal proportions. Female organs are formed on both mycelia, but no male organs. When the two strains are grown in association, these female organs develop into normal perithecia containing spores, but when one mycelium is present alone, they undergo only a partial development, giving rise to small spherical 'bulbils.' Occasionally a few bulbils may continue growth and form dwarf perithecia, which, however, never contain spores.

Numerous experiments have been carried out in an attempt to induce a more abundant production of dwarf fructifications and to stimulate these to develop spores in the absence of the second strain of the fungus. The experiments include the addition of various chemicals to the nutrient medium on which the cultures were grown, the addition of a filtered watery extract of the other mycelium, the action of heat, extreme cold and ultra-violet rays. These methods, however, have all failed to produce the desired result, and it appears probable that mature perithecia can only be formed by the interaction of the + and — strains.

Prof. O. V. Darbishire.—*Observations on the Occurrence of Air Pores in Lichens.*

Little is known of the detailed anatomy of lichens, and still less of their physiology. In their general anatomical differentiation lichens resemble somewhat the higher green land plants, but the lichen organism differs markedly from the green land plant in the varying development and the unequal and irregular distribution of air pores, by which the air in the loosely plectenchymatous medullary layers might be connected with the outside air. In some foliaceous and fruticulose lichens the air passages of the medulla seem to be completely cut off from the surrounding air. In others practically the whole underside of the foliaceous thallus is open; in still others we find well-defined cyphellæ of definite size and shape, and in a few, special pores of smaller size. In *Peltigera prætextata* these resemble stomata in appearance. The air pore is the response of the fungus to the control by the lichen gonidia. The algæ must be kept from drying up. This control results in the fungus of the corticate lichens always keeping the algæ well covered. Transpiration in lichens is always slow despite the absence of any cuticle. Air pores are generally found on the lower side of the thallus, and thus transpiration through them is kept under. The size and structure of the air pore is a compromise between the general requirements of the algæ and the average danger of their desiccation. The method of development of the pores in *Peltigera prætextata* may possibly serve as an indication of the way in which stomata originated in the very earliest ancestors of the green land plants of to-day.

Gen. the Rt. Hon. J. C. Smuts, P.C., F.R.S.—*Observations on a Botanical Tour in Tanganyika.*

Afternoon.

Dr. F. M. L. Sheffield.—*Some Cytological Aspects of Virus Diseases in Plants.*

Dr. J. Caldwell.—*Investigations on Some Physiological Aspects of Virus Diseases in Plants.*

For the physiological investigations discussed in this paper one virus Aucuba Mosaic of Tomato in its host Tomato (*Lycopersicum esculentum*) was largely used. This virus was chosen because it is easily transmitted by juice inoculation, gives consistently well-marked and definite symptoms and remains active in vitro over

long periods. Other viruses and host plants were used primarily to confirm the results obtained with Aucuba Mosaic in Tomato.

The movement of the agent about the host plant after inoculation was studied. When inoculation is made on any one leaf the symptoms of disease appear systemically, there being no localisation of symptoms about the region or side of inoculation. When a band of tissue half-way up the stem is killed so that only the xylem system remains functional, infection may be induced either above or below the lesion, but the agent will not pass from one part of the plant to the other when inoculation is made in only one part.

Virus juice may, however, be introduced through a cut petiole directly into the xylem vessels, and can travel freely in the water stream. On the other hand, the agent is apparently unable to leave the vessels in which it has travelled to infect the living cells of the plant. When the vessels are ruptured and their contents brought into association with the living cells of the mesophyll infection takes place. In the diseased plant the agent does not normally enter the xylem vessels, and is not found in the water stream or in the hydathode exudate.

The evidence available suggests that the agent does not normally enter an unbroken cell, and that rupture of the wall and of the protoplast is necessary. On the other hand, if too extensive damage be done, or if the inoculum be slightly toxic, numbers of cells are killed round the point of inoculation and the agent is unable to enter the living cells of the leaf. It would appear that movement from cell to cell in the plant along the protoplasmic strands is fairly easy. The removal of a large proportion of the vascular tissue does not retard the upward movement of the agent. Further, the rates of movement of the agent up or down the plant from the point of infection appear to be the same. There is no evidence of a tendency to greater movement in either direction.

The general effect of the disease on the metabolism of the plant has been studied. The rate of respiration has been selected as a criterion of general activity. The rate is being determined for diseased as against normal tissue. It has been found that, while virus disease considerably alters the appearance of the host plant—especially as regards Aucuba Mosaic in Tomato—the stage of development is apparently not affected. That is to say, the diseased plant is as fully developed at any given time as is the healthy control.

Dr. H. G. Thornton.—*The Influence of the Host Plant in Controlling the Formation and Functioning of Root Nodules in the Leguminosæ.*

Miss M. D. Glynne.—*Infection by Synchytrium endobioticum of Potato Varieties previously considered immune.*

Varieties of potato which appeared immune to wart disease in the field were formerly supposed to be absolutely immune, showing no trace of susceptibility under any conditions. More intense methods of infection have, however, shown that among these varieties are a considerable number in which incipient infections appear, but do not develop into true warts. They tend to disappear as the plant grows. A microscopic study of incipient warts shows that the parasite reaches different stages in different immune varieties and may produce ripe summer sporangia and much more rarely winter sporangia. Necrotic areas are found frequently in the neighbourhood of the parasite by means of which it is possible that the plant sloughs off the parasite or reduces the virulence of its attack.

Mr. R. H. Stoughton.—*On the Cytology of Bacterial Plant Parasites.*

There are two distinct but allied problems in bacterial cytology and morphology. First, do bacteria possess a true nucleus comparable with that of all other known living cells; second, have bacteria any means of reproduction other than simple transverse fission and endospore production, and if so, what cytological changes are involved in the formation of these reproductive bodies? Both of these problems have been attacked in the present work, carried out mainly on *Bacterium malvacearum*, the cause of a serious disease of the cotton plant, but extended to include certain observations on various other plant-pathogenic bacteria.

Using special technique it has been possible to show that in the normal vegetative cell of *B. malvacearum* and some other types there is a single, central, deeply-staining, spherical body. This body reacts to all nuclear stains, including the elective Feulgen stain. Immediately prior to, or coincidently with, the division of the cell-body, this central structure also divides in a characteristic way, and the two halves pass one into each daughter-cell. From this and other evidence it is concluded that this body represents either the nucleus of the bacterial cell or some part of it.

At least three different types of secondary bodies have been found to be produced in cultures of *B. malvacearum* and their method of formation followed, mainly in stained slides. The first is a minute spherical body, formed in the wall of the cell and liberated either by simple extrusion or by growth on a stalk. This body is comparable with the 'gonidia' of other workers. It has not been proved to be capable of further development. The second type is a spherical coccoid form, much larger than the first, formed by a process of budding from the parent cell. The formation of this body is associated with a division of the nucleus-like structure, and half of the parent 'nucleus' passes into the new body. The coccus becomes abstricted off and subsequently germinates to form a normal rod.

The third form appears to be produced from two cells attached at one end. From the point of junction arises a body similar in appearance to the coccoid body, but often more deeply-staining. After liberation this body is apparently indistinguishable from the latter.

Extending the same technique to other plant bacteria it has been found that, while certain species appear to conform closely in their structure to the type of *B. malvacearum*, others show decided divergences. Attempts are being made to group the forms into classes characterised on a morphological and structural basis.

Monday, September 28.

Presidential Address by Prof. T. G. Hill, on *The Advancement of Botany* (see page 196).

Discussion on *The Training of Botanists for Economic and Industrial Positions.* (Sir John Bretland Farmer, F.R.S.; Prof. V. H. Blackman, F.R.S.; Dr. W. B. Brierley; Mr. J. Ramsbottom; Sir Arthur W. Hill, K.C.M.G., F.R.S.)

Sir J. B. Farmer, F.R.S.

In view of the increasing number of research posts at home and still more in the Empire overseas, which are open to botanists, it becomes a pressing matter to reach some understanding as to the best ways of training the candidates for such positions.

The ideal would be that it should be based on the right sort of biology which ought to form part of the curriculum of the schools. Biology is taught in a growing number of schools, and in some it is very well done. In the majority, however, there is still much room for improvement. Of course the school training alone will not fit a boy to be a candidate for these appointments, which are mainly open only to those who have had a suitable university training in which botany has occupied an important place. But a good introduction to the subject at school will have laid an excellent foundation for the later university work, and though the lack of it may, it is true, be partly or even largely made good by subsequent effort, the boy who has had such a grounding will possess an incontestable advantage over those who are without it. But unfortunately by no means all the schools give a real scientific grounding, and the university might well utilise the pressure which the scholarships and exhibitions they provide enable them to exert, to see to it that breadth rather than height is aimed at during the earlier years at school. Such a course would assuredly lead to a better final product. And this is true not only of biology.

But as things are, it must for some time be the case that some—probably many—of the potential candidates for these appointments will continue to enter the university with little or no preliminary knowledge of botany, and only decide to 'change over' when they learn of its possibilities at this stage of their careers. Nevertheless, they need not be discouraged, provided they are prepared to face up to the work in spite, it may be, of some initial handicap.

As to the university course, a good class in the Honours Final School is expected from the candidates, who will normally take four years to complete the course for the degree. During the undergraduate period it would be a mistake to emphasise the vocational aspects of the science. These will come later on. The undergraduate period is the stage during which he needs to be trained broadly in scientific method, and in the main branches of his principal subject. Thus he will acquire not only a right scientific insight, but a suitably broad outlook over his branch of science, in this case botany. By no means should the more vocational objects be allowed to interfere with this laying of a broad foundation of knowledge which will be priceless to him later on, but if missed now will probably never be made good.

It is specially important that the training should encourage the desire of investigation and research. And by this I do not mean a *set* 'research,' which I deprecate at this stage, but that the laboratory work itself should be so planned and conducted as to serve as a training in *research*, instead of, as is sometimes the case, a more or less mechanical method of merely *verifying* what the student has been told in lectures. This is a really vital matter which may largely make or mar the student as an investigator.

As to the subjects which the course should include, it is clear that the biological interactions of the plants and their environment (in the largest sense) must form an important part of their work. Plant physiology, and the essentials of pathology, will necessarily form subjects of study. Some knowledge of genetics and cytology will naturally be included, but genetics is a vast and specialised subject, much of which is more suitable for study after a man has taken his degree, and then mainly for those who have the special qualifications required for its successful prosecution.

As a rule, when a man has taken his degree he will apply for an appointment, but it is very profitable for himself, and at least equally for his employers, if he can spend further time in being trained specifically in research. He will thus be shown how to escape pitfalls and to formulate his plans of investigation on most profitable lines. In other words, this is the period when he should spend some time in vocational training.

Adequate scholarships are provided for this both for cadets entering the Colonial service and also for those engaged in research under the Empire Cotton-growing Corporation. These scholarships are usually continued for two years. There are also means for training of an analogous character provided by the Ministry of Agriculture and various other bodies, whilst some of the planting associations and companies are not seldom willing to give facilities for special training to their entrants before embarking directly on their professional careers. And opportunities of 'study leave' are fairly amply provided for, and are invaluable as enabling a man to keep in real touch with current investigation.

Dr. W. B. Brierley.

The narrow academic vision and experiential inadequacy of most university teachers of botany, reinforced by their irrational fear of applied developments and by the vested interests of the subject, have led to an almost total neglect of 'applied' botany and an obsessive concentration on 'pure' science. As a result most academic schools are not competent to train botanists for economic and industrial positions, and are only competent to teach 'academic' botany to students who in turn become teachers of 'academic' botany. The training of botanists for economic and industrial positions demands a much wider basis of knowledge and experience, a different outlook and a different balance of values from those in present academic teaching. This orientation does not imply any loss but rather an enhancement of educational and cultural values, which do not lie in a subject *qua* subject but in one's relations to a subject. It does, however, imply a different source of inspiration—the growing plant in both its natural and its humanly created environment rather than the preserved specimen; industrial and economic need rather than academic tradition; the practical reality of everyday life rather than the precious artificiality of scholasticism. It implies different ideals and cultural values having root not in classicism but in human welfare and industrial and agricultural progress. An adequate training would involve a fundamental reorganisation of the university botanical curriculum so that the institution and the training become fitted to the needs of the subject and the student; at present the subject and the student are made to fit the needs of the institution and the training. An adequate training

would involve the outbreeding of university staffs, intimate relationships with non-academic agricultural, horticultural, forestry and industrial institutions of both research and of practical nature and botanical organisation comparable with hospital training in medicine or works experience in engineering. The necessary facilities for such a training could perhaps be organised profitably and adequately at one or two centres only, and it would need to be recognised that students embarking upon a career of industrial or economic botany and not 'academic' teaching would proceed for their education to those centres. The basic ideas of the training would be, on the one hand, the realisation by the student that botany is not a subject of scholastic or cultural value only, but has its real root and justification in the practical problems of industrial and agricultural life and, on the other hand, not only the education of the student's mind but the acquisition by him of such knowledge and such experience as would ensure that he become a competent worker in the economic as well as in the scholastic sense.

Mr. J. Ramsbottom.

In training botanists for economic and industrial posts it is essential that they should first undergo a course in general botany, in which the principles of the subject are studied. With this foundation the specialised study becomes more easy of attainment and more certain in its application. Considering these posts from the point of view of a taxonomist one sees that a working knowledge of classification plays a greater part than is usually recognised. Some have obviously little concern with taxonomy in general, though even here the success of a project may depend upon the certainty of an identification. The majority, however, need a much wider outlook on taxonomy than is at present obtainable at most of our universities.

The courses in botanical training at the universities have undergone little real change during recent years, though the majority of botanical posts, as such, are no longer at the universities; moreover, botany taught in schools is mainly a diluted university course.

It is not possible to cover the whole field of botany at a university and, consequently, if the courses were reorientated so as to give proper attention to the teaching of the principles of taxonomy no loss in value as a discipline would result; 'botanical classification, when complete and correct, will be an epitome of our knowledge of plants.'

The facilities so far as they exist for post-graduate training in the practice of taxonomy are in need of drastic overhauling. The kind of training should be related to fitting a man for the post he is to occupy, not to producing a herbarium systematist. If the universities are unable to supply the whole of the necessary training, an attempt should be made to arrange for this to be undertaken elsewhere.

Tuesday, September 29.

(Section meeting in two divisions.)

Division 1.

Discussion on *Factors Governing the Distribution of Plants.*

Division 2.

Series of Papers on certain aspects of Plant Physiology (as below).

Dr. F. G. Gregory.—*The Control of Physiological Processes in the Barley Plant by Mineral Nutrition.*

Dr. F. C. Steward.—*The Accumulation of Solutes by Living Cells: Some Experimental Results.*

An experimental examination of the environmental conditions which determine the absorption and accumulation of potassium bromide from dilute aqueous solution by potato tissue revealed that there is an intimate connection between salt absorption and the respiration rate of the tissue. A suitable technique was devised which secured maximum absorption and also admitted of satisfactory control of all the known variables affecting absorption by storage tissue (temperature, oxygen-carbon-

dioxide relations of the tissue, stirring, surface-volume relations of the tissue, external salt concentration, &c.) and permitted accurate estimation of the carbon-dioxide produced by the tissue. This involved several novel features especially designed for this investigation. Maximum absorption was obtained under those conditions which produced maximum aërobic respiration, and this was secured by using a rapid, controlled stream of gas of known composition for aëration. A preliminary survey of the variables affecting absorption of potassium bromide by potato suggested that this accumulation is determined by the metabolic activities of the living cell. The oxygen partial pressure in the aërating gas stream by determining the respiration rate and, therefore, the energy available to the system, also determined the salt absorption. The relationship between respiration and salt absorption was also evident when the behaviour of similar discs of varying thickness were studied. It was found that a thin surface layer of immersed discs of potato tissue respired much more actively than the central core. Moreover, it was apparent that these cells of highest respiration rate attained the highest salt content. These observations could be correlated also with certain anatomical and cytological changes which occur in the surface cells. The form of the absorption-time curves seems to indicate that previous curves supposed to indicate the attainment of a physico-chemical equilibrium are really due to the neglect of the oxygen-carbon-dioxide variable. Data obtained simultaneously for salt absorption and respired carbon-dioxide revealed certain difficulties in the way of interpreting salt absorption as a direct interchange of $\overset{-}{HCO_3}$ ions for absorbed anions and $\overset{+}{H}$ ions for absorbed cations. It appears probable that anions and cations may be absorbed in stoichiometrically equivalent amounts. The potassium bromide absorbed appeared in the readily expressed sap, could be recovered quantitatively, and caused the expected increase of electrical conductivity of the sap. Apparently it existed in true solution in the vacuole. These facts are in accordance with the idea that the living cell does work, the source of energy being a metabolic one, not only in effecting absorption but in maintaining existing concentration gradients, and are not in accordance with an adsorption or other equilibrium mechanism.

Dr. W. H. Pearsall.—*Temperature Effects in Plant Metabolism.*

The transformations of material taking place in plant tissues kept at different temperatures have been studied, in collaboration with Miss M. Pilling. The tissues used consisted of leaves kept at temperatures between 0° and 26° C. for periods of from thirty to fifty hours, and also of apples stored for more prolonged periods. Insoluble materials tend to be hydrolysed at low temperatures below 5° C. as well as at higher temperatures above 15° C. These changes have apparently not been recognised previously for proteins or nitrogenous constituents. They occur when carbohydrates are present in excess and their existence throws considerable light on certain current problems.

Dr. Winifred E. Brenchley.—*The Value of Nitrogen at Different Periods of Plant Growth.*

The value of nutrient elements, as nitrogen, phosphorus and potassium, varies throughout the lifetime of the plant, and their presence or absence is more critical at some stages of development than others. With barley, grown in water culture, the absence of nitrogen during the earlier stages causes rapid decrease in general growth and in ear development. If nitrogen is withheld for several weeks at this time the plants tiller freely, though ear formation is inhibited, but if starvation is very prolonged tiller production ceases and a travesty of an ear is produced at the expense of the nitrogen stored in the grain, little response being made to later supplies of the element.

The reverse process, *i.e.* withdrawal of supplies after an initial period in the presence of nitrogen, is less disadvantageous. Normal ears are produced even when nitrogen is given only for the first month, and if it is present for the first eight or ten weeks the plants develop well and produce plenty of ears, which ripen off in good time. Longer periods of presentation tend to encourage increase in height up to a certain limit, but eventually maximum yield is reached, and later supplies of nitrogen have no beneficial effect, tending rather to delay ripening and render the plant more susceptible to disease attacks.

The behaviour of *Vicia faba* under parallel circumstances is very similar, except that, owing to the relatively large amount of nitrogen supplied by the seed, the effects of early privation are somewhat delayed, and greater proportionate growth is made where nitrogen is entirely withheld.

With both species tested, although the yield of dry matter is not increased by the provision of nitrogen during the later weeks of growth, the uptake of nitrogen continues, resulting in a progressively higher percentage content until the plants are approaching maturity. From experiments now in progress it will be possible to determine the distribution of this later absorbed nitrogen throughout the plant.

Comparison of the response of barley to early and late supplies of nitrogen and phosphorus suggests that while the absorption of either element, if available, continues to a very late stage of growth, yet for the production of maximum yield nitrogen must be supplied for a longer period after germination than is necessary with phosphorus.

On the other hand, though the supply of both elements during the first month or more of growth is essential for satisfactory ear production, the omission of phosphorus at this time causes more complete inhibition of ear formation than if nitrogen is withheld. It would appear, therefore, that barley can utilise supplies of nitrogen, after initial periods of starvation, more readily than it can respond to the provision of phosphorus under similar circumstances.

Dr. Blodwen Lloyd.—*Bacterial Denitrification.*

Bacterial denitrification is an oxidation-reduction process whereby nitrate is completely reduced to elementary nitrogen. The reduction takes place in three stages, from each of which oxygen is obtained by the reducing organism. The oxygen thus available is utilised for respiration. Nitrogen is liberated as waste metabolite only at the last stage.

Dr. Cranston and Dr. Lloyd have found from their recent researches that by continuous measurements of the rate of gas production a ‘ denitrification curve ’ may be obtained for an actively denitrifying broth culture. Such a denitrification curve resembles in general a growth curve, but the two curves do not correspond exactly at the initial stages, for there is a ‘ chemical lag ’ in addition to the ordinary bacterial lag. The total amount of nitrogen produced by a culture is a measure of the aggregate activities of the contained bacteria, and the rate of gas production at any given time is a measure of the number of active bacteria in a culture at that time.

The following experimental conditions favour denitrification : low initial concentration of nitrate (6 mg. per 10 c.c. of broth culture), high incubation temperature (37–40° C.), an alkaline reaction (pH 8·2), and little or no free oxygen.

Dr. E. P. Smith.—*Calibration of Flower-colour Indicators.*

An attempt has been made to determine the pH value of the cell-sap of some coloured flowers by calibrating the anthocyanin pigments of the flowers. The pigment is extracted directly from the flower by heating the petals in samples of buffer mixtures of known pH values. In this way a series of colour-standards is obtained, showing the colour assumed by a particular flower-pigment over a chosen range. The short period of heating (5–10 seconds) gives a clear, brilliant extract which keeps its colour well, especially in the acid range. Four different buffer-mixtures were tested, of which the ‘ Universal Buffer Mixture ’ of the British Drug Houses proved most satisfactory. (For further details of the method see Smith (2).)

It was found that the flowers fell into three groups, as has been suggested by Buxton and Darbishire (I), namely, pure red, magenta (intermediate) and blue. The pure reds and blues can usually be assigned to their group by inspection, but not always (*cf. Primula sinensis* var. ‘ Etna,’ *Viola cornuta*). The reds were difficult to calibrate with the pH standards, because there were often two ranges of brown-red, about pH 6 and again about pH 9. Two cases were noted in which the anthocyanin was so concentrated in the cell sap that it was not ‘ indicating ’ ; these were the royal blue *Cineraria* and the very dark red *Nicotiana*, Sander’s hybrid.

The following flowers were studied :—

Name.	Colour of Flower.	pH of Flower.
1.—*Pure Reds.*		
1. *Anemone* 'St. Brigid'	scarlet.	3
2. *Cineraria*, Feltham Beauty strain (Sutton)	brick-red.	6·2?
Grandiflora (Storrie)	brick-red.	6·2?
3. *Hibiscus* sp.	scarlet.	3
4. *Nicotiana* × *Sanderae*.	very dark red.	—
5. *Primula beesiana* × *bulleyana*	bright pink.	about 4
6. *Primula japonica*, red form (near Briscoei)	salmon pink.	3
7. Tulip 'Cramoisie Brilliant'	scarlet.	—
2.—*Magentas* (intermediates).		
8. *Anemone* 'St. Brigid'	puce.	4·5
9. *Cineraria*, Feltham Beauty	deep magenta.	6·4
10. *Petunia*, hybrid	rose pink.	5·0
11. *Primula burmanica*	pale lilac.	4·5
12. *P. Japonica* (*type*)	magenta.	3·5
13. *P. pulverulenta*	magenta.	3·1
14. *P. sinensis*, 'Etna'	deep red.	3·1
15. *P. sinensis*, 'Reading Ruby'	deep magenta.	3·4
16. *P. Sinensis*, 'Blue Star'	violet blue.	5·2
17. Tulip	wine-purple.	—
3.—*Blues.*		
18. *Anemone* 'St. Brigid'	purple.	4·5
19. *Cineraria*, Feltham Beauty	royal blue.	—
20. ,, 'Wonder Queen'	pure blue.	7·4
21. *Delphinium tatsienense*	pure blue.	7·3
22. *Ipomœa Leerii*	pure blue.	7·8
23. Petunia × 'Poor's Blue'	royal blue.	5·0
25. *Viola cornuta*	violet.	4·5
26. *V. odorata*	violet.	4·4

The genetic relations of some of these forms is suggestive. The brick-red Cineraria has arisen in commerce from a pink strain ; the pinks supplied to the writer by Messrs. Storrie & Storrie as the parent strain have all proved to be *magenta-pinks* on testing. This raises the question of the origin of the other type of pigment. A comparison between the two forms of *Primula japonica*—' type ' and ' red form, near Briscoei,' shows that their pigments would place these in distinct groups ; the systematic importance of this will have to be considered. The hybrid *Primula bulleyana* × *beesiana* is interesting. The *bulleyana* parent has an orange flower, showing a ' red ' anthocyanin on the tube of the corolla and the backs of the petals : there is yellow plastid pigment and flavone throughout. The *beesiana* parent is a magenta. The hybrid has a bright pink flower with a yellow eye, the anthocyanin (which is here distributed over the whole flower) is ' red,' and plastid pigment and flavone are present. The ' St. Brigid ' strain of Anemone shows all three types of pigment: their genetic behaviour should be interesting.

References.

1. Buxton, B. H., and Darbishire, F. V. (1929).—' Behaviour of " Anthocyanins " at varying Hydrogen-ion concentrations.' *Journ. of Genetics*, xxi.
2. Smith, E. Philip (1930).—' Flower Colours as Natural Indicators.' *Trans. Edin. Bot. Soc.*, xxxii.

Afternoon.

(Section meeting as a whole.)

Two semi-popular Lectures with Cinematograph Illustrations :—

(*a*) Dr. R. N. Chrystal.—*The Wood Wasp—Sirex.*

(*b*) Dr. F. M. L. Sheffield.—*Cytological Aspects of Virus Diseases in Plants.*

Wednesday, September 30.

Miss B. D. Gregory and Prof. L. Newton.—*Further Light on the Life Histories of Gymnogongrus Griffithsiae and Ahnfeldtia plicata.*

Mr. R. E. Harries and Mr. G. Bebbington.—*Further results in Cultural Work on Laminaria.*

Miss E. M. Lind.—*The Life History and Cytology of two species of Ulothrix.*

Until recently it was considered that individual plants of all the Chlorophyceae were haploid. During the last five years an alternation of haploid and diploid phase has been demonstrated in several genera. A study of two species of Ulothrix was undertaken in order to investigate the nuclear changes in the life cycle.

The two species described—Ulothrix zonata and Ulothrix rorida—were found growing mixed together. As much attention has already been devoted to U. zonata by previous workers, the only points discussed in this paper with regard to this species are those upon which there is still some difference of opinion. The existence of microspores has recently been doubted. These are described, and also a form of vegetative reproduction by fragmentation and a hitherto undescribed branching of the filaments. The variation shown by this alga under different conditions of environment is emphasised.

Ulothrix rorida (Thuret) is considered to be a form described by Thuret in 1850 but not since included in systematic works on the Chlorophyceae. It is distinguished from U. zonata by the smaller average size of its filaments, the possession of only one pyrenoid per cell and by its spindle-shaped zoospores. This species begins to develop in October, reaches a maximum in February and then decreases rapidly. It produces zoospores throughout the autumn and winter, but in March each year there appear very numerous strongly coiled gamete-forming filaments. Gamete production coincides with low temperature, bright sunshine and low *p*H, and is followed by the death of the filaments. The change from zoospore to gamete formation is very sudden, and is conditioned by the environment rather than by the nature of the filament.

All filaments are haploid, and no reduction division can be demonstrated in the formation of any of the reproductive cells. The germination of the zygote has not yet been studied beyond an early stage, but it is suggested that here, as in Ulothrix zonata, the zygote represents the only diploid phase in the life-cycle.

Mr. F. W. Jane.—*Chromosome Studies in the Hypoxidoideæ.*

Diploid nuclei of six genera of the Hypoxidoideæ have been studied, and the variety of chromosome form occurring in this section of the Amaryllidaceæ is discussed.

In *Hypoxis* the chromosomes are small and numerous, ranging in number from about forty to ninety in different species. In *Curculigo recurvata*, where there are eighteen chromosomes, and in *Tecophilea cyanocrucus* and *T. violacea*, where there are twenty-four, the chromosomes are very small, and little success attends any effort to elucidate their structure.

The chromosomes of *Anigozanthos flavida* and *A. rufa* are larger and number twelve. These, together with the large chromosomes of *Bomarea* (18) and *Alstrœmeria* (16) yield material in which details of chromosome structure are more easily seen. The occurrence of chromonemata is described. In *Bomarea* an endogenous spiral or zig-zag thread is visible in anaphase chromosomes, and comparable threads occur in prophase nuclei of plants of this genus and *Alstrœmeria*. In *Alstrœmeria* at metaphase paired chromomeres are visible in each daughter segment of the splitting chromosomes. Constrictions and spindle fibre attachments are also described.

Miss Helena Heslop Harrison.—*Further Studies in the Chromosome Number and Morphology of the Genus Euphorbia.*

In an effort to trace the correlation between chromosome number and morphology in this genus and phylogenetic relationship, the cytology of fifty species has been studied. In every case material for examination was provided by the root tips of

seedling plants, the identities of which were afterwards confirmed while in the flowering stages. The results of this research have been extremely illuminating.

The chromosome complements have been classified according to size, number and shape of the constituent chromosomes, occurrence of satellites, position of constrictions and incidence of fragmentation. There are probably two distinct polyploid series, one in which the haploid numbers are 6, 9, 12, 15, 18, 21, 24 and 36, and one in which the numbers 10 and 20 occur. Two species have been examined, E. segetalis var. portlandica and E. fætica, in which the diploid number is 16.

Dr. J. Latter.—*Meiosis in Lathyrus.*

DEPARTMENT OF FORESTRY (K*)

Thursday, September 24.

Discussion on *Wood Preservation* :—

(1) Mr. R. S. Pearson, C.I.E.—*Research in Wood Preservation.*

A brief review is given of the different lines of approach necessary to investigate the problem of prolonging the life of timber by antiseptic treatment and of controlling the attack on timber by either fungi or insects. Stress is laid on the necessity of co-ordinating research work carried out by the different groups of investigators, without which the results cannot claim to have been based on sound scientific principles. A short review is given as to the means of obtaining co-operation between investigators in Wood Preservation, Chemistry, Entomology, Mycology and Utilisation, when studying the subject of ‘ Preservation and Control of Damage to Timber.’

(2) Mr. K. St. G. Cartwright.—*Toxicity of Preservatives against Wood-destroying Fungi.*

After giving a short general account of the damage to timber caused by fungus decay and the increased importance of wood preservation owing to the shortage of the world's timber supplies, some of the methods used for the testing of the relative value of different wood preservatives in the laboratory are described.

The comparative value of tests on an artificial medium and on wood blocks is discussed.

The necessity of amplifying laboratory tests by service or large-scale ones is emphasised, and the paper concludes by indicating other lines of research on which wood preservation may develop.

(3) Dr. R. C. Fisher.—*Prevention and Control of Damage by Wood-Boring Insects.*

Damage by insects to structural and manufactured timber in this country is of three distinct types dependent upon the insects causing it. There is the damage caused by longhorn beetles and pinhole borers, both groups essentially forest insects ; there is the damage caused by powder-post beetles occurring in partly seasoned and recently seasoned hardwoods, and finally there is the type of injury for which the death-watch beetle and its allies, the furniture beetles, are responsible in long-matured and old timber. The paper describes the occurrence of these different types of insects in timber at different stages in its utilisation and discusses the food relations of the insects at each stage. The development of preventive and control measures based on a knowledge of the biology of the insects is then described, and the use of wood preservatives and insecticides against wood-borers in this country is also discussed.

Particular attention is devoted to the problem presented by the death-watch beetle. Although a number of palliative measures are available against this insect, knowledge of the details of its life-history and ecology are lacking. It is pointed out that the acquisition of fuller knowledge of the biology and physiology of the death-watch beetle is the most hopeful contribution which can be made towards the solution of this problem, and will afford a sound basis for the application of improved control measures.

Pending the acquisition of such data, tests of wood preservatives and insecticides for the prevention and control of wood-boring insects in this country are strictly limited. An outline is given of the type of test which is at present possible, and a parallel is drawn with similar work in progress in India, Australia and other countries where termites (white ants) present the most serious timber insect problem.

(4) Mr. J. Bryan.—*Methods of Applying Wood Preservatives.*

Assuming the necessity of deep impregnation of the antiseptic for the preservation of exposed timbers, various methods are discussed of obtaining the desired impregnation. The various standard methods of impregnation by pressure difference are described, as well as methods depending on the diffusion of water soluble preservatives.

Incising as an aid to the impregnation of refractory timbers is of comparatively recent introduction, but is now being used commercially for Douglas fir railway sleepers. Different methods of incising and their application are discussed.

(5) Mr. W. G. Campbell.—*A Chemical Approach to the Study of Wood Preservation.*

The natural durability of certain woods is explained by the presence in such woods of extraneous components which are toxic or otherwise intolerable to wood-destroying organisms. It would appear that the majority of these natural preservatives are merely specific in their action, since, with few exceptions, all woods are susceptible to decay in one form or another. It is suggested that the scientific investigation of the problems of wood preservation can be materially aided by concomitant studies of the intimate biochemical relationships which exist between decay organism and host.

An account is given of chemical methods which have been used to indicate the ultimate effect of insects and fungi on wood substance.

Exhibition illustrating *Wood Preservation*, by the British Wood Preserving Association.

Friday, September 25.

Empire Forestry :—

(1) Address by Sir Alexander Rodger, O.B.E., Chairman of Department of Forestry.—*Forestry in the Empire during the last* 100 *years.*

In attempting to compile a short account of Forestry in the British Empire during the last hundred years I have been struck by the fact that almost the whole of the history of scientific importance, with a few notable exceptions such as India, is confined to the present century. It is true that forests have played quite a large part in the development of great countries such as Canada and Australia, but Forestry as a science has been until recently entirely neglected in many parts of the Empire. I would go even farther and say that many of the most important doctrines held by the forester have been consistently violated on a very large scale, with the result that the forest capital of several important countries has now been very much reduced. In the majority of the places concerned the story has been very much the same. The forests, if not actually regarded as an incumbrance of the ground and a hindrance to the immediate development of the country for the purposes of agriculture, were considered to be a species of gold mine from which all the valuable products could be extracted and sold, none of the revenue so realised being put back in the form of maintenance and improvement. There were a few striking exceptions to this process, India being the most important, and Ceylon, Mauritius and New Zealand are among others which may be mentioned.

It is probable that the exploitation of forests, at any rate in the tropics, began with the practice of shifting cultivation. The practice has been widespread in many parts of the world up to the present day, and was prevalent among the Mayas of the ancient civilisation of Central America. Patches of forest are cut and burned and one or two crops are taken off the ground before the cultivator moves elsewhere to repeat his felling and burning. This destructive method of cultivation has undoubtedly been responsible for many changes, and usually deterioration, in tropical

forests, as softwoods and bamboos nearly always occupy such areas and take the place of more valuable timber trees, and it may take many years, even centuries, before the timber trees find an opportunity of retaking their rightful place. It is estimated that at the present time some 2,000 square miles are destroyed by this method annually in Nigeria alone. In Kenya and Ceylon large areas of forest have been destroyed, and in many other parts of the Empire the process still goes on.

The energy and ability of the cultivator have been taken advantage of to introduce plantations of useful trees among the agricultural crops, the method having been first introduced in Burma with teak about 1870. It has spread during the present century to India, Africa and elsewhere, and has attained very considerable importance as an effective method of introducing valuable hardwoods into areas which may have been devastated many years ago or may never have been of value.

In most parts of the Empire where valuable forests existed heavy and wasteful exploitation began as soon as a use was found for timber, and, without dwelling on the record of this depressing period, I shall try and trace briefly the history of conservation and control.

The story has often been the same. Startled by the destruction and waste of an important national asset, governments have usually pushed through legislation to enable them to make State reserves, to appoint officers of a Department of Forestry, and to control the excessive felling by traders and licensees. These laws, which to begin with were better than nothing, have usually been amended, improved and made stricter by subsequent enactments, and it is not difficult to realise that the early legislation was often very defective.

As a preliminary to the introduction of systematic management it was customary to employ a trained forest officer, usually from India, to report on the forests and the measures to be taken to preserve them, and many of the old reports then produced are of the greatest interest. These officers not only examined and reported on the forests in detail, but also made proposals for staff, reservation, afforestation and so on, and sometimes even drew up preliminary working plans.

In one case a Japanese was engaged to inspect the forests of a British possession, but I have not been able to find his report.

We must not forget the large extent to which forestry was influenced by botany during the early years, and much of the progress made was due to the efforts of distinguished botanists, both British and foreign, who were quick to see the damage that was being done to the forests and the opportunities that still existed of conserving and improving the timber resources. Several excellent reports on the forests of India and Burma were written by botanists, and it is curious to find that some of these have not yet been superseded by any modern work of equal merit. Mention should also be made of a number of army officers who showed a special aptitude for preliminary forest work and occupied important positions before the advent of trained forest officers. This is the place to mention two great Empire foresters, both Germans, Sir Dietrich Brandis and Sir William Schlich.

A hundred years ago the old forest rules were in force more or less in Great Britain, Mauritius and one or two other places, but for the most part the forests were free to the people, like air and sunlight, and were valued as lightly.

The Ceylon Forest Ordinance was passed in 1840, and regulations to protect certain forests in New Zealand were made in 1841. An old Act of 1859 in S. Africa is called the Forest and Herbage Act of Cape Colony. The Indian Forest Act was passed in 1865, marking the beginning of proper modern legislation to protect forests in the Empire.

Up to the year 1900 little progress was made in this direction in other parts of the Empire, but certain tentative efforts before that date were made in Cyprus, New Zealand, S. Africa, Great Britain, S. Australia, Ontario and elsewhere. During the last thirty years the development of measures of protection has been rapid. In almost every part of the Empire the State has made great efforts to obtain control of the remaining valuable forests which have been sometimes reserved, sometimes completely protected by an adequate staff, and sometimes controlled as regards felling by traders and permit-holders.

It has been the rule, rather than the exception, for these measures to arouse opposition, not only from the inhabitants of neighbouring villages, who naturally considered their vested interests were being interfered with, but also from more enlightened people, officials and others. It has often happened that the forest

departments were regarded as oppressors, the people and sometimes officials being quite unable to understand that the forests stood in any need of protection.

The good work, however, made great strides between 1900 and 1930 in Canada, Australia, Tasmania, New Zealand, the Malay States, West and East Africa, Borneo, Fiji, British Honduras, British Guiana, the West Indies, Palestine, Tanganyika and Cyprus. British Somaliland represents probably the most primitive country in the Empire as regards development of its natural resources. There is no Forest Department, and shifting cultivation has done a good deal of damage, but a start has been made in inducing the natives to plant trees, and certain areas of cedar have been preserved. In Kenya the area of forests is very small, only 6 per cent. of the inhabited area of the Colony, but the pencil cedar is a valued product, and scientific management of the forests is well in hand.

Ceylon is a colony where forest management has been in force for a long time, but it was noted at the Australian Conference in 1928 that 'up to quite recently our main work in Ceylon has been the exploitation of our hardwood forests for the supply of timber to public departments at cost price.' This unfortunate system had most undesirable results, as exploitation was looked upon as the main duty of the forest officer.

A census of woodlands of Great Britain has been completed, the total area being only 4,700 square miles, of which a good deal is classed as unprofitable. Great Britain, Spain, Greece and Australia seem to come well at the bottom of the list showing percentage of area under forest in each country, and Great Britain can at present supply only about 5 per cent. of her annual timber requirements. Small places such as Trinidad, Tobago, Dominica, Antigua and Hong Kong have been reported on, and a certain amount of interest is taken in their forest growth. In Hong Kong, in 1920, 8,000 pine plants were put out on bare hills, and there is a botanical and forestry service with two Europeans.

In 1920, 1923 and 1928 Empire Forestry Conferences were held in Great Britain, Canada and Australia respectively. These proved of the greatest value in co-ordinating forest work throughout the Empire, in bringing forest officers together, in encouraging the care of forests in every possible way, and perhaps greatest of all in the creation of a 'forest sense.' By 'forest sense' I mean a general feeling among the population that forests are an essential and natural part of the life of a nation, that the country cannot get on without them, any more than it can without air and light. Such ideas have existed for many years in some of the countries of Europe, notably Germany, France, Austria and Switzerland.

Among the important subjects dealt with at the conferences were :—

Survey of Resources.
Protection of Forests from Fire.
Forest Research.
Forest Education.

It will be evident that, in estimating the forest resources of the Empire, the figures must be in some cases only approximate. Indeed a number of countries have been unable to give any exact details. A useful beginning has been made, however, in estimating the areas under forest, both accessible and unprofitable, and the quantity of timber that may be made available. Details of this kind will become more valuable year by year as surveys and working plans cover more and more of the remote areas, and it will suffice to mention here that India has 230,000 square miles of forest, Canada nearly a million, Australia 38,000, and New Zealand 18,000.

The following resolution was adopted by the third Empire Forestry Conference held in Australia in 1928 :—

'The widespread damage to timber, property and life resulting from uncontrolled forest fires is a menace to the economic well-being of the British Empire, and constitutes the greatest single deterrent to the practice of forest management.'

This resolution had particular reference to Canada.

Forest research has made great strides in the Empire during the present century. The number of subjects covered is very large indeed, and includes comprehensive investigation into silviculture in all its branches, including forest management on modern lines, nursery and plantation work, the protection of hills and slopes against erosion, and the use of valuable exotics, for example conifers in Australia, New Zealand and S. Africa, and eucalyptus in S. India.

On the side of forest utilisation or forest economy great progress has been made

in the investigation of the uses of timber and minor forest products such as dyes, tans, oils, resins, lac and pulp from bamboos. Other useful lines of inquiry are seasoning of timber, antiseptic treatment of railway sleeper and other woods, the structure of timber and the qualities of timber as regards strength and elasticity.

Under the head of protection the most important items are the control of damage done by insects and fungi, damage which has attained great dimensions in many forests in tropical and temperate regions.

Forest research has been carried out principally in India, and also to a certain extent in Great Britain, the Malay States, Canada and elsewhere, and is steadily increasing in importance.

A most important development as regards research and education has taken place recently at Oxford, where the Imperial Forestry Institute has been established. Although badly hampered in its work by lack of funds, under the admirable guidance of Prof. Troup this Institute has already made great progress, and has attained a commanding position in the Empire as the centre of forest research, forest education and a means of disseminating technical information.

The training of forest officers of the higher grades for service in the Empire began in this country in 1885 at Cooper's Hill, and after 1900 it was taken up by several of the universities in Great Britain, Canada and elsewhere. A committee appointed by the Government went fully into the whole matter during the present year and expressed the opinion that four years were necessary to train a forest officer. The importance of maintaining a supply of properly trained experts cannot be overestimated.

To sum up, we may say that we have a forest estate of about 2 million square miles, that large parts of this have been grossly misused in the past, but that the prospect at the present day that the forests will now be properly managed is good and that an increasingly valuable asset will be assured for the Empire.

(2) Sir R. L. Robinson, O.B.E.—*Empire Forestry during the past Decade.*

(3) Mr. Fraser Story.—*The Empire Forests as a Resource.*

Prof. E. P. Stebbing.—*Afforestation Work in the Plateau Central of France.*

Tuesday, September 29.

Mr. R. Bourne.—*Site as the Basis of Ecological Survey and Forest Practice.*

Attention is to be concentrated on a basic truth, a more general recognition of which would help to remove many of the *primâ facie* problems of ecology and forestry from the realms of controversy.

There is evidence from all over the world that the degree to which *undisturbed natural vegetation, i.e.* virgin forest, reflects the habitat conditions, or site, varies region by region. In some regions, particularly in hot and dry and frequently in cold climates, a map of the dominant species or mixtures is to all intents and purposes a site map. The close association of site and natural vegetation is very marked, and is best illustrated by air photographs which picture the distribution of dominants and the well-defined boundaries of vegetative types, or sub-types.

In most regions under hot and constantly humid climatic conditions the climate is so favourable to the growth of many species that a map of dominant species or mixtures only coincides locally with a detailed site map. Quite marked differences in soils, aspects, &c., are of small significance in controlling the distribution of species. Apart from the general climatic type of a region, distinct vegetative types are clearly associated only with extreme edaphic conditions, either exceptionally shallow and dry soils, or swampy areas.

Within more mesophytic and milder climates seasonal variations generally render edaphic conditions of considerable significance in the distribution of species. In many regions a map of dominant species or mixtures closely resembles a detailed site map, but in others coincides only with site-groups, as in hot and humid climates.

Thus the general truth to be emphasised is the regional association of natural vegetative types or sub-types, with site-units or site-groups. If natural forces have

operated unchecked for long periods of time, the resultant vegetation is so obviously associated with habitat that the principles of ecological survey and classification and the problems of their application in forest practice are easily appreciated. On the other hand, if human intervention has modified or replaced natural vegetation, the issues are obscured to varying degrees.

In inhabited regions some of the most obvious differences in vegetation often bear little relation to site. The character of a forest may have been completely altered or the forest replaced by grassland and arable crops. However, in such areas, detailed investigations always reveal some qualitative or quantitative differences in vegetation according to site. The full significance of this fact cannot be appreciated apart from a recognition of the general truth. But, if the truth is accepted and due allowance is made for the successional development of vegetation, it will be realised that the distribution of unit-types or group-types, as the case may be, is always coincident with the extent of site-units or site-groups.

In conclusion, the practical problem of recognising unit or group-types may be defined. In uninhabited or sparsely populated regions the types can easily be distinguished by a direct survey of the vegetation. In developed regions a site survey is an essential preliminary. The historic record is rarely adequate for a direct solution of the problem, and therefore all investigations should be based on distinct sites. In all cases, both with natural or artificial vegetation, the degree to which unit-types can be differentiated depends upon the regional conditions. In some regions unit-types are clearly associated with site-units. In others practical considerations restrict survey to the recognition of group-types and site-groups.

(If time permits, one of the examples which have been worked out in recent years in developed country and which clearly demonstrates the general truth, will be quoted in illustration of principle and method.)

Mr. J. F. Annand, O.B.E.—*The Culbin Sands.*

The paper deals with the origin and composition of sand dunes on the north-east coast of Scotland, and particularly with the problem of the stabilisation and afforestation of 5,000 acres of mobile sand dunes known as Culbin Sands on the Morayshire seaboard. The various stages of the work of stabilisation are described and the part played by natural plant life in completing the fixation process is examined. The merits of various species of pine for commercial timber crops on the sands are compared. Illustrations by photographs and lantern slides.

Dr. M. L. Anderson.—*Preliminary Stages of an Investigation into various Races of European Larch.*

The Scandinavian theory of the existence of a Dunkeld race of European larch is examined. Points in the theory upon which further light is desirable are considered. Steps taken to investigate some of these points are described, and the paper deals particularly with an intensive nursery experiment, carried out simultaneously in eight nurseries with seed-lots of four different origins, one from the Tyrol, one from Silesia and two from Scotland. The difficulty of assessing differences in the early stages between plants derived from the four seed-lots is dealt with in some detail. The assessment is considered under the following heads: production of plants per pound of seed; size of plants produced; rate of germination; time of leaf-flush and defoliation; resistance to disease; other differences. Under the last head a novel means will be described, whereby subtle differences between races have been demonstrated. Plants grown in this experiment have been planted out in four forests, and will be the subject of further study, this paper being concerned only with the seedling stage.

Dr. A. S. Watt.—*British Forestry and the Philosophy of Action.*

Present practice in British forestry is considered in relation to past British and current Continental practice. Modern practice in this country is not entirely the outcome of native experience but, based on an experience proper to Central Europe, lies unconformably upon it. Modern British forestry is not an organic growth racy of the soil; our art is imitative instead of being creative.

The training in our universities is largely responsible for this acceptance of form and system without adequate consideration of the conditions of application. These conditions require scientific investigation, hitherto hampered by a too rigid and

narrow view of what a forest is, particularly in the fields of biology and soil science. The outlook is hopeful, but a speedy realisation of the need for a thoroughly scientific approach to forestry is urged.

Wednesday, September 30.

Mr. J. S. Corbett, O.B.E.—*Railways as Consumers of Timber.*

During the past ten years vast strides have been made in research and education in forestry matters. The demand for timbers of Empire origin has considerably increased, and the general public at last shows signs of taking an intelligent interest in forestry matters. This is all to the good, but have any steps been taken to improve the condition under which Empire timbers are marketed ? In this connection it may be worth while to consider the position as it affects some of the largest consumers of timber within the Empire.

For the purpose of this paper an industry which probably consumes more timber than any other undertaking is dealt with. Figures will be given showing the annual consumption and value of the different species of hardwood and softwood timber used by the various departments, together with the country of origin, and suggestions are offered showing how, with efficient organisation, the whole of the timber used might well be of Empire origin.

Mr. W. P. K. Findlay.—*The Effect of Progressive Fungal Decay on the Mechanical Strength of Timber.*

The general fact that fungal decay lowers the strength of timber is well known, but no definite information as to the amount of damage caused by different fungi at various stages of decay has been available. Experiments are described in which small, carefully selected, test pieces of Sitka spruce were exposed to the attack of *Trametes serialis* growing in pure culture under controlled conditions. At various periods after infection these were tested for strength, and then chemically analysed. The results showed that the wood loses strength very rapidly, and that this loss may be closely correlated with the chemical changes brought about by the fungus, which precede any loss in weight of the wood due to the respiration of the fungus.

Dr. R. N. Chrystal.—*Studies in the Biology and Forest Relations of the Pine Shoot Moth, Evetria (Ryacionia) buoliana, Schiff., by the late Clement Crayshaw Brooks, M.Sc.*

Brief history of the species with particular reference to its economic importance. The conditions in the pine areas of East Anglia. Life-history and habits of the insect. Types of damage caused by the caterpillar and their effect upon the host tree. Some factors regulating the economic damage of the insect. Control Measures : Natural control—The insect parasites—Artificial Control—The method of debudding. Some recent trials of the method and their probable significance. General conclusions as to the importance of the insect as a factor in the pine areas of East Anglia.

Mr. Alexander Howard.—*Our British-Grown Hardwood Trees* (*No.* 3).

The paper calls attention to early history of the building up of the woodlands of Great Britain and Ireland.

Reference is made to the attitude of successive Governments in penalising, by heavy taxation, those who ought rather to have been regarded as benefactors and patriots for having provided the country with the woodland beauty which has made England famous all over the world, and with the necessary reserve of timber which has stood them in good stead on so many occasions.

The paper continues to insist upon the absolutely vital necessity of some vigorous step being taken to replace the hardwood trees which are now being destroyed to the point of extermination.

Reference is made to the almost world-wide destruction of hardwood timber trees, together with the certainly world-wide failure in making any sufficient attempt to reproduce them, and refers to the satisfactory action in those few countries where scientific forestry is practised and real efforts are being made, and calls for the appointment of a Government Forestry Department based upon the best scientific plan which can be devised.

Pictures were shown on the screen of forest life and interest over a great many parts of the world

SECTION L.—EDUCATIONAL SCIENCE.

Thursday, September 24.

Presidential Address by Sir Charles Grant Robertson, C.V.O., on *Educational Development; 1831 and 1931; A Centenary Retrospect and a Forecast* (see page 215).

Dr. E. Deller.—*London as a Pioneer in University Education.*

For reasons which can only be dimly guessed, and into which it is not necessary now to inquire, there was no university in the capital city until the nineteenth century, and it was not until the end of that century that the University of London ceased to be a purely examining body and assumed the functions of teaching and research which are properly the concerns of a university.

It is desirable, therefore, not to limit this paper to the share of the university as a pioneer, but also to take into account the contributions of other bodies which were concerned with higher education before the creation of a 'teaching' university in 1898. Most of these have subsequently taken their places as parts of the university.

Among these are the medical schools where, before the days of 'scientific' medicine, important contributions were made to its advancement. University College, for example, which from the outset aspired to be a university (and was known for some time as 'London University') was based on principles—*e.g.* freedom from tests—which have been accepted as commonplaces in other institutions in London and the provinces. It was an innovator in other respects :—by the position it gave to pure and applied sciences and other subjects in its curriculum, the admission of women, &c. Bedford College was among the first of the institutions for higher education open to women. The Royal College of Science and the School of Mines (since incorporated in the Imperial College) were pioneers in the methods of teaching science and in the practical applications of science. The London School of Economics was an innovator in the social sciences.

The University came into existence in 1836, and by reason of the political compromise which led to its foundation its activities were restricted to examinations for degrees—'to perform all the functions of the Examiners in the Senate House of Cambridge.' Originally it was limited to students from University and King's Colleges, but the list of institutions presenting candidates was rapidly extended, and in due course candidates (other than candidates in medicine) were examined irrespective of their training. This function is still discharged on the 'external' side of the university.

There are limits to the usefulness of a university whose functions are confined to examining, but examinations when wisely planned and skilfully conducted can have a potent influence for good. The London examination system was in many respects 'pioneer.' From its beginnings the matriculation examination and the pass examinations have made considerable demands on candidates and exercised a great influence on schools and colleges. Many subjects were by this means first given university recognition. The possibility of obtaining degrees by 'all sorts and conditions of men' led to a new university class. The opening of degrees to women (1878) was a further step forward.

The 'examining' University of London acted as a foster-mother to colleges in the provinces until they had attained university status, and it still performs a similar office.

The pioneer work of the University since its reconstitution in 1898 has taken, necessarily, a different form. It has, by the institution of new degrees and diplomas, given recognition to new branches of study, but it has also, by the creation of new chairs and departments, itself marked out new lines of development. In Medicine, Science, Oriental and European languages and the Social Sciences, several new departures of potentially great significance have been initiated. In post-graduate studies, which are likely to be of increasing importance in London, in History, Public Health and Medicine, some notable progress has been made, and the creation of specialized post-graduate institutes seems likely to be one of the most fruitful methods of development in the future.

Discussion. (Sir Frank Heath, G.B.E., K.C.B.; Sir William Beveridge, K.C.B.; Sir Robert Blair; Sir John Gilbert, K.B.E.; Sir Philip Hartog, K.B.E., C.I.E.; Rev. Dr. Scott Lidgett.)

Report of the Committee on *General Science in Schools with special reference to Biology.* (Sir Percy Nunn; Mr. G. W. Olive.)

Friday, September 25.

The Establishment of a Central Institute for Imperial Education:—

Prof. F. Clarke.

The case for the Institution in London of a Central Organisation for the study of a vast and diversified range of educational activity. Cultural diversity of the Empire. The impression of a society at once British and foreign which an Englishman must form as a permanent citizen in the Dominions—the influence of custom, usage, speech, craft, politics and government. The necessity of grasping the *foreign-ness* as well as the *English-ness* of the Dominions. Autonomy of the Dominions—now being completed—will stimulate the cause of unity and necessitate closer and frequent consultation and appreciation of permanent common interests. Of the unity of the British Commonwealth of the future, consent will be the basis and community of faith the cement. Necessity for maintaining the British Empire adapted to meet the new world requirements.

The plan of organisation and work of a Central Institute to be available for systematic study of British and English methods and institutions of education—England a library almost uncatalogued. Overseas teachers to expound their own doctrines and work to students from all parts of the Empire.

Systematic investigation of the education of non-European peoples in the Empire. Educational influence not to be a one-way traffic. The winds of chance in English education may blow from any part of the Empire.

Effective co-operation with the universities and kindred library and collection of official publications.

The Analogy of Greece in the days of Plato and Aristotle.

Rt. Hon. Lord Eustace Percy, P.C., M.P.—*The Establishment of a Central Institute of Imperial Education.*

The British Empire is just awakening to the fact that it has a common tradition of education, and also a common duty in education. Its awakening is indicated by the existence (still in rather a hesitating form) of the Imperial Education Conference and by other signs, such as the interest recently manifested by the Canadian Universities in the extension of facilities for post-graduate study in the University of London for overseas students. The Imperial Education Conference of 1927 was, to me at least, a most impressive revelation of the reality of our common tradition and common duty, alike in the Dominions, in India, in our older colonies and in those dependencies in Tropical Africa and elsewhere where we are only just beginning to take in hand systematically the problem of native education.

With this awakening comes, as is right, a widespread questioning of our existing educational standards and methods in many directions. There is, I think, no part of the empire where those responsible for educational policy are not concerned, for instance, with two grave questions: the place of technical education in any national system of education, and the character of the examinations which so largely determine the character of the work done in the schools. And within the next five years I believe that we shall all be engaged in considering an even bigger question, now just forcing itself upon our consciousness: the whole question of university curricula and degree standards.

True, other nations are considering these things; yet there is a real frontier between education in the empire and education in other countries. For one thing, the English-

speaking nations missed that peculiar educational revival which, for most countries in Western Europe, has been associated during the last hundred years with 'the growth of nationalities'; nor have we been much affected by ideas of a uniform system of popular education which, in different forms, play so large a part in the educational policies of the United States, France, Germany, Italy and, we may add, Soviet Russia. More than any of our neighbours, we have relied on empiricism in education; and that is precisely the reason why it is so necessary for us to create some central institution where the results of our experiments can be constantly compared and from which the conclusions drawn from them can be disseminated.

As to the character of this central institution, two things seem to me to be clear. In the first place, it has urgent practical work to do. I confess to being a sceptic about 'research' in education, in the sense of recondite enquiries into method. But no one who has ever had any administrative responsibility for education can question our crying need for constant enquiry into educational policy. I have mentioned examinations and technical education. Add to these the whole question of textbooks, especially for non-European populations, in which we are far behind, for instance, France. Add also the whole question of the place of languages in higher education, in which the English-speaking world is at present frankly in a state of hopeless indecision.

And in the second place, such a policy-forming institution should emphatically be a University, not an official, body. It is a vital principle that educational policy should be formed in close association with actual teaching. A feature of our peculiar British tradition in education is the independence of our universities as towards our Ministries of Education, even where the university is a State institution. This has many advantages, but it has the disadvantage that our Universities tend to be unconscious of their responsibility for policy, while Departments of State struggle vainly to bring to the formation of policy the sound judgment which only teachers are really qualified to exercise.

Is there a more obvious place for such an Institute than the University of London?

Mr. F. H. C. Butler.—*International Aspects of the Work of a Central Institute for Imperial Education.*

The Chancellor of the University of London, in the course of his Presentation Day Address last May, said that he wondered whether, especially in these troubled times, universities could not, if they were asked, come to the help of the Commonwealth.

Perhaps in no more effective way could the Central University of the British Empire help the Commonwealth than by the formation of a Central Institute for Imperial Education. The complexity of modern civilisation has forced upon mankind problems that are capable of solution only by extensive intellectual co-operation and research; in these matters the community should naturally look to the universities for assistance.

In Great Britain, up to the present time, the direct relationship of the universities to the rest of the educational system has been almost entirely 'examinational.' The corrective of this narrow academic influence must be the placing of the study of educational practice on a broad imperial and international basis. The *comparative method* of science must be applied to education, and, thanks to the generosity of the Thomas Wall Trustees, this will be rendered possible in the Institute by the establishment of a Readership in Comparative Education. In addition to the post-graduate work in the training of teachers, it is desirable to establish a Department of Educational Administration, to train graduates intending to take up administrative posts in education either at home or overseas. It is essential that these students should be well versed in the study of foreign education and of education in Scotland and the British Dominions; special attention should be given to local and central administration, and these other educational systems should be compared with that of England and Wales. In the preparation of students for the Teachers' Diploma Examination a similar though briefer study of contemporary educational practice should be considered at least as important as a knowledge of the history of education.

It is assumed that the Institute will be organised in some such way as (1) a Department of the Theory and Practice of Teaching; (2) a Department of Educational Administration; (3) a Department of International Information. We wish, in particular, to press the claims of the last-mentioned unit, even if, at the outset, its inception has to be postponed. Not only would it be invaluable for the work and

activities of the Institute to possess a clearing-house for international information, but such a department might well prove to be of imperial, and possibly of international, significance. At present the authoritative centre in Great Britain for general information on educational matters is the *Special Inquiries and Reports Branch* of the Board of Education, which has performed a unique service to British education for over 25 years. It might be more economical in course of time for the Board to delegate the bulk of its general inquiries to the Institute, which could also perform a like service for the Colonial Governments and the more important Local Education Authorities. The establishment of ' contact-points ' in the various countries will be considered ; for instance in the case of U.S.A. there are the *Teachers' College of Columbia University* and the *U.S. Office of Education* (Department of the Interior) at Washington; in Germany we have the *Zentralinstitut für Erziehung und Unterricht.*

It is possible, however, that our Institute may be the pioneer of National Institutes of Education in the chief countries of the world, which would serve as mutual ' contact-points ' for the international exchange of educational information. The organisation and co-ordination of this work might well be carried out by the *Bureau International d'Education* at Geneva, which, it is to be hoped, will eventually become a department allied to the League of Nations, in a capacity somewhat analogous to that of the *Bureau International du Travail.*

DISCUSSION. (Major A. G. CHURCH, D.S.O., M.C., M.P. ; Dr. J. C. MAXWELL GARNETT, C.B.E. ; Sir PERCY NUNN ; Sir MICHAEL E. SADLER, K.C.S.I., C.B. ; Dr. M. P. WEST.)

Sir MICHAEL E. SADLER, K.C.S.I., C.B.

Students of education living in all parts of the British Commonwealth would value access to a British institute at which they could attend informative discussions and courses of lectures about the administrative arrangements, progress and problems of education in all parts of the British Empire and adjoining lands. The institute should be central. London is indicated as most suited for the purpose, because the institute would also be frequented by students of education from all European countries, from the United States, South America and the Far East. At present there is no European counterpart of the admirable and very influential work which is being done by Teachers' College, Columbia University, in the City of New York. The importance of education in contemporary politics and social movements is growing. An institute dealing not less systematically with the educational problems of the world (especially as they affect British policy) than the Institute for International Relations deals with other categories of public questions would render public service. Educational aspirations are part of the ferment of change in all countries. The working of the ferment should be watched by students of public affairs. But at present, so far as Great Britain is concerned, our study of the subject is too intermittent and occasional, lacking continuity of observation and record. There are so many common elements in the educational movement which is now sweeping over the world that administrators and teachers in every country can get guidance by informing themselves about the experience of other lands.

A few concrete examples of the subjects about which the institute would be a valuable source of sifted information are :—

(1) the progressive movement in secondary education which aims at securing liberal education for all ;

(2) the university problems which have arisen in the west and in the east through the rapid growth in numbers of students and the need for promoting social consolidation through appropriate courses of study not so specialised as to produce an unemployed academic proletariat ;

(3) the fuller articulation of the work of primary schools in rural districts to the needs of the community, *e.g.* the working of the Jeanes teachers in the southern States of the American Union and in Tropical Africa.

On all these points British experience can make a contribution which has not yet been effectively realised at home or overseas.

The institute, starting from modest beginnings, would not be a costly undertaking.

It would need a large house containing rooms for conferences, lectures, library and staff. On the selection of the latter, which might at first be small, success would depend. Though the institute would need the friendly but informal co-operation of many Government departments, including the Colonial Office, the Board of Education, the Scottish Education Department, the Foreign Office, the India Office and the Ministries of Agriculture and Health, it seems undesirable that it should be under the direct auspices of any British Government department. First, because no single Government department focuses all the activities with which the institute would necessarily be concerned, and secondly because discussions of policy in foreign countries would at times raise controversial issues from which Government departments should rightly stand aloof. It would be more appropriate that the work of the institute should be affiliated to a university, connection with which would attest scientific impartiality in the investigation and presentment of problems. To a university institution thus conducted, aid from the Government grant would be appropriate. Convenience of access, library facilities and the neighbourhood of sources of official information indicate London as the best seat for the Institute. As, however, the work of the institute would depend upon the friendly co-operation of local authorities in all parts of Britain and on the goodwill of British universities and schools, it might be found desirable to form an advisory council, upon which the chief bodies of experience could be represented.

Dr. M. P. West.

There is but one point which I would wish to add to what has been said already, and that is to emphasise the need of making the proposed institute as wide and international as possible in its character.

The problems with which it will have to deal cannot be localised in their scope, nor confined even to so large a unit as the British Empire. In studying a problem of the schools in Bengal some of our help came from the Philippines, from Pekin, and from studies made in America whose authors had no thought of our problems in their mind. And the outcome of our work has been of assistance to teachers in Canada and in Iceland whom we never intended to benefit. There is no one problem in education which does not tend to spread its network in this same way, taking help and (with good fortune) giving help in unexpected spaces and corners of the map.

If this institution becomes only a training ground of educators for the British Empire it will be useful. But if it becomes something more than this—a world clearing-house of educational ideals—it will be something of historical importance.

There has never been a time of greater educational unsettlement, of more varied experiment. The old ideals are crumbling away quicker than we can replace them.

There has never been a time when co-operation and co-ordination of effort was more necessary.

In Dr. Sandiford's article in the *Canadian Forum* some three years ago (which was perhaps one of the first expressions of the present scheme) there was mention of the work of Columbia University in training overseas educators. And in the later discussion one point which has been made in favour of this proposal has been that education in the British Empire may (owing to the work of Columbia University in this direction) be over-influenced by American educational ideals—and that this would be unfortunate.

It would be unfortunate if work of such magnitude and importance were allowed to be over-influenced by any one set of present-day educational theories. We cannot be so sure of the rightness of American methods and ideals as to allow them to predominate. *Nor yet can we be so sure of our own.* Nor, in an era so fluid and unsure, can we either trust blindly, nor yet leave out, any system—American or English, or other.

I would plead, therefore, that, in its connections, in its organisation and finance, the institute should be made as international as possible; that, so far from attempting to 'do it on our own,' we should invite—and go forth into the highways and seek—all possible outside aid.

If this institute is going to make educational history—as I believe its destiny to be—that history must be made for the world as a whole, and by as much as can be mustered of the whole world.

THE EDUCATION OF BACKWARD PEOPLES :—

Mr. S. RIVERS-SMITH, C.B.E.

We must decide what is worth while teaching and what is not, and whether our accepted standards of knowledge can be applied in the early stages of the evolution of a system of education suitable to a backward race.

The first essential would appear to be a resynthesis of knowledge, with its natural corollary of a generous reconsideration of the content of the syllabus and the fullest elasticity in its interpretation and application.

Knowledge to be of value must be vital. Our problem narrows down to a decision as to the criteria by which the knowledge of any particular group can best be measured, with the view of discovering natural aptitudes and the possibility of linking occupations and traditions with civilisation and progress.

An appreciation of the native point of view is not attainable in a moment and as the result of casual association during the routine of administrative work. The wide gulf between the social codes and the race prejudice with which the European is hedged about make the establishment of a working confidence a task of great difficulty.

A careful economic survey should precede the formulation of education policy, and anthropological and ethnological research should go hand in hand with educational development. Economic demand has to be met, and does not wait on the results of scientific research, but, just as trades need the aid of science in the development of industry, so must education look to the scientist to co-operate in the evolution of education for backward races.

African music and African crafts are worth preserving, but suffer neglect owing to the conviction that everything European must be better by virtue of the superiority of the ruling races. The mechanisation of modern life increases the difficulties of the educationist. Never before has a nation experienced such a violent sociological shock as that experienced by the races of Central Africa.

The same mental processes which produce urban immigration in industrialised countries operate in undeveloped Africa. The education problem is largely to protect the native against himself. The schools must harbour and refine native culture and native lore, and reflect all that is best in native social and artistic life. Educational aim must be the reorientation of outlook and the creation of confidence in the permanency and the capacity for development of native culture. The educationist must have confidence in the qualities of the African and his capacity to fit in to a more ordered society.

The task is beyond the capacity of the educationist unaided. He has not the leisure to conduct the research necessary to avoid the pitfalls which await the unimaginative and the uninformed. Close co-operation between the field worker and the teacher will clear the path of educational progress and free the future from the litter of the mistakes of the past.

Major H. A. HARMAN, D.S.O.—*Some Difficulties and Inconsistencies in the Education of the Backward Peoples of Africa.*

African education is full of dangers, difficulties and inconsistencies. In a fuller recognition of the value of the native teacher, and in framing all systems so that he is the channel to his people, lie the means of avoiding or overcoming these difficulties.

The material of education comprises first the so-called intelligentsia and their children ; second, the children at present in primary schools of all types ; and third, the masses of children and grown-ups in villages still untouched.

The motive for giving or requiring education is different in each group. So, too, is the form of education sought or given. Among the forms may be recognised : that subservient to a particular form of religion or necessary if adherents are to be kept and new converts obtained ; that based on the training of character ; the purely vocational, whether due to a transient or persistent need for clerks and artisans, or prompted by a desire to keep the people of an area engaged on its staple industry or occupation, or to give them a new and suitable occupation ; that aiming at the provision of future rulers from among the existing ruling class or of leaders from among all classes ; and, lastly, the aimless type.

Failure on the part of European personnel may usually be traced to disregarding the rate of possible progress, making the task undertaken too personal to the individual worker, working towards mistaken ideals.

In determining the course of training preliminary to the work abroad, this vital point is often disregarded: the African teacher is the obvious teacher of the African. In addition to the specialised training fitting the worker for some definite side of his task, his general training must include a study of methods appropriate to the education of the very young: a knowledge of general science sufficient for coping with problems to be met both as student and teacher; a knowledge of general linguistics; a general anthropological course if not already embraced by the foregoing.

Early difficulties encountered by the worker are due to the paucity of records of work already done and of material collected, and to the absence of policy on the part of controlling authorities. The first leads to much waste of labour, and the second to the framing of many and contradictory policies.

The cultivation of a sense of responsibility should be the main plank in his policy, coupled with a desire to help Africans towards the things which they feel they need.

How can our work be directed towards making the African more fully responsible for the formation of new schools? Might not rural education and all schemes for village schools be placed more directly under native control? How can we drive home the value of each grade of the more technical sides of training?

Other difficulties are connected with the conflict between vernaculars and English as the medium of education; the disappearance of native institutions under the influence of education; the multiplicity of authorities with varying interests responsible for the control of education; the passing of control to Government as the price of assistance in educational work.

Dr. A. R. Paterson.

Discussion. (Sir John Adamson, C.M.G.; Mr. C. W. Hobley[1]; Mr. A. Victor Murray; Right Hon. Lord Raglan[1]; The Hon. Hugh A. Wyndham.)

The Hon. Hugh A. Wyndham.

A group which has been organised by the Royal Institute of International Affairs to make a comparative study of education in Java, Indo-China, the Philippines, Malaya and the Japanese dependencies and mandated islands. This study was in every way suited to Chatham House owing to its close international implications. The first step in the inquiry would be to decide what tests should be applied in passing judgment on the education given, and the next would be to apply the tests comparatively having regard to the national characteristics of the five governing powers, the force of the impact of Westernism in each of the dependencies, the results achieved in the past and the influence of the Native on the imported culture. The co-operation of everyone interested would be welcomed.

Monday, September 28.

School Clinics and Child Guidance:—

Dr. G. A. Auden.—*The Difficult Child.*

Amongst the various forms of maladjustment in children one stands out especially on account of its frequency, *i.e.* that of the eldest child who finds himself displaced from his position as the centre of attention in the home by the advent of a second child. The trauma produced by this displacement often results in behaviour which causes much concern to the careful, and sometimes over-conscientious, parent.

The psychological basis of this and other maladjustments may be explained as follows:—

In the early months of infancy the child is essentially a 'self of enjoyment.' This individualist attitude is the outcome of his increasing apprehension of the contrast

[1] Representing Section H (Anthropology).

of a self and a non-self world around him. His reactions to the non-self world are essentially protopathic, but he is subjected to a ceaseless stream of suggestions of social import which demand an epicritic form of mental activity and involve the suppression of the phylogenetically older protopathic instinctive reactions. 'He becomes reasonable, and is no longer controlled by the pleasure principle, but follows the reality principle' (Freud). This pleasure-pain antithesis enunciated by Freud is in essence foreshadowed both by Plato and Aristotle. Out of his experience of living the child forms an ideal construction or 'Schema' of his individuality in relation to the non-self world. This Schema includes the external world of matter, space and motion, in which his body has place, and a world of social import, based upon epicritic data, in which other conscious beings have place. This Schema constitutes the 'Ego.' There thus arises a conflict between the two forms of reaction, and as the strength of an instinctive tendency is in proportion to its antiquity in the race, there is small wonder that victory very often goes to the protopathic individualist reaction in children. In later life the same victory is often achieved by Regression to a less integrated schema of a self of enjoyment still untrammelled by social demands and inhibitions.

The importance of this point of view lies in the fact that the Juvenile delinquent and the Difficult child are but overt expressions of something in the repressed Unconscious of every one of us, and that it is only by a projection and identification of ourselves with the child under consideration that we can understand the springs of conduct, however bizarre and inconvenient to the community these may be.

An analysis of the social circumstances of 200 cases of juvenile delinquency shows that 50 per cent. of these were associated with 'broken homes.' Further, the type of misdemeanour in the various age-groups throws a useful sidelight upon the psychological outlook which underlies them.

The formation of a schema of self can be represented diagrammatically. Various illustrative cases are quoted in the paper.

Dr. Wm. Moodie.—*Environmental Factors in Maladjustment.*

Dr. A. Macrae.—*Psychological Examination as an Aid to Vocational Adjustment.*

The aim of vocational guidance is prevention rather than cure. It is true that in the case of the maladjusted child the choice of an occupation may be an especially critical one, and that, in such a case, a wise choice of work may have some therapeutic value. But it is by no means *only* the 'problem' child who requires assistance in this matter. Vocational guidance is a service for the normal just as much as for the abnormal.

Vocational guidance is not a new thing. In the past much excellent work has been done by official organisations concerned with juvenile employment, as well as by many individual teachers. The psychologist does not despise these efforts, and, although he seeks to introduce more exact methods, he regards the results of his special examination not as a substitute for, but rather as a valuable supplement to, the observations of others.

The psychological examination includes a number of standardised tests. First, a test is used for the estimation of intelligence. Often the vocational misfit is not so much a square peg in a round hole as a little peg in a big hole, or a big peg attempting to fit into a hole that is far too small. There is little doubt that advice based on the results of an intelligence test alone would materially reduce the number of unfortunate vocational choices.

Tests for special capacities have, for the most part, not been so thoroughly authenticated as have the tests for general intelligence, but a number of such tests—particularly those which measure constructional and mechanical aptitudes—are widely used and have proved of distinct value.

In addition to estimating abilities, the vocational psychologist studies temperament and character. His judgments here are based partly on his own observations during the testing and during a systematically directed interview with the child, and partly on reports of parents and teachers, from whom he also obtains information regarding the child's personal and family history.

In the investigation of occupational requirements comparatively little scientific work has been done, and at present the fitting of the child to the job is not an exact operation. Probably vocational guidance will always be more of an art than a science.

The National Institute of Industrial Psychology has carried out a considerable amount of research on the subject, and is the recognised centre in this country for the practical application of the best existing technique. It is a field of work in which conclusive results are not easily or rapidly obtained, but such limited follow-up studies as have so far been possible at the Institute have yielded results of a distinctly encouraging kind.

Discussion. (Dr. C. W. Kimmins; Dr. C. L. C. Burns; Dr. R. G. Gordon; Mr. D. T. John; Dr. Emanuel Miller; Dr. J. R. Rees.)

Prof. R. Ruggles Gates, F.R.S.—*Eugenics in Education.*

Between education and eugenics there are many intimate interrelationships. The problems of eugenics can only arise consciously in an educated population, but it is an undoubted fact that many of the regulations and taboos enforced by primitive tribes are eugenic in their effects, although others may be clearly dysgenic.

In civilised modern society, conditions which are deleterious to the future welfare of the race arise partly through economic pressure and partly from the tendency of a highly civilised society to protect its weakest members against the rigours of a natural environment.

Since eugenics must deal with man as an organism and a species its problems, such as those of differential birth rate, marriage selection and the increase in the unfit, need for their successful study the co-operation of the biologist, anthropologist and sociologist.

An enlightened public opinion on eugenic questions requires some background of biological instruction in the mass of the people. Increased biological teaching in schools is necessary, to enable the next generation to visualise the problems of race and of heredity. It will be for biologists and educationists to see that this is supplemented by leadership in relation to eugenic questions of population, the production of children, racial crossing and similar matters. An elementary acquaintance with biology should be regarded as essential for all pupils in secondary schools. No one can have any real understanding of his own body and its functions without some knowledge of other organisms.

An informed public opinion alone can lead to adequate measures for dealing with our 300,000 mental defectives. Among 2,000 local authorities, only 29 have provided anything like adequate accommodation for them, yet expenditure on education of defective children in this country amounts to £93 per head, while on normal children it is only £12 per head. Already there is considerable wasted effort in the attempt to educate some of the children in elementary schools beyond their powers of absorption. Racial measures which will prevent the multiplication of defectives are obviously overdue.

Discussion. (Mr. C. Wicksteed Armstrong; Prof. Patrick Geddes; Prof. Julian S. Huxley; Prof. E. W. MacBride, F.R.S.; Sir J. Arthur Thomson.)

Prof. E. W. MacBride, F.R.S.—*The Teaching of Eugenics in Schools.*

The object of Eugenic propaganda is to drive home the importance of heredity. But the main points in the science of heredity have been embodied in ordinary folklore for ages and are enshrined in ordinary proverbs, *i.e.* 'What is bred in the bone will come out in the flesh.' It is common knowledge that parents of weak constitution have unhealthy children, and that clever children are born of clever parents; and there is a widespread belief in the inheritability of the tendency to consumption. It might be answered that the laws of Mendel ought to be taught. But an increasing number of naturalists view these laws with suspicion as statistical statements which fit only extreme cases. In any case, the attempt to dogmatise on such subjects leads to

opposition, as these so-called laws express a Calvinistic determination which is resented.

Far better would it be to include a knowledge of sex reproduction and the pressure of population on the means of subsistence in a simple course of natural history or animal biology which dealt with the habits and activities rather than with the structure of well-known animals.

Tuesday, September 29.

Report of Committee on *Educational and Documentary Films.* (Sir Richard Gregory, Bt.; Mr. J. L. Holland. See page 325.)

School Broadcasting (with Demonstration):—

Mr. Frank Roscoe.

Discussion. (Sir Walford Davies; Rt. Hon. Lord Eustace Percy, P.C., M.P.)

Questions. (Miss Mary Somerville.)

Prof. Winifred Cullis, C.B.E.—*Broadcasting in Adult Education.*

Broadcasting, through the matter sent out in the general programmes, must inevitably exert a powerful educational and informative influence. But, in addition to this general way of assisting education, the B.B.C. is experimenting in two special fields with the giving of more definite and formal educational instruction. The present paper refers to its work in the field of broadcast adult education, which is being steadily developed in the light of experience and of analysis of accumulated data.

Extensive developments in this direction date from the publication in 1928 of 'New Ventures in Broadcasting,' the Report of the Hadow Committee set up jointly by the B.B.C. and the British Institute of Adult Education. From the outset the work has been closely associated with the adult education movement. It has come to supplement not to usurp the activities of others. To secure the proper adjustment of functions, the Central Council for Broadcast Adult Education was set up in 1928. It is a Council nationally representative of the major interests in organised adult education in this country. Its present chairman is the Archbishop of York.

While development has been along lines of co-operation, the Council has also been concerned with exploring the new and distinctive features of a broadcast service which necessarily call for the elaboration of an independent technique and a study of the new audience. A survey of the work since 1928 concerns itself, therefore, with these two main problems: the interesting and getting together of this new and specialised audience and the development of a specialised programme technique.

The Problem of the Listening Public.—Here there were special conditions to be faced. An audience of isolated units lacking the cohesion and discipline of formal classes, and embracing immense varieties of type and of standards of intelligence. An inherent danger of passive and uncritical reception of facts and opinions. The prejudices and suspicions roused in the general public by anything bearing the doubtful hall mark of 'education.'

To secure the active interest and response of an intangible audience, it has been necessary to develop a mode of presentation which will itself evoke active response, and to support this by supplementary measures. These have included the preparation of associated literature. First, a Talks Programme to evoke interest in what is to come, secondly, special pamphlets in connection with special courses and, thirdly, the *Listener*.

Further, there has been the encouragement of the formation of study circles or discussion groups in connection with courses of broadcast talks. (The number of discussion groups formed during the winter of 1930–31 was well over a thousand.) Education officers have been appointed to various areas, and Area Councils have

been set up which, being locally representative of educational and cultural interests, can more easily secure the interest and co-operation of their particular localities.

Area Councils are now in existence in the West Midlands, in the North-West, in Yorkshire and in the West Country ; a local committee covering Dumfriesshire and Lanarkshire has been formed to supervise a special experiment made possible by the generosity of the Carnegie Trustees. A similar experiment in Kent has already been completed.

To increase the association of listeners with the activities of the Corporation and of the Council, a series of conferences have been held, confined mainly to those interested in the development of discussion groups. These were first held regionally, and this year, for the first time, nationally. This year, too, a Summer School was held at New College, Oxford, at which seventy students attended. In the Western area a register of listeners is to be compiled.

Programme Technique.—This has been elaborated in the light of the analysis of correspondence, of reports from education officers, and of resolutions carried at the local and national conferences of listeners.

The most important points of principle which have emerged are: necessity for great simplicity of treatment and informality of manner; choice of topics which have wide general interest (such are literature, economics, philosophy, psychology, and industrial and political problems), and a realist rather than an academic approach. Controversy is demanded on all sides and, within subjects, a continuity of treatment over as long a period as possible, on given nights of the week.

The new programme for the autumn and spring embodies the most ambitious attempt yet made to secure not only continuity of treatment, but also a unity of theme running throughout the programme which should give a cumulative effect to its educational aspect. Concentration upon live issues and upon the forces of transformation which have affected the lives as well as the ways of thought of all listeners, secures that relation to experience which appeals to the general audience.

Discussion and Questions. (Mr. C. A. Siepmann.)

Report of Committee on *Education for Life Overseas.* (Sir John Russell, F.R.S.; Miss Gladys Pott. See p. 291.)

SECTION M.—AGRICULTURE.

Thursday, September 24.

Science and the Production of Food Crops :—

Dr. E. S. Beaven.—*Plant Breeding and Biometrics.*

The breeding of modified races or forms of farm plants is one of the few hopeful methods of assisting the agricultural industry in its present almost desperate state of emergency.

Since 1910 (excluding two of the War years), systematically replicated cultures of Barley on a uniform plan have been carried on at Warminster in the hope of improving the practice of selecting cultures raised from single plants either of established races already in cultivation, or of artificially produced hybrids. A considerable mass of data has been accumulated representing countings, weighings and analyses of the produce of over three thousand plots of barley of many distinct races all chessboarded on the same plan—the majority from single plants of the F3 or F4 generation following cross-fertilisation. A sample of these results is now presented.

The problem attempted has been the discovery of intrinsic factors of productivity, and of the quality of the produce. These are necessarily different for each species of farm plant. For each species an economic problem arises and for this reason Biometrics are more useful than Mendelian Genetics. For instance, with Sugar Beet the problem would be to predict from some inherited and determinable morphological or physiological plant characters the best yield of sugar per acre under various cultural conditions. With different races of grasses and forage plants the

ultimate problem would be to discover the relative value of different inherited plant characters in terms of value of Milk or of Meat. It is fairly obvious that theories of chromosomal effects on these relative values are far too complex for determination. There are too many genes involved. We are reduced to empirical methods aided by statistical analysis.

The ultimate object of the cultivation of barley is the production of the maximum quantity of grain on unit area of the quality required either for feeding to animals, or for conversion into beer ; the latter being its more profitable use.

The breeder of any hybrid cereal requires to adopt methods which will give indications of value at an early stage in the multiplication of his individual plants. Nothing in the way of plant selection can be done until the F3 generation is reached and by this time there will be some hundreds of plants, any one of which may be a more desirable parent of a new race than any other. It is impossible to distinguish effectively between inherited and fluctuating differences due to environment at this stage. At least two thousand plants of any single plant culture must be raised and compared with a similar number of other single plant cultures before this distinction can begin to be established and comparisons made of genetic characters. To bring hundreds of cultures to the stage of field trials would require very extensive areas, expenditure and time.

A Table is exhibited showing for two races of barley ' factors of productivity ' in nine years. In each of these nine years eight races of barley were grown chequerboard-wise, with either sixteen or twenty replications for each race. The table shows for each year the means of determinations made on two of the eight races and the ratios of these values on the 164 pairs of plots. The two races selected for this sample of the available data are Spratt-Archer and Plumage-Archer, both of which are widely cultivated and together occupy more than half the total barley area of Great Britain.

Produce of grain on unit area is given sufficiently closely by $n \times T \times E$ when n is the number of plants, T the average number of tillers surviving to produce ears, and E the average weight per ear. The same yield (Y) is also given by $n \times P \times M$ when n is as before the number of plants, P the average weight of the entire plants, and M the ' Migration Co-efficient,' *i.e.* the ratio of weight of ears to weight of entire plant. This is therefore TE/P. It follows of course that PM=TE and is the mean weight of ears per plant. It also follows that the products of the ratios, PA/SA, for each determination give the ratio of the yields.

The data show that for these two races a prediction made in seven of the nine years from the factors T and M only, in the form PA/SA (TM−·03), would have given a somewhat better estimate of the mean relative yields for the nine years than is given by the actual yields in the same years. It cannot be claimed that this does more than point the way to further research for similar equations which might be of general application. It is, however, only to be expected that a result of this order should be obtained owing to the fact that under the climatic conditions of this country the critical periods for cereal crops are at the tillering and grain-forming stages. Differences of habit in races at these stages are well shown to be hereditary, whilst differences in plant population which greatly affect total produce are mainly due to incidental and environmental conditions. Moreover, both T and M can be determined relatively in any year on small plots with a much lower probable error than can gross yields.

Concerning quality of produce, Dr. L. R. Bishop, working at Rothamsted for the Institute of Brewing, has calculated from a great mass of data a prediction equation based mainly on nitrogen content of grain which gives with a very small probable error the amount of brewers' extract obtainable. Differences in nitrogen content are clearly shown to be hereditary and, applying Dr. Bishop's equation to the above prediction equation for relative yields, we get a prediction for the mean relative quantity per acre of matter available for brewing given by the two races when grown under parallel conditions for nine years. The validity of this prediction is confirmed by results obtained from trials of the same races made in 1925/6/7/8 by the National Institute of Agricultural Botany at five stations in England. There were ten replicated field plots of each race in nineteen trials, giving 190 pairs of plots. The produce of each of the nineteen trials was malted and the relative values of brewery extract was determined.

The final ratio obtained by combining the TM equation, given above, with Dr. Bishop's equation gives for the 164 pairs of Warminster plots PA/SA=·955. The

actual corresponding result obtained on the produce of the 190 pairs of N.I.A.B. field plots is, PA/SA = ·959, both in terms of available brewing material per acre.

The limitations of the conclusions to be derived are obvious and afford abundant matter for discussion.

Prof. R. G. Stapledon.—*Improvements by New Varieties.*

Recent years have seen great developments in the breeding and selection of herbage plants. In Denmark and Sweden the work is being pursued by public institutions and by private seed houses, and in Germany to a large extent by private individuals and Farmers' Associations. In this country the breeding of herbage plants is receiving attention at both Edinburgh and Aberystwyth, and at the hands of a few of the large seed houses. Selection work with European species which have proved their worth is in progress at the new station in New Zealand, and in South Africa Dr. Pole Evans is engaged upon critical studies of the species indigenous to South Africa.

The work in progress in all countries seems to point to one fact of outstanding importance, namely, that the most suitable material for breeding from is that indigenous or proper to the country for which it is desired to produce improved strains. Although, for example, it is well known that oats like Crown and Victory bred at Svalöf, in Sweden, have proved of outstanding value in this country, the indications are that even the best strains of rye-grass and cocksfoot bred at Svalöf are not really well suited to our conditions—not as well suited as, for example, Akaroa cocksfoot from New Zealand, or as strains built up from our own indigenous stocks. In this connection it is interesting to remark that the Akaroa cocksfoot in effect represents a selection from our own indigenous stocks, since 60–80 years ago—when the species was originally introduced into New Zealand—the seed of commerce was largely 'wild' seed and had not been reduced to any standardised type.

It is important to emphasise that indigenous means proper to a particular country, and that consequently it does not follow that pedigree indigenous strains *qua* pedigree indigenous strains have an outstanding value outside the range of territory for which they have been explicitly produced. There is a tendency in the trade to use the word 'indigenous' almost synonymously with the word 'wild'—no matter where 'wild' or where 'indigenous': this, if persisted in, can only lead to confusion and disappointment.

A striving in the direction of improving our herbage species is, then, world-wide, and in view, firstly, of the regional applicability of the products of improvement, and, secondly, of the difficulties connected with the proper seed production and adequate isolation of pedigree strains in the case of cross-fertile plants, a highly interesting and somewhat difficult situation with somewhat intricate international ramifications has occurred.

Every country will, of course, be wishful to extend the sale of its own productions over its own borders, and the word pedigree as such, when applied to seeds, undoubtedly gives a certain status and enhanced value to the seeds so categorised.

It has to be remembered that in the past, and that still to a large extent, the seed trade is conducted very much on an international basis—seed changing hands largely on the basis of sample and price, and without due regard to inherent suitability on the basis of strain.

There is, then, a real danger of this movement towards the production of pedigree strains of herbage plants defeating its own ends—a danger that can only be met by traders and farmers alike fully appreciating the inner significance of pedigree, and by judging every pedigree product on its merits for the particular purposes required.

Organised seed production and organised trials of the various introductions would seem to be an absolute necessity. It is interesting to remark that in Germany the tendency is in the direction of forming Farmers' Seed Growing Associations, and of encouraging farmers to grow their own seed under supervision—the idea rather being that every characteristic locality should as such be self-supplying in the matter of the pedigree strains of herbage species best suited to its particular needs.

This much in any event is certain, the handling of seed on sample only is an antiquated method of procedure, and the seed trade of the world will be driven by sheer necessity to adjust itself to making its large scale transactions on an entirely

new footing—a footing based on pedigree inspection of standing crops and a knowledge of the precise geographical and regional range of usefulness of the various and increasingly large number of commodities which it will handle.

Dr. E. M. Crowther.—*Improvements by Fertilisers.*

As the centenary of the British Association almost coincides with that of the artificial fertiliser industry, the opportunity is taken to examine the development and the present position of the use of fertilisers and the method adopted in studying some of the practical problems of the present day. The production of the principal raw materials—Chilean nitrate, rock phosphate, crude potash salts and ammonium sulphate—expanded at surprisingly constant relative growth rates, especially during the interval between the agricultural crisis of the late 'seventies and the war. It happens that the phenomenally rapid expansion of the synthetic nitrogen industry during the war and again since 1923 has raised the total world supply to about the level which would have been reached if the pre-war rates had continued unchecked. Although obvious economic factors limit the use of fertilisers in individual areas, it would appear that the steady progress in consumption shown over long intervals is limited ultimately by the diffusion of the results of investigation and research.

In the older agricultural countries there is a consistent movement towards increased proportions of nitrogen and potash, but this is balanced by the increased consumption of phosphoric acid in those newer countries which use little else.

The rates of fertiliser consumption per unit of cultivated land are still extremely low except in a few special areas of intensive agriculture or large-scale market gardening, as *e.g.* in South Lincolnshire, Holland and the Atlantic Coast of U.S.A. The proportions of N, P_2O_5, and K_2O vary widely, and appear to depend more on political factors and proximity to supplies than on the inherent requirements of crops and soils. Thus, the relative effects of nitrogen and potash on crop yields are similar in East Prussia and Denmark, but the former uses five times as much potash per unit of nitrogen as the latter. Similar discrepancies are probably common, and have led on the Continent to an enormous output of work on the estimation of fertiliser requirements. There is a great need for co-ordinated schemes of field experiments on commercial farms to analyse present practice and to show the way to the more efficient use of fertilisers. Modern developments in the design and technique of field experimentation make it possible to obtain reliable results with little disturbance of normal farm operations. Experiments on the use of standard fertilisers under varied conditions are more needed than a multiplication of trials of closely related materials under uniform conditions. Some of the more ambitious experiments now being undertaken at experiment stations are reviewed. These include the study of the relationship of yield to the amounts of added nutrient, the dependence of response to fertilisers on weather, residual values over a period of years, differential responses of different varieties to fertilisers, the relationship of fertiliser effects to cultural conditions, and the development of a suitable technique for grassland experimentation.

Dr. D. J. Hissink.—*Improvements by Land Reclamation.*

1. *Kwelders and sea-polders.*

By the recurrent tides muddy deposits are laid against the dikes of Holland until they are so high as not to be covered by the summer tides. The salts are then for the greater part washed out of the upper layer by the rain-water, so that a grass-flora makes its appearance. These grass-grown deposits are called ' kwelders ' (German : Groden) ; they are covered only by high tides, especially in winter. When the kwelder is high enough a dike is built to keep out the sea-water ; the kwelder becomes a polder.

The soil of a young sea-polder, especially if it contains a large amount of clay, is extremely fertile. It is highly permeable to water, and nearly all the salts are washed out of the upper 50 cm. by the rain in the first winter after building the dike, so that the young polder is immediately ready for cultivation.

2. *The enclosure of the Zuyder Zee.*

A dike, 30 km. long, is being built from the coast of the province of Noord-Holland, by way of the island of Wieringen, to the coast of the province of Friesland, and will probably be completed by 1932, the Zuyder Zee being transformed from that moment into a lake.

3. *The reclamation of part of the Zuyder Zee.*

In this lake four polders will be made (total 225,000 hectares), equal in area to 10 per cent. of the area now available for cultivation in Holland. One of these polders, the Wieringermeer polder (about 20,000 hectares) was already endiked at the end of 1929, the water being pumped out in 1930.

4. *Experimental Polder near Andijk.*

The soil of the new Zuyder Zee polders consists partly of sandy soils, partly of more or less clayey deposits, with little or no structure. The reclamation of the young Zuyder Zee clay polders is therefore quite a different matter from that of the young polders which were previously grass-grown kwelders. For the study of this problem an experimental polder of 40 hectares was therefore made in the Zuyder Zee, near Andijk, in 1926–27.

The soil of the greater part of this polder consists of a very clayey mass rich in calcium carbonate, with little humus and with 2·8 per cent. NaCl on dry matter. In 1927 the original muddy soil was very wet, containing an average of 172 gm. water per 100 gm. dry matter, without any structure, and being nearly impermeable to water.

In order to be converted into normal soil this muddy mass has first to dry to acquire structure and to become permeable for water. In our humid climate a permeable soil rapidly loses its salts. The muddy mass of 1927, on an average 50 cm. thick and coloured black by FeS, with 172 gm. water and 2·8 per cent. NaCl per 100 gm. dry matter (approximately 16 gm. NaCl per litre soil water), had changed in two years, from 1927 to 1929, to a grey coloured, fairly dry upper layer, 20 cm. thick, with 66 gm. water and only 0·2 per cent. NaCl per 100 gm. dry matter (approximately 3 gm. NaCl per litre soil water), and a layer, about 23 cm. thick, from 20–43 cm., still in parts black and fairly wet, with 116 gm. water and 1·3 per cent. NaCl per 100 gm. dry matter (approximately 11 gm. NaCl per litre soil water). Meanwhile the original sodium clay soil, or rather the sodium magnesium clay soil, has to be changed into a calcium clay soil. Whereas the clay substance of normal Dutch polder soils contains an average of 79 Ca, 13 Mg, 2 K and 6 Na per 100 milligram equivalents of exchangeable bases, the muddy mass of the Andijk Experimental Polder contained, in 1927, about 24 Ca, 49 Mg, 8 K and 19 Na per 100 milligram equivalents. With good aeration the calcium carbonate comes into solution as calcium bicarbonate, which transforms the Mg-Na-clay into calcium clay. As this process is reversible, it is particularly important that the sodium bicarbonate formed should be led off.

The main thing for the reclamation of the soil of the Andijk Experimental Polder was therefore a good drainage system. Very fortunately, the summers of the years 1928 and 1929 were very hot and dry, so that the soil dried beautifully, large fissures and non-capillary pores appearing, the originally structureless soil acquiring in consequence a very good structure and a high permeability for water.

5. *Further investigations in the Wieringermeer polder.*

To permit of a further study of the extremely important problem of the drainage of the new Zuyder Zee soils, a drainage experimental field was laid out in the Wieringermeer polder, near Kolhorn, about 30 hectares in extent, where various drainage systems (ditches and drains) are being tested. In addition to the chemical values, the alterations in various physical soil figures, such as permeability, air-capacity (according to Kopecky) and volume-weight of the soil, are being studied. Some of the results of these tests and investigations will, it is hoped, be shown to those who attend the meeting of the Sixth Commission of the International Society of Soil Science, to be held at Groningen at the end of July 1932. They will then at the same time be able to see for themselves to what extent the cultivation of part of the Wieringermeer polder has been successful.

Afternoon.

Science and the Production of Industrial Crops :—

Rubber :—

Mr. F. D. Ascoli.—*Survey of the Present Position.*

Rubber as a commercial product is obtained almost solely from the Hevea Brasiliensis, which is not likely to be ousted by rubber-bearing bushes such as Guayule (America and Russia) and Chondrilla (Russia).

The Hevea Brasiliensis was first cultivated some fifty years ago, and presented many difficult problems, the most important being the problem of extraction of the product, on which the superiority of the cultivated over the wild product depends, and the problem of evolving the best type of tree, on which must depend the predominance of European over native plantations.

Up to 1900 world's requirements (50,000 tons) were met by wild rubber. The plantation industry, derived from seed brought from Brazil in 1876, arose in the East, and became of importance from 1900 ; it outstripped the wild product by 1914, and now supplies 97½ per cent. of the world's requirements. Supplies of wild rubber have declined considerably, the decline being due to the economic advantages held by plantation rubber.

The plantation industry was started by Europeans, but its great profits early in the century resulted in the establishment of native plantations. So rapid has been the increase of the latter that they now supply half the world's rubber requirements, with a considerably greater potential share in the near future.

Native plantations are organised on a system quite different from the European, the native relying on large stands of undeveloped trees per acre. Conditions approximate to those of the tree in its jungle state, and this system may have definite scientific advantages. The total native area is about 4 million acres.

The European planter relies on a small stand of highly developed trees per acre, and he aims at the maximum yield per tree to reduce his overhead costs. The object of the scientist has been to increase the yield per tree, and he has substituted for an original yield of 300 lb. per acre yields of 1,200 lb., with the possibility of 2,000 lb. in the near future. He has done this on new areas by means mainly of bud-grafting and seed selection, while by soil conservation, &c., he has increased yields of old areas. The scientist has not generally been able to improve the quality of the product.

A contest is at present in progress between native and European ; the native advantage of low production costs is offset by heavy marketing costs ; the European is attempting to reduce his higher costs (mainly overhead) by increasing yield per acre. The result of this contest cannot yet be foretold.

World's production is considerably in excess of world's consumption, and after allowing for the various factors it is clear that productive capacity is excessive, and will increase heavily in the next few years. It is difficult to say if this excess will be absorbed or what the relative effect will be on European and native plantations.

It is difficult to adjust productive capacity to demand owing to the 7-year lag between planting and production, and the difficulty of arranging a planting programme is made more difficult by the fact that since its birth the industry has never been on a normal basis. It is certain that the wild product will never oust plantation rubber, but it is still doubtful whether the European or the native will win in the fight for predominance in plantation rubber.

Dr. B. J. Eaton (read by Dr. H. A. Tempany).—*Research on Rubber Production.*

Cotton :—

Sir James Currie, K.B.E., C.M.G.—*A Survey of the Present Position.*

Dr. W. L. Balls.—*Research on Cotton Production.*

Friday, September 25.

Presidential Address by Sir John Russell, F.R.S., on *The Changing Outlook in Agriculture.* (See page 231.)

Followed by Discussion on *The Application of Science to Present Problems of Empire Agriculture.* (Gen. the Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S., Sir Daniel Hall, K.C.B., F.R.S., and Mr. C. S. Orwin.)

Science and Animal Health :—

Sir Arnold Theiler, K.C.M.G.—*The Pathological Aspect of Mineral Deficiency in Cattle.*

At the Oxford Meeting, August 1926, a paper was presented by Dr. P. J. du Toit dealing with experiments on 'Minimum Mineral Requirement in cattle' carried out in Onderste Poort by Theiler, Green and du Toit. Striking results had been obtained by feeding young heifers with food deficient in phosphorus but adequate in calcium. A disease was produced resembling in all respects that known locally under the name of Stiffsickness, occurring in cattle on pasture poor in phosphates. On the other hand, heifers that had been fed on food poor in calcium but adequate in phosphorus did not show any clinical changes at all. It was concluded that for growth, the requirements in the case of phosphorus are higher than in the case of calcium. Some of the heifers used in these experiments were subsequently killed and the bones were collected for histological examination. The disease, Stiffsickness, was tentatively identified with that known in the European veterinary literature as Osteomalacia, but no proof could be given for the identity of the two. The present paper deals with the identification of the osseous lesions found in Stiffsickness by comparing them with those found in Rickets and Osteomalacia as described in men and higher animals. These two terms are used in pathology to signify identical changes : Rickets for the disease in adolescents and Osteomalacia in adults. From a pathological-anatomical point of view they include definite and typical lesions of the skeleton by which the disease can be identified, viz., a *superabundance of osteoid tissue*, which, when profusely produced in the epiphyseal lines of adolescents, interferes with the longitudinal growth of the bones, and gives rise to symptoms that are generally described as Rickets. The result of the investigation was, that in all cases where phosphorus was deficient, a marked degree of atrophy of the bony tissue was found, generally known as Osteoporosis, accompanied with a marked increase of osteoid tissue, corresponding more or less in amount with the degree of phosphorus deficiency in the food. The disease Stiffsickness can, therefore, be definitely identified with true Rickets and Osteomalacia. In the case of the heifers fed with calcium deficient food, the changes in the bones were but slight and amounted to a very moderate Osteoporosis, but there were nowhere indications of an increased osteoid formation. The atrophy in the bones of the phosphorus deficient heifers was very marked and more pronounced than that in human Rickets. Whilst Osteoporosis apparently does not always belong to the anatomical changes of Rickets in men, it was constant in our heifers. It has to be considered as a primary change in Rickets of growing cattle. It precedes the formation of osteoid tissue, which appears to be a secondary formation to compensate the loss of the bony tissue which becomes absorbed, the phosphorus being required elsewhere. Since the supply of phosphorus in the food is insufficient, the newly-formed osseous tissue must remain osteoid. In the case of calcium deficiency the atrophy was not so far advanced as to necessitate a compensation of the tissue removed, hence no osteoid. The fact that insufficiency of calcium in the food produced but a slight osteoporosis and did not give rise to clinical symptoms during life, has to be emphasised. It is, therefore, not justifiable to speak of Rickets and Osteomalacia in cattle as being caused by deficiency of lime, although theoretically such a possibility must be admitted. Accordingly, Rickets and Osteomalacia in pasture cattle are exclusively caused by extreme deficiency of phosphorus. The lack of activated ergosterol or vitamin D played no rôle in the causation of the disease in our cattle, since the heifers had been exposed all day long to the influence of the sun. The absence of light seems to be a factor necessary in the causation of artificial Rickets in rats and in that of children observed under natural conditions. It is evident that it is not in Rickets of pasture cattle. Rickets and Osteomalacia in pasture cattle must be considered as advanced stages of a true Aphosphorosis, by which term we understand a definite syndrome, typical of phosphorus deficiency : Osteophagia, Osteoporosis, Rickets (in young) and Osteomalacia (in adult animals).

Dr. J. B. Buxton.—*Tuberculosis.*

Dr. F. C. Minett.—*The Importance of Studying the Diseases of the Cow's Mammary Gland.*

The subject is considered from three points of view, viz., from the agricultural, public health and pathological aspects.

The Agricultural Aspect.—The diseases of the udder, which are included under the term 'mastitis,' owe much of their importance to their wide prevalence. The most common form of mastitis, caused by a certain variety of streptococcus, follows a chronic and insidious course. In the larger milking herds it is probably correct to say that on an average not less than 25 per cent. of the cows are affected with this form in at least one quarter of the udder, the disease being especially prevalent among the older cows. The main reason why the losses from this source are not more clearly recognised is because most cases of chronic streptococcus mastitis remain latent or occult for long periods, and only at times give rise to clinical disturbance. Nevertheless, there is usually slowly progressive destruction of the milk-secreting tissue, so that in the aggregate the reduction in milk yield must be heavy. In addition, the milk from diseased quarters is often altered in quality, although at the same time there may be no important alteration in its appearance. Such milk is unpalatable and may also be unfit for the manufacture of butter and cheese.

The effect upon the animal has also to be considered. Among the consequences of mastitis may be mentioned the depreciation in the value of the affected animals, the expense of maintenance and of treatment, and the excessive herd wastage. Moreover, certain forms of mastitis spread by contagion, so that the diseased animal is a source of danger to its companions.

The Public Health Aspect.—Apart from the question of specific milk-borne disease, *e.g.* tuberculosis, attempts at providing a pure milk supply have been chiefly concentrated in the past upon clean methods of drawing the milk and upon measures for reducing its subsequent contamination. From the public health standpoint there are several reasons why, except in the case of tuberculosis, comparatively little attention has been paid to the diseases of the producing animal. So far as mastitis is concerned, one of the main reasons for this neglect is that the streptococci, which are the common cause of this condition, are harmless when ingested by human beings. While opinion on this point may now be said to be fairly unanimous, milk containing such bacteria and the products of tissue reaction cannot be regarded as a desirable food, even when diluted with normal milk.

In several respects our knowledge of the importance of the animal factor in connection with the problem of a pure milk supply is looked upon as incomplete. A similar remark can be made in the case of the epidemiology of milk-borne outbreaks of scarlet fever and septic tonsilitis. With regard to the origin of these milk-borne diseases, the evidence on many occasions has pointed strongly to heavy and continued contamination of the milk, such as could only occur at its source. The suggestion that such outbreaks may originate through actual multiplication within the udder of pathogenic streptococci derived from human beings has been supported in a few instances by direct proof. More rarely there have been encountered cases of mastitis due to hæmolytic streptococci, indistinguishable from those which are recognised to be capable of causing disease in human beings. At present the significance of this observation is not clear, since as a rule there has been no history of illness among persons in contact with the cows.

The Pathological Aspect.—Bacteria, *e.g. Br. abortus*, streptococci, derived from other situations in the animal body, sometimes localise in the mammary tissue and produce disease. Thus, bacteriological studies, including in their scope the udder and its secretion, may contribute towards a clearer understanding of disease processes occurring in other parts of the body.

Reference is made to the possibility of using the mammary gland for investigating under natural conditions the properties of bacteria and the relations which exist between bacteria and living tissues.

Finally, an assurance that the gland is in a state of health may be regarded as essential for the scientific interpretation of the influence of various factors, such as heredity, breed and nutrition, upon the yield and composition of milk.

AFTERNOON.

SCIENCE AND THE UTILISATION OF ANIMAL PRODUCTS :—

Dr. STENHOUSE WILLIAMS.—*Milk and Milk Products.*

Dr. T. MORAN.—*Meat and Cold Storage.*

A brief review is given of the way in which research has assisted in the preservation of meat by means of low temperatures. In particular, attention is drawn to the recent establishment in the trade of the process of rapid freezing of meat and its transport and distribution in small joint or carton form.

The paper outlines the principle of the method, together with some of the problems still to be overcome in the freezing, transport and retailing.

Dr. S. G. BARKER.—*Wool.*

The attributes of wool may be classified under three headings, namely, dimensional, constitutional and structural. The interrelationship of these factors leads to a definition of the manufacturing ability and response to processing of the wool of a particular flock or breed. It is possible to control and alter the relationship of these factors one to another by careful breeding, and thus to produce wool for particular manufacturing purposes, of which the characteristics specially required therefore are most highly developed.

The economic consequences of production of a raw material for special manufacturing requirements must be fully realised. Methods are outlined and practical instances given of facts and figures showing the relationship of the attributes of the fibre to practical conditions both in agriculture and industry. Modern fibre analytical methods and the facts concerning the nature of the wool fibre ensuing from their employment are discussed, in the light of new uses for wool.

Monday, September 28.

SCIENCE AND ANIMAL PRODUCTION :—

Prof. J. A. S. WATSON.—*Breeding : The Application of the Empirical Method.*

Prof. F. A. E. CREW.—*Breeding : The Application of Genetics.*

Selection in the hands of the breeder has changed the breeds out of all recognition in a relatively small number of generations, in respect of conformation and the production of animal commodities. On the other hand, the same measure of selection has been unsuccessful when applied to the case of a single factor difference such as red coat colour in a black breed. The difference in effectiveness of selection would seem to be due to the fact that in the case of those qualities for which selection has been made, and in the instances in which it has been successful, dominance is relatively uncommon. If this is so, then the efficiency of selection will be determined by several conditions, *e.g.* the heterozygosity of the stock, and by the genetic complexity of the characters for which selection is made.

The purpose of all breeding operations is the production of a type that will flourish in a given environment and which will fulfil the requirements of its appointed destiny. There must be complete harmony between type, habitat and destiny. When destiny and habitat are fixed the type must be evolved through breeding. Most recent attempts at animal improvement have involved the importation of specimens of improved European breeds for the purpose of crossing with, or of grading up, a local stock. It is highly questionable, in the light of results of these attempts, that such a method possesses any advantage over the less favoured one of selection in a given direction and toward a desired type amongst the animals already existing in any particular locality.

Recent developments in our knowledge of the physiological factors affecting the processes of growth and of differentiation, and the sex and reproductive life of the

individual, make it probable that the genetical methods presently used for the purpose of securing improvement will give place to others which involve the administration of hormones or of physiologically equivalent synthetic chemical substances. Already the time of attainment of sexual maturity is more easily controlled by means of anterior pituitary than by selective breeding. The sire is no longer essential in lactation, and the problem of the scrub bull may thus be solved.

Dr. C. CROWTHER.—*Nutrition and the Pig.*

The steadily growing insistence of the consumer on smaller and leaner cuts of meat has in the case of the pig rendered virtually impossible the older methods of pig-raising in which the pig was cheaply but slowly grown to maturity and then subjected to an intensive fattening process. To secure the class of pig now mainly in demand for meat purposes, growth and fattening must proceed simultaneously, the animal being marketed long before it has attained maximum growth. Fat production may be regarded as relatively simple to control, and research therefore has tended to concentrate mainly upon the problem of securing quick growth. Attention has been directed in particular to the questions of protein requirements at various stages of growth and of the extent to which mineral and vitamin deficiencies are likely to assume practical importance.

The Cambridge measurements of the basal metabolism of the pig have now furnished the basis for more precise estimates of the energy requirements of the pig for different purposes.

Recent British and Scandinavian work are substantially in agreement in indicating as adequate a rather lower level of protein requirements than has hitherto been postulated. The only mineral deficiencies that commonly need be provided for are those of calcium and chlorine. The pig would appear to be relatively resistant to vitamin deficiencies, and only in extreme cases do these appear to give rise to trouble in farm practice. The nett economic result of the pig-feeding research of recent years has been a sensible reduction in costs of production as compared with former standard practice.

Mr. E. T. HALNAN.—*Nutrition and the Hen.*

The foods used for poultry production being similar to those used for pig production, the pig and the hen may be regarded as possible competitors in the farm economy. So far as the powers of digestibility are concerned, the hen appears as efficient as the pig for foods low in fibre. As the fibre in the food increases, the hen becomes less efficient than the pig, this lessened efficiency being associated with anatomical differences of the digestive tract and lack of bacterial digestion of fibre due to time relationships of the passage of food through the tract. Rapid egg production, associated with lack of food reserve in the body, emphasises the necessity of efficient nutrition, including protein and energy requirements, vitamin and mineral requirements, and regulation of the bulk of the food. Evidence is given showing the influence of food on yolk colour, the effect of protein on fat production, and the influence of sex on body composition. The relative economy of the fowl and the pig as food producers is discussed, and evidence is given to show that from the flesh standpoint the fowl is just as efficient as the pig, whereas, as an egg producer, the hen is much less efficient.

Tuesday, September 29.

ECONOMIC ASPECTS OF AGRICULTURE IN GREAT BRITAIN AND THE EMPIRE :—

Sir ALBERT E. HUMPHRIES.—*The Wheat Position and Outlook.*

Many years ago Joseph Chamberlain pointed out that if there be a demand for ten articles of one kind and eleven are produced, the price of the whole is reduced by an amount quite out of proportion to the surplus. This seems to be the case in respect of wheat. To some extent the price of wheat depends on the cost of other commodities for which the excess supply of wheat may be substituted. In this way the concurrent supplies and values of rye, rice, maize and barley affect the price of

wheat, and under some circumstances the value of silver is important. The depreciated exchange values of some currencies tend to minimise in some countries the ill effects of low prices, and may be delaying the reduction in supplies which normally would ensue.

If the position of wheat could be regarded as separate and distinct from that of other cereals and of commodities generally, it might be unnecessary to look beyond the influences of supply and demand, but, as matters stand, one is driven to take into account other considerations, including the quantity of gold or of credit based on gold, available for dealings in commodities. However, in the short time available, the author is obliged to confine his remarks to primary points affecting wheat. He gives a bird's-eye view of the statistical position. First he glances at the total world's acreage and production, but concentrates a mass of statistical data into a statement as to the exportable carry-over in the principal exporting countries at the end of each recent cereal year. He then takes each of these countries separately and in brief summaries indicates the position therein. This abstract is written in mid-July, and the paper will be read in late September. In the interval there may be great alterations in the statistical position, affecting actual and prospective supplies.

The ' World's Crop ' has increased in recent years from about 500,000,000 quarters to nearly 600,000,000 quarters per annum. The quantity of wheat and of flour stated as wheat entering into international commerce is about 110,000,000 quarters per annum. The exportable carry-over in the important exporting countries is likely to be, on August 1, between 40,000,000 and 50,000,000 quarters. The fact that very large quantities of wheat are in the hands of bodies who are not risking their own personal possessions in a venture and may be actuated by considerations other than commercial, is of itself the cause of great uncertainty in the minds of buyers, who apparently have reduced their holdings to small dimensions. If confidence could be re-established and stocks in users' hands reconstituted, the unwieldiness of surpluses would be greatly minimised.

The author examines alternative uses for wheat and in particular considers possibilities of increased consumption in countries using less than 4½ bushels of wheat per head of the population. If time permits he proposes to give the relative values, reduced to a common denominator basis, in recent years of British, Canadian, Australian, U.S.A., Argentine and Russian wheats compared with the values of rice. maize and grinding barley in this country.

At the present range of prices comparative costs of production hardly matter ; it is more a question of relative endurance on the part of producers, but the further question arises whether intensive mechanisation with diminished employment is preferable to the older methods of production with greater employment of labour. At present the cost of the wheat used forms substantially less than half of the price of bread, for the greater part of its cost represents either a practically constant or an increasing amount for dock charges, transport, milling, baking and the distribution of flour and bread, whatever the prices of bread may be. It follows that the return to the wheat-producing interests may be substantially improved without affecting materially the interests of the ultimate consumer of breadstuffs.

Sir William Haldane.—*The Meat Position and Outlook.*

Sir Daniel Hall, K.C.B., F.R.S.—*Farming Units.*

The size of the farming unit has always been measurable and to some extent determined by the ploughing unit, *i.e.*, how much work in the year could be accomplished by the mechanism in use at the time. In Domesday Book the land a man occupied is defined in teams of oxen ; down to recent times the economic farms were multiples of the area appropriate to a team of two or three horses, plus the appropriate grassland acreage. The unit of arable cultivation was therefore not less than 50 acres, and the continued diminution in the number of small farms in the English arable area during the nineteenth century reflected the economic pressure on acreages maladjusted to the unit of cultivation. Nowadays we are in a period when horse is being replaced by mechanical power and a larger unit becomes necessary. The development of tractors and power implements, like the combined harvester, is still too incomplete to allow one to dogmatise about what should be the new unit of cultivation which will give the new implements their maximum economic employment. In this

calculation consideration must be given to the accidented character of much of our farming land and its heavy soils. Most of the progress in implement design is taking place in America, where the task on the machine is lighter. Readjustment here waits on the adaptation of the new types of implement to British conditions; it is also held up by the necessity to recast our small-field system, a capital outlay for which farming at present prices cannot pay. Even if the minimum farm unit as dependent on the machine is determined, there still remains the problem of how many such units promises the maximum of efficiency. Rationalisation promises economies in buying and selling, more effective utilisation of labour by concentration on the task of the season, and the power to train and select for efficient management. Small-scale farming means that the land is controlled by an unselected slice of the population—good, bad and indifferent, with corresponding results. The drawback of rationalised farming lies in the difficulty of detailed supervision in a business which depends so much on personal attention, especially with live stock. This leads many people to deny the possibility of rationalised farming, and to hold by peasant farming as the only way of dealing with the land. Social and political considerations here enter, as in the consideration of the Russian experiment compared with the break-up of the great estates in other European countries.

Mr. C. S. Orwin.—*Specialisation in Farming.*

The suggestion underlying this paper is that much of the mixed farming practised in this country, more particularly in the districts predominantly arable, suffers under the handicap of a tradition which links up the maintenance of soil fertility with the practice of animal husbandry. Nearly every farmer regards the dung-cart as the foundation of sound arable farming practice, and few ever question the economy of rotations designed for an interlocking system of crop and livestock farming. The reason is plain enough, for these interdependent systems are as old as agriculture itself. Until quite modern times, the only fertiliser the farmer knew was animal manure, and although the primitive rotation was adapted from time to time, as new crops were introduced, the underlying idea was a cycle of crops, some for human consumption, some for animal consumption, the latter being returned to the land as manure to promote the growth of the former.

So firmly rooted was this idea that the introduction of artificial manures by the chemists of a hundred years ago, and subsequently, suggested no challenge to it. The mixed farming system was *farming*, and any deviation from it was not. To hardly anyone did the thought occur that chemical manures, available in greater and greater abundance and variety, might render the farmer virtually independent of the old natural fertilisers. Hardly anyone set himself to design new crop rotations in which there would be more saleable crops, by the elimination of those grown as stock feeds; to simplify cultivations by the release of the land from the sheepfold; to displace the dung-cart, with its trail of weed seeds; in fact, to reorganise the whole economy of crop and livestock husbandry, to reduce the costs of production.

Quite early in the history of artificial fertilisers, Sir John Lawes had demonstrated that they were sufficient in themselves to maintain the fertility of the soil. John Prout, of Sawbridgeworth, translated his work into successful commercial practice on the cold Hertfordshire clays; George Baylis, of Wyfield, did the same thing on the chalk and greensand soils of Berkshire. The rest of the farming community regarded the new manures merely as something to supplement the old ones, and persisted, often to their destruction, in the old mixed farming tradition.

To-day, the heavy cost of root and fodder crops has begun to shake the faith in the ‘ golden hoof ’; the old idea that what was lost on bullock-feeding was made in the corn crop, is giving place. But the basic principle remains, in general, as firmly rooted in the soil as ever, and in the mixed farming districts there is little indication of attempts to consider the extraction of the products of the soil by systems of farming designed to treat each product as a speciality, without any necessary dependence upon the other branches of the farmer's business.

It is the men who have become specialists, though probably unconsciously, who are most successful to-day. At home, the store stock breeders of the hills; the dairymen and the graziers of the valleys; the market gardeners, the fruit-growers, the poultry-keepers and the glasshouse men. On the Continent, the bacon and butter producers of Denmark. In the new countries, the meat producers; the specialists

in butter or cheese ; and, in normal times, the farmers engaged in wheat production, cotton growing, and the plantation industries.

In advocating the need for specialisation in the depressed mixed farming districts of England to-day, there is no suggestion that men should necessarily give up livestock farming, as did Prout and Baylis, to concentrate on corn and clover crops, nor that they should give up arable cultivation to concentrate on livestock products in one form or another, as so many have done during the last fifty years. Either course may be recommended to consideration in certain circumstances. In general, however, the implication of the specialisation theory is that farmers should decide what it is that their farms are best fitted to produce, having regard only to soil, climate, transport, market and other economic factors, and that they should then apply themselves to the production of these things in the cheapest possible way. It may be that the bare fallow should replace the fodder crops and that the sheep should be relegated to the grass land ; that the farmyard manure should be sold to the market gardener, and that artificials should do its work ; that bullock-feeding should be abandoned, and straw sold if a market can be found for it. Briefly, the farmer should disabuse his mind of the idea that the foundations of mixed farming as he has learnt them are fixed and immutable. On the contrary, he and his advisers should reconsider the economics of production in a new light—the light that agricultural science has shed upon his problems.

DEPARTMENT OF HORTICULTURE (M*).

Thursday, September 24.

DISCUSSION on *Plant Breeding* :—

Sir DANIEL HALL, K.C.B., F.R.S.—*Introductory.*

Mr. M. B. CRANE.—*Genetics in relation to Pomology.*

This paper gives an account of the investigations on cultivated fruits in progress at the John Innes Horticultural Institution. They are concerned with such problems as fertility and vigour and genetical analyses of various physiological and morphological characters.

Briefly, the investigations involve :—

1. Incompatibility in cherries, plums and apples ; its behaviour and inheritance. In the diploid cherries the results so far obtained are in accordance with the genetic interpretation of incompatibility based on a series of oppositional factors, as advanced by East and Mangelsdorf in *Nicotiana*, Lehmann in *Veronica* and Sirks in *Verbascum*. In the hexaploid plums further complications occur, and these appear to be directly due to the polysomic condition of the factors which determine incompatibility. From crosses between many of the diploid cherries it is now possible to predict how the resulting offspring will behave amongst themselves and with their parents with respect to incompatibility.

2. Generational sterility in *Prunus*, *Pyrus* and *Rubus*. A higher degree of fertility is required to give a productive yield in plums, cherries and raspberries than in apples and pears.

3. Morphological sterility, *i.e.* the abortion or suppression of sex organs. In the raspberry sex forms are the result of genetic differentiation. Many of our horticultural varieties, though themselves hermaphrodite, are heterozygous for sex, and in breeding experiments hermaphrodite, female, male and wholly sterile plants appear in mendelian proportions.

4. The breeding of the above-named fruits, also *Ribes* and *Fragaria*, in order to determine the genetic constitution of varieties, and the mode of inheritance of various characters.

In the diploid raspberry two factors are concerned with sex, two with colour and one with hairiness. *FM* is ☿, *F* ♀, *M* ♂ and *mf* neuter. The male condition is correlated with obtuse foliage and approximates to *R. idaeus obtusfolius* Willd. The segregation with respect to the male organs is not so sharply discontinuous as that of the female organs.

The colour of the spines is correlated with the colour of the fruits. Red- and tinged-spined forms have red fruits, green spined forms apricot or yellow fruits.

PT gives red spines and red fruits, *T* tinged spines and red fruits, *P* green spines and apricot fruits, and *pt* green spines and yellow fruits.

The origin of many of the polyploid fruits is involved and their genetic constitution is complex. Consequently, for whatever purpose they are raised, larger families are required in such fruits than in diploid forms.

5. Cytological studies. These investigations have shown that the principal varieties of our cultivated fruits are diploids or even-numbered polyploids, the latter in all probability of hybrid origin, for this appears to be a condition of their fertility. Where odd-numbered polyploids such as triploids and pentaploids are found, they are generally less fertile and of comparatively little value.

The condition of the apple group is exceptional; seed fertility is not so closely related to fruit-production, and 'triploids' are consequently of economic importance. The 'diploid' apples are, however, themselves of exceptional constitution; their gametic number of chromosomes is seventeen, but my colleagues C. D. Darlington and A. A. Moffett have concluded from the type of chromosome association they have observed that this number is the result of reduplication of an original set of seven in different proportions.

The results of these combined genetic and cytological studies have shown that polyploidy and hybridisation have played an important and progressive part in the evolution of many of our important fruits. The studies have also afforded information regarding taxonomic relationships and the practical possibilities of cross-breeding both with species and varieties.

Mr. W. J. C. Lawrence.—*The Genetics of Dahlia and its Bearing on Inheritance in Cultivated Plants.*

The garden dahlia (*Dahlia variabilis*) was introduced to Europe in 1789. Its subsequent rapid variation under cultivation is probably without parallel in regard to the diversity of colour and form produced.

As the result of morphological, genetical and cytological observations it has been possible to explain the reason of this wide variation and to construct a scheme showing the probable origin and constitution of *D. variabilis*. The value of these experiments is that they contribute to our knowledge of many important plants of similar constitution to *D. variabilis*, but not so amenable to experimental requirements.

With the exception of *D. variabilis* the species of *Dahlia* may be classified in two groups for flower colour: (i) ivory or magenta, (ii) yellow, orange or scarlet. *D. variabilis* unites both colour groups within itself.

Flower colour in Dahlia is the expression of two series of soluble pigments: (i) flavones, (ii) anthocyanins. The flavones form the ground colours (ivory and yellow) on which the anthocyanins are superposed. Pale anthocyanin on ivory gives magenta flower colour; on yellow, apricot. Deep anthocyanin on ivory gives purple; on yellow, scarlet.

The results of the breeding experiments are as follows: *D. variabilis* is self-incompatible, setting no seeds with its own pollen. Two independent factors, *I* and *Y*, govern the production of ivory and yellow flavone colours respectively. The inheritance of *Y* is tetrasomic, giving the characteristic ratios 5 : 1, 11 : 1 and 35 : 1. The inheritance of *I* is tetrasomic also, but dominance is incomplete.

Anthocyanin production is also governed by two independent factors, *A* and *B*. *B* produces deeper pigmentation than *A*. The inheritance of *A* and *B* is tetrasomic. *A* is cumulative in action.

Another factor or factors inhibit, cumulatively, the formation of yellow flavone, thus giving rise to the intermediate ground colours cream and primrose. Anthocyanin on these grounds gives intermediate flower colours such as crimson. The inheritance of the inhibitor is tetrasomic.

The profuse variation found in *D. variabilis*, therefore, is mainly due to :—

1. Self-incompatibility, which enforces cross-pollination, and thus maintains the heterozygosity of the species.

2. The occurrence of tetrasomic factors, giving a very large number of genotypic combinations.

3. The cumulative action of some of these factors, which further adds to the diversity of form.

Cytological observations indicate that *D. variabilis* is an octoploid species ($2n=64$). Five other species examined were found to be tetraploids ($2n=32$).

The combined data suggest that *D. variabilis* is a hybrid between two tetraploid species, one belonging to the ivory-magenta, and the other to the yellow-orange-scarlet flower colour group. In the descent of the tetraploid progenitors of *D. variabilis* from a diploid ancestral stock differentiation occurred, and, among other distinctions, gave rise to the flower-colour groups. Following hybridisation between members of these groups, doubling of the chromosome number occurred, giving the fertile octoploid species *D. variabilis*. The garden dahlia, therefore, combines the results of specific differentiation with a high degree of polyploidy, *i.e. qualitative* with *quantitative* differences.

Most seed-bearing polyploids are *hybrid*-polyploids with an origin and constitution more or less similar to that of *D. variabilis*. Since the majority of cultivated plants of economic importance are polyploids, it is desirable that we know their constitution and probable behaviour on breeding, and that appropriate methods are adopted for the breeding of these genetically complex types.

Miss C. O'Connor and Dr. Redcliffe N. Salaman.—*Recent Progress in Breeding Potato Varieties Resistant to Potato Blight* (Phytophthora infestans).

A rapid and fairly reliable method of testing the resistance of potato seedlings to Blight (Phytophthora infestans) has been developed, based on the method used by Müller in Germany.

The seedlings are grown in a closed glass case. It is shaded from direct sunlight, the relative humidity is kept at saturation and the temperature is controlled as far as possible. The seedlings are also sprayed with nicotine to prevent insects carrying virus diseases from living in the case.

To test their resistance to Blight, seedlings are sprayed almost daily with a suspension of P. infestans conidia germinating in water, an atomiser being used for spraying. The most susceptible seedlings are killed by the fungus five to six days after the first spraying. More resistant seedlings withstand this treatment for two or three weeks, and the most resistant or immune seedlings cannot be infected. All the batches of seedlings to be tested are sprayed at least twenty times before the survivors are removed from the case, and selections of desirable types are made from them. Later in the season, and in the following years, further tests are made on offsets and cuttings which are again put into the case.

Until 1928, no seedling derived from domestic stocks had been shown to be immune to Blight. Resistance that was claimed for some varieties such as Champion, has been shown to be purely physiological and due to late maturity. The senior author had found that the wild species, Solanum utile, was almost completely immune to Blight. He raised a large series of hybrids from the cross S. utile × Domestic variety. From these and certain back crosses with domestic varieties we have obtained a few highly resistant seedlings endowed with moderately good domestic characters. In the last three years we have succeeded in finding a few seedlings of purely domestic parentage which are also highly resistant to Blight. They have only occurred in certain families and then only about 1 in 500 seedlings.

The South American species S. antipovichi, Aya Papa, and Papa Amarilla have also been found to be practically immune to Blight. Seedlings of S. utile and S. antipovichi, selfed, are also 100 per cent. immune. So far, however, in the F_1 from crosses Resistant × Domestic, the great majority of the seedlings have been susceptible, and the evidence tends to show that resistance to P. infestans is a recessive genetic character. Unfortunately, bud-dropping, infertility and lack of pollen in potato flowers, make it very difficult to carry out this breeding work systematically.

Friday, September 25.

Discussion on *Vegetative Propagation* :—

Prof. J. H. Priestley, D.S.O.—*Polarity in Cutting Propagation.*

Isolated pieces of shoot or root of the flowering plant show very marked polarity in regeneration. This is particularly true of the Dicotyledon, in which the regenerative activities of the isolated piece seem to be closely connected with the cambial activity. When the cambial activity of the normal plant is studied, this is seen from its inception

to have a definite polarity in the sense that the development of cambial activity is always in a basipetal direction from the base of the lateral members of the developing shoot.

The basipetal differentiation of new tissues from the cambium, particularly of the phloem, would seem to be closely connected with the movement of food substances in the same basipetal direction and would also seem to be largely responsible for the phenomena of polarity observed in cutting propagation. This standpoint seems to be supported by observations upon cuttings of Coleus in which, when the original vascular strands from the leaves upon the cutting have been severed, basipetal differentiation of new 'commissural' vascular strands has followed.

Mr. L. B. Stewart.—*The Propagation of Leaves, Monocotyledons and Dicotyledons.*

In Gesnerads the propagation of leaves is very simple. In Gloxinia and Gesneria the first thing that takes place is the formation of tubers, these being the stem of the plant, any offsets which they may send on afterwards being yearly growths. These tubers give buds and make an annual root growth. When the plants grow older the tubers attain some considerable size and give a free condition of flowering.

In Alloplectus there is no tuber formation, and a mass of roots is formed at the base, and the stem is thrown up into the air. In many of these plants the amount of tissue in the leaf controls the offspring. By using a whole leaf one gets nearer an adult plant than if one severs the main veins when juveniles are formed, but in Alloplectus, although one takes a whole leaf, juvenile leaves always form at the base of the bud-growth first.

In the Melastomads, which are plants which have pleurocostate venation, if a large number of plants are wanted it is necessary to sever the main veins.

In Phyllagathis rotundifolia, where the leaf is much dissected, the young plants have exactly the same kind of leaf as the seedlings, *i.e.* very hairy. At the same time they take a considerable period before they drift into the adult stage. Where juvenile forms are got on leaf cuttings, sometimes the stage of transition is lengthy, but this depends entirely on the height of the plants and the manner of their growth. Plants of the Melastomads are interesting in their root formation, having a very red root tip which is enclosed in a globe of mucilage. One seldom finds that young plants with juvenile characteristics flower well until they reach the adult stage.

In many of the Acanthads such as Sanchezia nobilis, Ruellia and Acanthus, the food material in the leaves descends very quickly to the cut surface, and they are exceedingly easy to root. Bud formation of some of these leaves is rather marked. Many buds may be formed and one or other of the buds take up the dominant factor and the rest of the buds are left in a recessive state until such time as an accident may occur to the growing point when one or other of the buds will then take up the dominant position again.

In Begonias and some of these other plants, it is not necessary to keep entirely to the blade of the leaf for their propagation. Some Begonias which have a considerable leaf stalk, can be propagated easily from the leaf stalk alone.

The propagation of the Rose, although not a commercial proposition, can be done easily by leaves. Although rooting takes place in a very short time, if the leaf has not made buds and formed its growth before the winter comes on, there is considerable difficulty in carrying them right through the winter to the following spring, because at that particular time the activity of the roots is low.

Many of the Legumes can easily be propagated from the leaf, more especially some of the annuals and herbaceous plants. In Brownea, which plant can be rooted easily, three or four years may elapse before the root stalk gets sufficient nourishment instilled into itself and is able to make bud formation. After the buds are formed, the young growth shoots up into the air and for a portion above the soil makes only scale leaves before the formation of ordinary foliage leaves.

Dicotyledonous plants which have thick fleshy leaves are easy to propagate, because they have a liberal supply of food material at their disposal, *e.g.* Sedum, Crassula and Bryophyllum.

Monocotyledonous plants can be increased very rapidly by leaf propagation. By severing the leaf of Sanseveria, which is of economic value, into several portions, each part of the leaf gives off a plant, and sometimes many plants.

In some of the Aroids such as Zamioculcas Loddigesii, the pinnæ, when propagated, produce a tuber. This tuber may be looked upon as a proto-embryo, as the tuber after it is formed, if laid on its side, seems to have the power of developing a bud from any given point. The first growths that take place from Zamioculcas pinnæ are very juvenile in form, and it may take two or three years to produce the adult leaves.

In the scale leaves of Hæmanthus, buds can be formed on both the back of the scale and on the inside of the scale, but this must not be taken as a general statement where scale leaves are used for the propagation of certain genera.

Bowiea volubilis, whose scales are very thick and fleshy, shows bud formation taking place very quickly, but where you find plants such as you have in Ornithogalum, the natural reproduction of the scale leaf and the artificial reproduction of the young plants are entirely different. In Ornithogalum the young plants that are formed in natural reproduction crawl up the back of the scale, being fed with a strand from the base of the leaf. But when artificial propagation is resorted to, the buds are formed at the basal end of the scale on the inside of the scale and have an entirely different outward look. This is due to the fact that in the natural reproduction there is a tremendous pressure on the young plant which keeps it in a cylindrical form, whereas when artificial reproduction is resorted to the plants are very pointed and thin, and more numerous.

In Drimia the scale leaf has a thick tapering formation. In the younger scale leaf the food material is stored between the base of the scale and the centre of the scale, but as the scale leaf ages the food material seems to be all forced up to the apex of the scale. Buds do not seem to have any special place to arise from, but it seems that the buds are formed where there is a collection of food material.

In nearly all scale leaf propagation it will be found that the young plants given off from the scale leaf come to maturity very much more quickly than those raised from seed.

In some of the Coniferæ we have plants that may be easily rooted, and here again we, in Edinburgh, are working with a system which will never be of much commercial service, but which gives an idea of the possibilities of raising these plants by leaves, *e.g.* Ginkgo, Podocarpus and Taxodium.

The ordinary Daisy which occurs throughout our land is one of the most productive plants that can be found. If one sows a hundred seeds one gets a hundred seedlings. If the Daisy leaf is chopped up into twenty-five or thirty pieces, each about one-sixteenth of an inch in width, it will be found that a plant or plants are formed at the midrib of each portion of the Daisy leaf, and this is how, through the distribution of cut leaves by mowers, etc., so many of these plants are found on lawns.

Dr. T. Swarbrick.—*Methods of Fruit Tree Propagation, with special reference to Rootstock and Scion Relationships.*

Mr. R. G. Hatton.—*The influence of Vegetatively Raised Rootstocks upon the Fruit Tree, with special reference to the parts played by the Stem and Root Portions in affecting the Scion.*

Exact information is now available which proves that clonal races of vegetative rootstocks influence the scion significantly, at least in the first twelve years, the amount of total wood growth, the habit of growth, the height and spread of the tree, the stem girth, earliness and nature of blossom bud formation, time of blossoming, percentage of flowers setting, actual number of fruits borne, size, colour and quality of fruit, degree of susceptibility to diseases such as Scab and Mildew, and nutritional differences as exhibited by leaf scorch symptoms.

All these characteristics go to make up 'rootstock influence,' which cannot be detected merely by measuring the growths of one- or two-year old trees. When such trees on clonal roots vary in size, they still behave similarly in all other aspects of 'stock effect.' Although no single term such as 'vigorous' or 'semi-dwarfing' is adequate, vegetative stocks have fallen readily into groups with certain common characteristics.

There is evidence to show that part of this influence is in the stem piece of the rootstock, the path by which the materials, collected by the roots, pass to the scion. This has been proved by the use of pieces of stock stems used as 'intermediates' between roots and ultimate scion. Workers in America studying the effect of scions grafted direct upon seedling roots concluded that stock influence lay in the stem portion.

Evidence is now presented to show that stock influence is also significantly apparent upon two varieties of scion worked directly upon clonal vegetative piece roots in two repeated experiments, the trees being three and four years old respectively. The influence is apparent in type of growth, early fruit bud formation, percentage of blossom setting, amount and quality of fruit, and in maiden growth and weight of these trees lifted at two years old. When examined at that age there were no significant differences caused by scion effect on vegetative piece roots.

It is shown that, at least up to eight years old, a series of comparable trees of four varieties, growing upon their own roots and worked upon vigorous and semi-dwarfing stocks, the latter profoundly modify for the fruit growers' benefit the behaviour of the tree on its own roots. In addition, it is pointed out that in this country, the obtaining of satisfactory trees on their own roots or bench grafted on seedling roots has never been a practical proposition.

Examples of the practical value of vegetative rootstocks to the fruit grower are given, and further evidence is produced which shows that stem worked trees are less variable when worked on clonal stocks than when worked on seedlings.

The influence of vegetative rootstocks has not decreased with years, though the relationship between growth and fruiting and the interrelationship of trees on one stock and another have, in some cases, as anticipated, been qualified by time.

Monday, September 28.

Discussion on *Glasshouse Problems* :—

Dr. W. F. Bewley.—*Some Factors which affect the Health of the Tomato under Commercial Conditions.*

(1) The total crop produced in any year varies directly with the total number of hours of bright sunlight between April 1 and September 30.

(2) Potash and Nitrogen requirements also vary in accordance with the amount of prevailing sunlight.

(3) Potash is the most important fertiliser and affects the quality of the fruit, resistance to certain diseases and power to withstand sudden harmful changes in the environment.

(4) The timely application of Nitrogen and Potash is most important—the plant requiring most potash in the early stages of growth and increasing nitrogen as the plant ages.

(5) Correct air and soil temperatures are important. A uniform night temperature is necessary for maximum crop. Raising the soil temperature by artificial devices has given promising results.

(6) Mosaic disease reduces the crop yield on many commercial nurseries. It can be controlled largely by the use of clean seed.

Dr. O. Owen.—*Carbon Dioxide in relation to Tomato Crops.*

Carbon dioxide in glasshouses under ordinary cultural conditions is irregularly distributed throughout the whole atmosphere. The average concentration falls to a minimum between 1.30 p.m. and 2.30 p.m. and rises to a maximum at about 5.30 a.m.

When the concentration of carbon dioxide is increased artificially, the gas may be distributed in ascending layers of decreasing concentration, but such a distribution persists for a very short time only.

The introduction of excess carbon dioxide into a house is followed by a heavy loss of the gas, despite careful attempts to prevent leakage. This loss commences

immediately and is fairly rapid ; for example, a concentration of 0·33 per cent. fell to a third of this value in fifteen minutes. The rate of loss follows a comparatively smooth curve and appears to be unaffected by the presence of soil.

A slow, steady evolution of carbon dioxide at the soil surface is almost without effect on the concentration in the atmosphere of the house in the daytime, and has but little effect on the crop.

To determine the effect of increased concentrations of carbon dioxide on crops the gas was generated from sodium bicarbonate in the earlier experiments and, later, from portable stoves burning patent fuel. Using the bicarbonate method crop increases ranging from 19 per cent. to 25 per cent. were obtained when tomato plants were treated with 20 to 30 times the normal concentration for two and a half hours daily. With the stoves, increases of the order of 20 per cent. resulted when the plants were subjected to a concentration of six times normal for one and a half hours twice daily.

Two types of plant designed to supply carbon dioxide on a large scale proved unsatisfactory.

Mr. B. D. Bolas.—*Carbon Dioxide in relation to Glasshouse Crops.*

Mr. N. L. Hudson.—*Heating of Glasshouses by Oil Fuel.*

Oil Fuel.—The oil fuel used is the resultant product after removal from the crude of the fractions which are volatile at ordinary temperatures. The Flash Point of the fuel oil commercially marketed in this country is usually about 200° F., which is well above the standard demanded by the Admiralty specification and by Lloyd's. The high Flash Point and low Setting Point ensure safety and ease of handling under all climatic conditions.

The calorific value is about 19,000 heat units a lb., the ash content about ·01 per cent., and the S.G. about 0·9.

Oil Burners.—The functions of these are to break up the oil into minute particles and mix with the proper proportion of air.

Types may be classified into four groups :—

(1) Steam Jet burners. An old type, the oil being usually fed by gravity to the burners, the atomisation medium being steam from the boilers themselves.

(2) Hand controlled Mechanical Atomisation burners. Suitable for nearly all big steam boilers such as the water tube, marine and Lancashire types.

(3) Semi-automatic burners. A class designed for those small boilers, for which hand-controlled plants are unsuitable. The installation generally consists of a storage tank, a pump for pumping the oil from the tank to the burner, a system of filters and an electric motor for driving the pump and the fan which supplies air for atomisation and combustion.

(4) Completely Automatic burners. Particularly suitable for central heating and hot water supply boilers, and owing to their completely automatic action they are particularly suitable for heating glasshouses. The burner is usually a self-contained unit and is self-starting, self-igniting, self-extinguishing and self-stopping.

Delivery.—This is by tanker, barge, rail, road, or in barrels.

Advantages of Oil Fuel.—(1) Absolute control of temperature by thermostat. This facilitates early marketing and ensures better prices, and, in addition, increases not only quantity but also quality of glasshouse products. (2) Economy is achieved by greater boiler efficiency and by reducing labour costs. It is noted that, although labour in Holland is cheaper than in England, the heating plant is generally inferior.

Recent Developments.—Considerable developments are taking place in Scotland in oil heating for tomato growing.

Research.—Experiments are now in progress at the Experimental and Research Station, Cheshunt, and at the West of Scotland Agricultural College, Auchincruive, and results although incomplete are so far favourable.

Conclusion.—It is considered that ultimate success will rest with those firms who apply scientific methods and apply modern equipment in their works. Heating with oil fuel offers to the British glasshouse grower increased output, reduced costs and the means of successfully combating foreign competition.

Tuesday, September 29.

DISCUSSION on *Fruit Storage Problems* :—

Dr. F. KIDD, Mrs. ONSLOW and Dr. C. WEST.—*Physiological and Biochemical Aspects of Senescence in the Apple.*

Dr. T. WALLACE.—*Factors influencing the Storage Qualities of Fruits.*

In considering problems relating to the storage qualities of fruits, three main points call for attention, viz., susceptibility of the fruits to rots, the length of the period of senescence under natural storage conditions, and the effects of special conditions imposed by artificial storage conditions of cold stores, gas stores, &c.

The factors which predestine storage qualities of fruits may be grouped as follows :

(*a*) Materials—including classes of fruits, varieties, rootstocks, age of trees.

(*b*) Environmental Factors embracing

i. Natural conditions, such as climatic factors ; soil factors ; incidence of disease organisms.

ii. Artificial factors, *i.e.* factors introduced by the fruit grower and merchant.

The storage qualities of any sample of fruit are the resultant of these factors, but certain of the factors exercise dominant influences which enable storage behaviour to be foretold with some degree of accuracy.

Various effects which have resulted from the undermentioned factors in storage tests carried out on numerous varieties of apples in an ordinary temperature store and in a cold store are described.

Rootstocks ; age of tree ; deficiencies of nitrogen and potassium ; cultural systems ; pruning systems ; fruit thinning ; bark ringing ; time of picking ; size grading of fruits.

The interrelationships between certain of these factors have also been established.

The effects of the factors on the contents of certain ingredients of the fruits—total nitrogen, acidity, reducing sugars, cane sugar, total ash and ash constituents have been determined and attempts made to correlate these with storage behaviour.

Storage qualities do not show any simple relationship to the content of any of these constituents.

Dr. A. S. HORNE.—*Rotting in Fruit Storage.*

The two main factors influencing the occurrence and amount of wastage are infection and resistance to infection and invasion.

Infection is of primary importance. In the case of orchards where fungal numbers are exceptionally high, apples from such orchards may show high mortality even when the fruit is moderately resistant. On the other hand, low fungal numbers and few pathogenic forms are often associated with low mortality in store, although the apples may be very susceptible to attack.

Resistance to invasion is mainly conditioned by the following chemical factors : nitrogen content, acidity, total sugar content, proportions of the three sugars (glucose, fructose and sucrose) found in the fruit, and possibly potash content. Thus, low nitrogen, high acidity, high sugar content and high fructose or sucrose tend, in general, to increase resistance.

The chemical composition of the fruit and consequently resistance may be modified by stock, manurial treatment and by ringing the tree. Thus, the application of sulphate of ammonia is found to increase the nitrogen content of the fruit and to lower resistance.

Resistance of apples may be increased by injecting them with solutions of malic acid, cane sugar or various potassium salts.

Dr. C. WEST and Dr. A. J. SMITH.—*The Control of Atmospheric Conditions in Fruit Storage.*

In the preservation of fresh fruits two biological systems have to be considered—that of the fruit itself, a living system in senescence, and that of the microbial flora on its surface. Both can be profoundly influenced by modifying the atmospheric conditions in the storage chamber.

In theory, the necessity for controlling atmospheric conditions cannot be avoided, if only because the metabolism of the fruit is continually changing the composition of the atmosphere. By accident if not by design, carbon dioxide must be removed and oxygen must be added, and the amounts of these gases present will represent a balance between production or consumption and removal or addition.

Both gases exert characteristic effects on plant metabolism, oxygen as an essential for the supply of energy to the living machine, carbon dioxide as a stimulant in small doses, a depressant in moderate doses, and in still greater amounts a poison.

An atmosphere containing less oxygen and more carbon dioxide than is present in ordinary air retards the ripening processes of certain varieties of apples, and is now being commercially utilised for their preservation. Caution is necessary in generalising this result, since the beneficial effect may easily pass over into a harmful one when the percentage of carbon dioxide is further increased, or that of oxygen further diminished, beyond a certain optimal amount, which varies with the variety, and is also a function of the temperature of storage. ‘ Brown heart ’ in commercial shipments of Australian apples was one of the damaging effects of gas-storage conditions, inadvertently obtained and inadvertently carried beyond a safe limit.

The growth of moulds is often inhibited by absence of oxygen or by high concentrations of carbon dioxide, and is considerably retarded by concentrations such as can be tolerated by apples.

Water vapour ranks as one of the most important constituents of the atmosphere of the cold store, although its actual percentage by volume may be only about 0·5 per cent., and its control is difficult because the small amount present is governed by the rates of two relatively rapid, opposed processes—evaporation from the fruit and condensation on the cooling pipes. In turn, the concentration of water-vapour governs the rate of evaporation. Loss of water is to be avoided as far as possible, but a high relative humidity favours the growth of moulds. The problem of balancing these requirements becomes particularly acute in the storage of soft fruits, where the tendency to lose water and the susceptibility to mould attack are both greater.

The more complicated volatile products of the metabolism of fruits, which are largely responsible for their smell and flavour, have in some cases a harmful effect when allowed to accumulate in the storage atmosphere. They are probably instrumental in causing ‘ scald ’ in apples. Acetaldehyde is of particular interest as a very simple compound of this class, which probably occurs as an intermediate compound in normal metabolism. The respiration of apples and of oranges is considerably increased at all temperatures by the addition of small concentrations of the vapour acetaldehyde. The germination of mould spores and the growth of moulds, on the other hand, are retarded or inhibited.

Anæsthetics such as chloroform and ether form another group of gaseous substances which have been employed to control the metabolism of living plant products, and whose physiological effects are moderately well known. Small concentrations of hydrogen cyanide greatly accelerate the rate of respiration of potatoes, apparently by increasing the rate of starch hydrolysis.

Ammonia and other gases have recently been shown to retard very considerably the development of moulds. Strong claims for ozone, in a low concentration, have also been made in this connection, but are not yet well-established, although ozone may in any case have a useful application in the oxidation of odorous compounds present in the storage atmosphere.

Ethylene is noteworthy as an accelerating agent for the ripening process in bananas and some other fruits : some ethylene derivatives and other hydrocarbons have also been shown to exert similar effects.

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

The Conference was held in the Jehanghir Hall, Imperial Institute, London, on September 24 and 29, under the presidency of Sir Arthur Smith Woodward, F.R.S.

Fifty-eight delegates signed the register, representing sixty-four societies.

Thursday, September 24.

Address on *Geology as a Subject for Local Societies*, by Sir Arthur Smith Woodward, F.R.S., President of the Conference.

When I had the honour of presiding over this Conference in 1905, my introductory remarks were somewhat discursive and ranged over various aspects of the work of the Corresponding Societies. On the present occasion, as befits advancing age, I propose to be less ambitious and refer only to a single subject connected with the researches which have interested me during most of my life. It has been customary of late years for the Chairman in this way to limit his opening address, and I cannot do better than follow the example of my immediate predecessors. I wish to speak of geology and palæontology as furnishing opportunities for profitable research to members of the Societies who have other daily occupations and can only devote some of their leisure to the pursuit.

Modern learning—natural science—is in danger of becoming as much the prerogative of privileged corporations as was mediæval learning when it was monopolised by the monasteries and other ecclesiastical bodies. The results, it is true, are no longer expounded in Latin or Greek; but modern scientific papers and treatises are so thoroughly permeated with newly-invented technical terms which are supposed to give exactitude even if they do not express new ideas, that it is increasingly difficult for a layman to understand them. The conclusions which are prepared for outsiders in plain language have often to be taken on trust. There are still some branches of science, however, which a non-specialist can help by making desultory observations and collecting materials; and geology and palæontology are among the number.

Perhaps the most obvious service which the casual observer can render to geology is to watch temporary excavations and prepare exact records of them accompanied, if possible, by photographs. The Geological Survey has now published maps of all parts of the country, usually with explanatory memoirs, and it is easy to use these as the basis for further observations which may either confirm, extend, or correct those already made. If there are fossils or special rock-structures to be collected, as many as possible should be kept until they have been exhaustively studied.

Among temporary excavations may be included the working-faces of quarries in which fissures or caves are sometimes exposed. The contents of these fissures are usually thrown away as rubbish without adequate examination, but they sometimes include bones and other fossils which are of the greatest interest. It is sufficient to recall the fissures near Frome in which the late Mr. Charles Moore discovered important Rhætic fossils, the fissure near Ightham, Kent, from which Mr. Lewis Abbott obtained the remains of several Arctic Mammals dating back to the Glacial Period, and the fissure at Doveholes, Derbyshire, from which the late Sir William Boyd Dawkins recovered many teeth and bones of Mammals of Pliocene age.

Gravel pits are also well worth watching not only for the fossils they may contain, but also for occasional 'foreign' pebbles which are either remnants of geological formations which no longer exist or denote changes in topography of which there may

be no other evidence. In regions where there are deposits of the Glacial Period, observations on the pebbles are also most important.

In this connection geologists on the coast should not overlook the stones which friendly fishermen may bring home in their trawls. Some years ago a new fossiliferous Tertiary formation in the bed of the North Sea was thus discovered through the fishermen from Aberdeen. On the Yorkshire coast there are Ammonites representing geological formations which do not occur on the land; and on the Norfolk and Suffolk coasts it has long been known that interesting remains of Pleistocene Mammals can be recovered by the trawlers from the sea-bed on the Dogger Bank. Some fresh-water mussels once found at the bottom of the sea at the Atlantic end of the English Channel seem to show that there are river-deposits in this Channel from which valuable specimens may also be dredged at any time.

Finally, newly ploughed fields after rain should never be neglected for well-weathered fossils or for stray pebbles and human stone implements.

In addition to the field observations and collecting, good service can be done at home by those who have the time and patience to make preparations of the softer sedimentary rocks to show the nature and relative proportions of their several constituent particles. After the rock specimens have been disintegrated, the particles can be separated into groups by using fluids of different densities, and some further separation can then be done laboriously under the microscope. Preparations of this kind are needed in large number to determine the possible origin or origins of the sediments, thus adding to our knowledge of the land areas and the direction of the rivers and currents at the various geological periods. It must also be added that for those who are able to arrange more elaborate apparatus, there is still almost unlimited scope for making useful microscope-sections of both rocks and fossils.

The collection of fossils in most parts of this country is now much more difficult than formerly. Better facilities for transport and the relatively high cost of building in stone, have caused most of the smaller quarries to be closed; while the larger quarries have introduced machinery which usually prevents much careful searching. Some former industries which led to the discovery of many fossils—such as pyrites-gathering on the shores of Kent and Dorset, and phosphate-workings in East Anglia—have also come to an end. It is thus no longer easy to obtain fine specimens like those contained in the great collections of last century's amateurs which form the greater part of the British geological treasure in the British Museum. Many of those early amateurs were well-to-do men and women who supplemented their own efforts by employing patient and skilled professional collectors who could devote their whole attention to the task. Under present changed circumstances, such collectors could no longer make a livelihood.

Fossil-collecting, however, remains just as important as ever for the progress of our science, only most of it must be done in a different manner and usually with a different object. While nearly perfect specimens are still welcomed whenever they can be obtained, the exact localities and formations in which they were found must be truly stated, and not falsified to mislead rival collectors as was often the case in pioneer days. These fine specimens are no longer the main object of search, but rather a multitude of typical fossils of each stratum, whether complete or fragmentary, which can be studied intensively from our present standpoint. Fragments are often as important as whole specimens, for they may display features which are obscured when the fossils are not broken. Being now convinced of the truth of the doctrine of organic evolution, we are anxious to work out as many genealogies as possible among the fossils of successive geological periods, and so to discover some general principles. This can only be done by attending to a multitude of minute details.

In most cases alterations in the groups of fossils in successive layers of rock are due in large part to the migration of faunas following changes in local conditions. We can then only begin to study the evolution of the various animals represented after we have collected the evidence from many localities. In any great geological formation it is necessary to determine the precise succession of groups of fossils in the series of beds in each available section where they occur, so that one succession may be compared with another. When this has been done in many sections over a large area, it is possible to discover the local gaps in the succession and what would be the complete series if all the groups happened to be shown superposed on one another in any one spot. The late Mr. S. S. Buckman most laboriously studied the Lower Jurassic formations in this way, tried to discover local gaps, and eventually supposed

that he had made out the complete succession at least in England. The fact that many of his conclusions have been questioned by subsequent observers on the same ground, shows how desirable it is that these intensive studies should be made repeatedly in each district before we are satisfied that we have a real knowledge of the facts. They can be best undertaken when there are sea-cliffs or river-cliffs to give long sections, or where there happen to be exposures of series of beds in the courses of streams.

Sometimes, however, when conditions have remained nearly uniform during the deposition of many layers of sediment, the majority of the fossils in an upper (or newer) bed are the same as those in a lower (or older) bed, only slightly modified during lapse of time. In these cases the course of evolution can be followed to a large extent in one and the same geological section, and no effort should be spared to collect and prepare everything, even if a large proportion of the specimens are discarded as soon as the detailed study of them is complete. As examples of such research may be specially mentioned the important observations which have been made during recent years on the evolution of certain animals as they are traced by fossils through the successive layers of the English Chalk. They were begun by the late Dr. Arthur W. Rowe who discovered and described the changes in some sea-urchins, and they were continued by Dr. W. K. Spencer on star-fishes, and by Dr. W. D. Lang on polyzoans.

I have mentioned that by studying the succession of the fossils we hope to discover some of the general principles in the evolution of the groups represented. If we succeed, one result will be the simplification of the naming of the specimens. So long ago as 1858, when the Geologists' Association was founded, the difficulty of naming fossils was already recognised, and it was one of the special objects of the Association to ensure mutual help in the labelling of all fossils with ' the correct names.' During more recent years difficulties have greatly increased, and in many groups both generic and specific names have multiplied with so little reason and in so haphazard a manner, that nomenclature is now usually bewildering. Among Ammonites and Brachiopods, for example, almost every specimen described in some recent publications seems to have a separate name, and this is often given without any tangible diagnosis. If only, by the intensive study of lineages, we could discover the changes passed through in time in each well-marked form, it would suffice to employ specific names in as wide a sense as that adopted by most of the older naturalists, and to distinguish between the immature traits of its earliest representatives, the marks of vigour in its most flourishing time, and the oncoming signs of old age just before its disappearance. These stages need have no separate names—they can be described as immature, dominant, or senile forms respectively, or may even be indicated by the technical terms which are already in use by some who study lineages.

Perhaps the average collector will prefer to leave the naming of his fossils to specialists who have had the opportunity of making detailed studies of the several groups. Even so, he can much facilitate this work by carefully preparing the specimens and making them ready for study. The patient cleaning of fossils, and the devising of methods of displaying their structures, make a fascinating occupation; and success provides satisfaction akin to that which an artist feels when he has completed a masterpiece. He may merely divest them of surrounding matrix by using needles and knives, as did Mantell when he first cleaned fishes from the English Chalk; or he may follow Rowe and others in more delicate preparation by metal brushes and other tools worked by the dentists' engine. He may devise means of uncovering parts of limestone fossils by solution in acid, following the lead of Norman Glass when he first displayed for study the loops and spirals inside fossil Brachiopods. If he can obtain the necessary apparatus, he may even make serial sections in various ways. He will, however, experience the greatest satisfaction and do the best service by studying each individual case, and discovering appropriate means. Students of fossil plants have been particularly successful in this way, and Dr. O. M. B. Bulman's new preparations of Dendroid Graptolites and the remarkable fossil *Palæospondylus* may be mentioned as specially ingenious achievements.

One who has not access to much literature of the subject may say that the collecting and preparation of rocks and fossils in the manner I have indicated are tasks beyond the power of the average amateur. Fortunately, however, there are now helpful books giving both practical and theoretical instruction, and I would particularly mention the late Prof. Grenville Cole's ' Aids in Practical Geology ' and

Prof. H. H. Swinnerton's 'Outlines of Palæontology.' Furthermore, no real enthusiast in research who strives to help himself, will fail to receive a sympathetic welcome from those who have made such research their profession and are in a position to give friendly advice.

On the motion of Mr. T. Sheppard the President was warmly thanked for his Address.

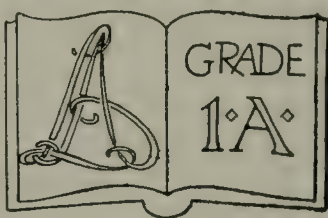
The Conference then considered the subject of *The Durability of Paper for Scientific Publications*, Dr. F. A. Bather, F.R.S., moving the following resolution:—

> 'The Delegates of Corresponding Societies in Conference on September 24, 1931, in London, desire to impress on the editors of all scientific publications, especially those issued by the Corresponding Societies, the importance of printing at least a limited issue of those publications, both text and plates, on a durable paper (such as those designated Grades 1 and 2 by the Library Association in its recent Report, 1930), and requests the Council of the British Association to communicate this resolution with its endorsement to all publishing societies with which it is in correspondence.'

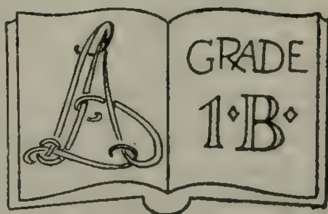
Dr. Bather referred at length to the serious risk of certain valuable records being lost or not available for future reference because of the non-durability of the paper upon which they have been printed, and he urged the importance of printing and publishing communications and records of local scientific observations upon paper of proved durability.

Mr. Norman Parley.

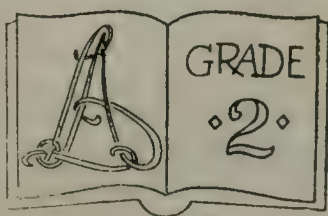
The Library Association issued last autumn its report on 'Durability of Paper.' Until then there had been no public standard of durability in this country, very few papers were watermarked, and price was no reliable guide: that is, an expensive paper could not safely be assumed to be a durable one. Nor could an editor, a librarian or a publisher, still less an ordinary book-purchaser, ascertain whether a book was printed on a reliable paper or not. This state of affairs, which applied also in other countries, led the International Committee on Intellectual Co-operation of the League of Nations in August, 1928, to resolve:—



> 'That the attention of Governments should be drawn to the necessity of using for documents (and printed matter) of permanent value . . . only papers manufactured according to (given) specifications.'



During the same year Mr. Arundell Esdaile of the British Museum had mentioned to me his interest in the subject, and in an article in the *Library Association Record*, edited by him, I had revived a scheme for grades of paper, made to specifications to be agreed. This project was formulated by the Society of Arts in 1898, and carried a stage further by the Library Association between 1905 and 1913, but had not achieved permanent results.



Resolutions passed at two Library Conferences in the autumn of 1928 led the Library Association to set up a committee, which, strengthened by co-options and supported by technical and propaganda sub-committees, completed its main task with the issue of the unanimous report late in 1930, a very condensed summary of which is given below.

This report deals first with the durability of

paper from a historical standpoint. It then dwells forcibly on the necessity for a testing station, discusses practical possibilities and explains the consequent scope of the present inquiry. Then it endeavours to settle the question of Grades of Permanence in paper, and proceeds to treat in some detail that part of the problem which relates more particularly to paper used in present-day book production. Specifications are laid down for grades of durable paper, the important question of the market prices of such papers being fully considered. The Committee conclude by dwelling on what seems to them the practical outcome of the whole matter.

It is entirely with the practical outcome we are concerned here to-day. The grades of paper have been set up and registered and grade marks adopted (see illustrations) by which they can be identified. A sufficient number of mills of high standing are making grade papers to the specifications (at the same prices as those ruling for unspecified papers), and they are obtainable without difficulty through many of the largest and best-known suppliers in the country.

But now comes the crux of the whole problem. Somehow we must promote an educated and steady command for these durable papers, or the makers will not continue to produce them. Up to the present this demand has not travelled far outside the immediate influence of members of the Committee.

We have the librarians behind us, are gradually obtaining the support of the learned and scientific societies, and of such bodies as the British Record Society, the British Science Guild, the Institute of Municipal Treasurers and Accountants, the Institute of Public Administration, the Royal Institution, the Royal Society, while one of the largest and most influential firms of publishers in London has adopted grade 2 for all ordinary purposes.

What we ask you to do is to use your influence with your own society or societies to ensure that future publications shall be printed on one of the grade papers, and in general to give the new National Mark your blessing and your practical support.

Summary of Report.

During the last thirty years many scientists in different countries have pointed out that, owing to the employment of papers manufactured by certain modern processes, a large part of the printed and written productions of the present age will possibly have perished before they can be utilised by the learned world of the future. These conclusions have never been seriously questioned, but there has been comparatively little organized effort to deal with the situation from the point of view either of printed books or of archives.

The general problem of Papers used for Printing and Archives.—The first aim of the Committee was, therefore, to raise the standard of paper used for the production of printed books by specifying papers which should be durable and yet no dearer than the ordinary papers at present used by publishers; and to get these actually put on the market. This has now been done.

The Committee has not prepared separate specifications for papers for Archives (including Registers, Accounts, Minutes, and all kinds of original Memoranda and Correspondence), because it is agreed that for these the same basic specifications can be used. Other special problems of Archives are mentioned below.

Grades.—The Committee came to the conclusion that the only practical plan was to recognize grades of 'absolute' and 'relative' permanence (the latter being such as would give to a reasonably distant posterity the opportunity to use the productions of our age), and to set up corresponding grades of paper identifiable by registered mark. The following grades were established :—

Grade 1, for use where 'absolute' permanence is required, is based on the characteristics of papers which have actually stood the test of centuries. Only first quality rag or other pure cellulose may be used.

Grade 1 is subdivided into two categories :—

1 (*a*). All rag, hand-made.
1 (*b*). All rag, machine-made.

Grade 2, for use where 'relative' permanence is desired, and a low price is necessary, is to be manufactured from properly prepared all-chemical wood stock.

Specifications for each of the above, covering chemical and other requirements, will be found in the report.

Price and Production.—The vital conclusion arrived at is that durable papers of these grades can be produced at prices no higher than those now ruling for present-day

average printing papers; in fact, the all-rag machine-made paper (Grade 1 (*b*)), can be obtained, as will be seen from the price ratios given below, at a ost less than hitherto.

As a consequence of the issue of the report and the beginning of a demand thus created, papers corresponding to each grade have been put on the market and extensively advertised by a number of the principal suppliers in this country.

The special problems of Archives.—The difficulty here is that of predicting, at the time when papers are written, which will be preserved and should therefore be on permanent paper, and which may be consigned to the cheapest possible material. The report, however, suggests among other things:—

(*a*) That there are always certain larger series, whose preservation can be decided in advance, which should be on Grade 1 paper;

(*b*) That the section in any office which controls the ordering and distribution of paper should be acquainted with the importance of the matter and the grades of paper available, and encouraged not only to consign probable survivals to (at least) a Grade 2 paper, but also, by using the cheapest possible paper for ephemeral purposes, to make the use of Grades 1 and 2 more possible in other cases;

(*c*) That in the case of Archives it is useless to consider qualities in paper without considering also other writing materials: though this matter is not strictly within the terms of reference of the Committee, it is suggested that the materials to be considered include inks and paints, carbon and other copying papers, and typewriter ribbons, pencils, 'chalks,' pens and stamps, the materials used for the surfacing of papers (particularly photographic papers) and their subsequent treatment, and adhesives. As a matter of general practice the exclusive use of black in carbon papers and typewriter ribbons, and of a gallo-tannate and ferro-tannate writing ink, will ensure safety.

Practical Proposals.—The all-important thing is now to set up a steady demand for good paper, and in particular to secure that those who use paper for purposes which involve a serious contribution to knowledge—the publication of books of permanent value or the writing of Archives—should realise that grade papers at competitive prices[1] are now actually on the market, and proceed to take practical advantage of this fact.

The Library Association will, therefore, be very much helped in implementing the findings of its committee if organisations which are in general agreement and which desire to use durable paper for their publications and records, whether printed, typewritten or manuscript, will

(i) *Order papers manufactured to the specifications of the Committee; and*

(ii) *Notify the Library Association (or other body through whom this Memorandum is distributed) of the assistance they are thus prepared to give to the scheme.*

It will generally be found sufficient to order the papers by quoting the number of the grade required, but if supply should present any difficulty, a list of firms stocking papers to the Committee's specifications can be secured on application to the Secretary of the Library Association, 26-27 Bedford Square, London, W.C.1, from whom also copies of the Report can be obtained, 1*s.* 1*d.* post free.

[1] Present prices (in ratio) may be summarized thus:—

NON-GRADE PAPERS (*Papers in general use, of corresponding character, but not made to the Committee's specifications.*)	GRADE PAPERS (*The figures given in this column are for Grade papers already on the market.*)
90 to 96 units	Grade 1 (*a*): 90 to 96 units
About 40 ,,	Grade 1 (*b*): 27 to 40 ,,
11 to 16 ,,	Grade 2: 11 to 18 ,,

Mr. Parley called attention to the special exhibition of specimens illustrating the subject which was being held in connection with the Conference.

A discussion followed, in which the following took part: Dr. Arundell Esdaile, Dr. C. J. J. Fox, Mr. Hilary Jenkinson, Dr. L. J. Spencer, Mr. James Strachan, and Major J. Edington Aitken, the last proposing as an amendment the deletion from the resolution of the words specifying the grade of paper recommended. Upon being put to the meeting the amendment was lost and the resolution carried.

Tuesday, September 29.

DISCUSSION on *The Effects of Urban Expansion upon the Flora and Fauna of the Countryside.* (Sir E. JOHN RUSSELL, F.R.S.; Prof. E. J. SALISBURY; Mr. T. SHEPPARD.)

Sir E. JOHN RUSSELL, F.R.S.

It is a commonplace that the spread of the urban population has a devastating effect on the native fauna and flora. There are two chief ways in which this is done: direct and indirect. The direct methods are the uprooting of all wild plants thought to be scarce, the complete collection of all visible flowers, thereby reducing the possibility of seeding; the taking of birds' eggs and nests; the destruction of young animals by the cats and dogs that accompany the human invaders.

The indirect effects are the destruction of quiet breeding and nesting places, in the 'tidying-up' that follows the division of a peaceful countryside into 'desirable building plots,' the cutting down of trees and hedges in the widening and straightening of lanes, and the conversion of moist places into dry ones by drainage operations, and the pumping that has lowered the water table in England considerably during the past fifty years.

Examples of all these are given.

Some of these effects are inevitable, and we can hardly hope to preserve our native flora and fauna intact where the human population is increasing. A good deal of the destruction is, however, unnecessary, and arises from the fact that the new population coming into the country districts is entirely urban in its outlook and education and lacks the knowledge of country things, of the modes and manners of country life, and of the places likely to afford good nesting or homing places for birds and other wild animals. It would be very helpful if someone with the knowledge, the time and the tact would produce a handbook written for the new country dwellers, setting forth the things that can be done and those that should not be done; in this way much unintentional damage could be avoided.

Even more useful would be concerted efforts to save the small woodlands and coppices which dot the countryside. These afford sanctuary for all but the shyest of animals, and even these soon learn the places where they are left unmolested. This should not be an expensive matter, especially in view of the increased value of the neighbouring sites as the result of the better preservation of the amenities of the district.

On the edge of the newly developing building areas there is an intensive preservation of game for sporting purposes: this involves much destruction of wild life by gamekeepers. As against this there is a reduction of gamekeeping in some of the remote rural districts.

Prof. E. J. SALISBURY.

These effects are due to both direct and indirect factors. One of the most important of the latter is man's influence on the natural balance between species. Even rare species show marked persistence where conditions are stable. Protection necessarily involves artificial control owing to natural succession. The best palliative would seem to be the scheduling of large areas to be maintained in their present condition by careful control.

Mr. T. Sheppard.—*The Effect of Urban Expansion on the Fauna and Flora of East Yorkshire.*

Probably few areas have been altered so much by the necessary interference of man as has the East Riding of Yorkshire, and certainly few can show such a marked change in the consequent flora and fauna. Natural forces, aided by man, have added to these changes.

In comparatively recent times the 'Isle of Holderness' was a land of meres and marshes, resembling the Norfolk Broads of to-day. The place-names and others indicate the former mere conditions. Until the area was drained in the eighteenth century the villages which were placed for the most part on the raised glacial mounds were separated from each other during the winter months, communication being possible when the carr lands were frozen over, or by flat-bottomed craft known as carr-waddles. In prehistoric times pile-dwellings existed inside these meres, and pelicans, cranes and other birds were denizens.

To the west of this area is the sweep of the high chalk Wolds, formerly largely waste, devoted to rabbit warrens and woodland, and the home of the wolf, deer, pole-cat and great bustard, the last of which was trapped in 1832. Through the energies of the Sykes family all the area has been reclaimed and put under cultivation, and to-day is one of the most fertile districts in the country and, in addition, is famous for its breed of horses.

To the west of these again is the Vale of York, with its accumulation of blown sand, which, as a result of drainage, is now particularly suitable for potato and other crops.

In the western extremity of the Riding—in the Goole and Selby areas—important changes have taken place. In the reign of Charles I, Vermuyden, a famous Dutch engineer, drained the area, and the name 'Dutch River' and numerous Dutch personal names in the district remind us of these changes. The effect on the fauna was immediate, and as the population which made its livelihood by catching wild fowl and by fishing (particularly for eels) saw their quarry disappearing, they cut the dikes, and in other ways endeavoured to prevent the improvement of their land. An oil painting exists showing 'King Charles I killing deer driven into Thorne Mere' by an enormous number of boatloads of men. Neither the mere nor the deer now exist at Thorne, and further alterations to this large area are now being made as the result of the discovery and working of coal.

Great changes still occur as a result of coast erosion. For thirty miles between Bridlington and Spurn the soft glacial and post-glacial beds are being washed away at an average rate of 7 feet a year, and a large number of townships and villages have been engulfed by the sea in comparatively recent times.[1] Others have been saved by the protection of groynes and sea-walls. The fine mud and sand derived from these cliffs have been carried south and into the Humber, and in this way the area known as Spurn Point, Sunk Island, Broomfleet Island, and Read's Island (the last the only *island* now) have been formed and have added thousands of acres of valuable land to the Riding.

Spurn Point, a narrow neck of land 4 miles in length, the breeding place of the Lesser Tern and Ringed Plover and the home of the sea holly and other rare plants, is now an important zoological and botanical centre, though of recent origin. Myriads of birds assemble here on migration. While a service was being held in Easington Church news was brought that the 'Woodcock has arrived.' (These birds were usually so exhausted that the villagers were able to kill them with sticks.) The parson ordered the doors to be closed while he took off his surplice and cassock so that 'we can all start fair.'

To-day the Yorkshire Naturalists' Union employs a watcher during the season, and the area is kept as a nature reserve as much as is possible. 'Sunk Island,' as one large reclaimed area is known, caused the havens of Patrington and Hedon to be closed, and the latter township, which once returned members to Parliament, is now a small market town. As well as Spurn Point, Hornsea Mere, the last of our lakes, and the cliffs of Bempton and Speeton, are 'breeding grounds' of many species of rare birds, and these are watched by paid keepers. The Dotterill Inn near Flamborough is called after a bird now long extinct in the district, and the raven, eagle, brock (or badger), wolf, wild cat and other names still preserved in place-names

[1] 'The Lost Towns of the Yorkshire Coast.' T. Sheppard, 1912, 340 pp.

are an indication of the former condition of things. Botanists year by year have to record the changes in the flora owing to the improvements in drainage and the progress of the plough.

Dr. Vaughan Cornish.

It has been thought desirable that a somewhat wider scope should be given to the discussion than that indicated in the programme, and my contribution will relate to the human factor.

The aspect of urban expansion to which I wish in the first place to draw attention is the ubiquity given to the excursionist by the freedom of movement in which the motor differs from railways.

The latter, focussing in every great town, have long since made the countryman familiar with the city, so that the yokel, unacquainted with town fashions, only remains as the comic countryman of the stage.

But townsmen in the mass have not yet had time to make the corresponding adaptation of manners to environment, and the charabanc too often destroys the quiet of the country when it comes, and leaves behind a legacy of litter when it goes. Now if it be sad that the country should be spoilt in this way, is it not at least equally lamentable that the urban crowd should be so blind to the beauties of nature that they can derive no inspiration therefrom; for it is evident that when there is no reverence there is no understanding ?

One half, perhaps the more important half, of our problem of preserving the charm of the countryside, relates to the education of the urban masses. We know that the broadcasting of appeals with reference to litter and the uprooting of flowers does something, but this is not enough. Something could be done by a lesson and exhortation in the elementary schools on the eve of the terminal holidays, but this is also not enough ; and most educational curricula are already so crowded that it is difficult to introduce new subjects without encouraging superficiality.

I turn, therefore, with hope to some pregnant sentences in the report of the National Park Committee presented to Parliament last April (Cmd. Paper 3851). The Committee say that—

'The growth of the open-air habit has been a notable feature of post-war life. Ramblers federations number more than 40,000 members, and recently a Youth Hostels Association has been formed to help all, but especially young people, to a greater knowledge, love and care of the countryside. . . . We think that these tendencies should be encouraged.'

To this list I would like to add the School Journey Association.

The Committee also say that ' the risk of abuse (of the amenities) will be lessened if visitors are encouraged to organise themselves in Societies,' and, in reference particularly to the recreational use of national parks, refer to the great advantage which will result where ' the societies which make rambling a feature . . . assume a definite responsibility for the behaviour of their members.'

My own observation confirms the opinion of the National Park Committee upon these points, and I venture to suggest that Corresponding Societies will be well advised to take active measures for the organisation of urban touring in the manner indicated.

There is one other point which I wish to make in conclusion. It relates to the tolerance by the uninitiated, even sometimes the preference for gaudy colour and restless pattern in bungalows and other buildings in the country. Here definite instruction by lectures to the members of rambling associations is desirable. Little is needed beyond insistence upon the cardinal principle that scenic beauty is primarily a matter of the relationship of the features of the landscape to the general background. Any building which takes its place quietly in the landscape will suffice ; any building which does not so conform is far more likely to be a blot upon the landscape than an adornment.

Dr. G. Claridge Druce, Mr. H. N. Dixon, Mrs. H. Forbes Julian and Mr. H. Edgar Salmon also spoke.

EVENING DISCOURSES.

THE PHOTOGRAPHIC ANALYSIS OF EXPLOSION FLAMES.

BY

PROF. WILLIAM A. BONE, F.R.S.

In principle the method usually employed for the photographic analysis of explosion flames is the same as that originally designed by Mallard and Le Chatelier fifty years ago. It consists in photographing the movements of the flame along a horizontal glass tube on a sensitised plate or film moving vertically at a suitable known velocity, thus obtaining (*inter alia*) a graph compounded of the two velocities, from which that of the flame at any point can be deduced.

Mallard and Le Chatelier employed horizontal tubes of diameters between 1 and 3 centimetres in sections, each 1 metre long, connected in series by means of caoutchouc rings. The whole was focussed, by means of a wide aperture lens, on a plate moving vertically with a known uniform velocity of about 1 metre per second. In this way, to quote their original memoir,[1] '*On obtiendra une courbe dont chaque point aura pour abscissa le chemin parcouru par la flamme dans la tube, et pour ordonné le temps écouté depuis l'origine de la combustion.*'

As the plates used by Mallard and Le Chatelier were not sufficiently sensitive to give satisfactory records with feebly luminous flames, such as those of hydrogen-oxygen explosions, they employed explosive mixtures of carbon disulphide with either oxygen or nitric oxide, whose flames are much more actinic, believing them to be typical of all explosive 'oxygen' or 'air' mixtures respectively. The behaviour of these mixtures on explosion was found to differ according as they were ignited at or near (*a*) the open, or (*b*) the closed end of a tube. In the former case the flame always proceeded for a certain distance along the tube at a practically uniform slow velocity, which was regarded as the true rate of propagation 'by conduction.' This initial 'uniform movement' was usually succeeded by an 'oscillatory period,' the flame swinging backwards and forwards with increasing amplitude, and finally *either* dying out altogether *or* giving rise to 'detonation,' according to circumstances. With some 'oxygen'-mixtures, the initial period of uniform velocity was short, and appeared to be succeeded abruptly by 'detonation,' without passing through any intermediate oscillatory period. When, however, the mixtures were ignited near the *closed* end of the tube, the forward movement of the flame was continuously accelerated until finally 'detonation' was set up. In 'detonation,' where the explosion is propagated from layer to layer by 'adiabatic compression,' the flame velocities are both uniform and high, *e.g.* usually of the order 2,000 to 3,000 metres per second.

These features of the flame movement will be illustrated by (i) a series of three slides reproducing some of Mallard and Le Chatelier's original photographs, and (ii) experiments showing the movements of explosion-flames through methane-air mixtures along a horizontal glass tube about 6 metres long and 6 centimetres internal diameter.

The experimental method was developed and improved by the late Prof. H. B. Dixon and his collaborators in Manchester during the 'nineties' of last century. They used a highly sensitive film rotating vertically on the periphery of a drum with

[1] *Annales des Mines* 8 *Ser.* iv. (1883), pp. 274–618.

some constant velocity (which, however, varied between 25 and 50 metres per second in different experiments), the explosion tube being placed at such a distance from the camera that the size of the image was about one-thirtieth of the flame. In this way they analysed the progress of an explosion from its origin up to the final attainment of its maximum force and velocity in 'detonation.' They also discovered (i) the wave of 'retonation,' which is thrown back through the still burning or chemically active medium from the point where detonation starts (a phenomenon also independently discovered by Le Chatelier in 1900), (ii) the effects of collisions between two explosion waves, as well as of the passage of 'reflected waves' through the hot products of combustion behind the flame front.

It is hardly possible to give any adequate idea of the wealth of new information derivable from the beautiful series of over seventy photographs of explosion flames included in the memoir ultimately published by Dixon in 1903;[2] but a selection of them will be shown on the screen, from which their principal features may be judged.

Within recent years the experimental method has been further developed and improved in our laboratories at the Imperial College, South Kensington, chiefly by means of the new high-speed photographic machines designed by Mr. R. P. Fraser, which have so increased its analysing power that (as will be shown later) it is now possible to photograph and measure movements in explosion flames occurring periodically with frequencies up to 250,000 per second. And with a new type of camera embodying the principle of a mirror revolving *in vacuo* at high constant velocity (30,000 r.p.m.) and projecting the image of the explosion flame on to a stationary film, we hope soon further to increase the analysing power four- or five-fold, so that in the near future we hope to be able to photograph and measure periodic flame-movements occurring with frequencies of a million per second. In reviewing some of these developments in *Nature* two years ago, the late H. B. Dixon, who had followed them with the closest interest, said that he envied us the luxury of our feelings.

A selection of some thirty typical photographs, illustrative of our results,[3] will be shown on the screen. They have already modified and corrected many of our former ideas concerning the nature of the initial phase of slow uniform flame-movement in gaseous explosions, and (*inter alia*) completely disproved the supposed 'law of flame speeds.' Indeed, it now seems probable that what Mallard and Le Chatelier visualised as flame propagation 'by conduction' is an ideal condition perhaps realisable only when a stagnant explosive mixture is ignited without impulse at the centre of a spherical vessel of infinite radius.

Important new information has been obtained regarding the influences of 'compression' and 'shock' waves upon the speeding up of combustion and flame movements during explosions; and it is now proved that the speed can be abruptly raised from a lower to a higher uniform value when a flame is overtaken by a 'shock' wave travelling in the same direction. Indeed, it has been shown that in such manner the flame-speed can be successively raised '*per saltum*' many times, and that it may assume any uniform value between the limits of that theoretically corresponding with propagation 'by conduction' and that due to propagation 'by adiabatic compression'; examples of this will be shown on the screen.

For purposes of exposition it is convenient to distinguish between *three* successive phases of a gaseous explosion as developed in a tubular enclosure, namely: (1) the *initial* period, when the flame is travelling at a speed *less* than those of any shock waves overtaking it in the medium, (ii) the pre-detonation stage, when the accelerated flame is travelling at a greater speed than any 'shock' waves, and therefore catching up with, and overtaking, such as may have already passed through it, and (iii) the final phase 'detonation.'

Much new light has been thrown on what may be termed the 'pre-detonation' phase of explosion, when the flame is advancing at a speed greater than that of any 'shock' wave through the unburnt medium, and is therefore overtaking any such shock waves which are ahead of it.

The photographs also show how 'detonation' is ultimately set up when a rapidly moving flame on the verge thereof is just about to overtake a shock wave immediately in front of it. Thereupon an 'ignition ahead' of the flame-front occurs, and

[2] Phil. Trans. A 200 (1903), p. 315.

[3] Some of these have already appeared in the Proceedings and Philosophical Transactions of the Royal Society during the past five years, and another instalment is now in the course of publication therein.

immediately afterwards ‘ detonation ’ is set up. Indeed, during the ‘ pre-detonation ’ phase a series of successive such ‘ ignitions ahead ’ may occur, detonation being set up immediately after the last one.

Photographs will also be shown analysing the phenomenon of ‘ spin ’ in detonation which was first observed five years ago in detonations of moist $2CO+O_2$ mixtures by C. Campbell and D. W. Woodhead, working in the late H. B. Dixon's laboratory, and later confirmed in our laboratories at South Kensington. It has also been observed in detonations of methane-oxygen, pentane-oxygen, undiluted acetylene and other media, and apparently is caused by the helical rotation of a luminous ‘ head ’ of detonation in the flame front together with a long luminous ‘ tail ’ behind it. The pitch (L) and frequency (f) of such rotation in detonations of a given medium varies with the internal diameter (D) of the tube, so that, while the ratio L/D for diameter up to about 1 inch is constant, and nearly equal to 3·0, the helical velocity of the rotating ‘ head ’ of detonation is approximately of the same order, irrespective of the medium or of the tube diameter. Thus, in the case of a moist $2CO+O_2$ medium, which exhibits the phenomenon most markedly, $f=44{,}000$ per sec., and $p=3{\cdot}95$ cms. in a tube of 1·3 cms. internal diameter, the velocity of the ‘ head ’ of detonation along its helical path being 2,500 metres per second.

These discoveries have necessitated a revision of the old classic conception of ‘ detonation ’ and opened up new lines of inquiry concerning it.

Within recent years the photographic method has also been used for the purpose of analysing (i) ignition phenomena, including the ‘ induction ’ period of explosions, (ii) the influence of moisture upon explosions of carbonic oxide-oxygen mixtures, and (iii) the influence of strong electrical fields upon gaseous explosions, and some results of such analyses were shown on the screen.

Of the thirty photographs shown on the screen during the lecture, the following eight are reproduced here by kind permission of the Royal Society showing the scope and power of the method in analysing the phenomena concerned in gaseous explosions. The thin black vertical lines in the photographs are due to ‘ reference marks ’ on the outside of the tube at regular intervals of 20 cms. from the firing spark ; a ‘ time scale ’ in millisecs. is shown in white on the right-hand thereof.

Phil. Trans. Roy. Soc., **A 228** (1929), Plate 9, No. 35.—No. 1 shows the effects of a series of superimposed ‘ shock waves ’ at regular intervals of 1/400 sec. upon the flame movement during the initial stage of a moist $2CO+O_2$ explosion initiated near the open end of a tube 2 metres long and 1·25 cms. internal diameter, the other end of which was closed. The flame, starting off from the igniting spark with an initial uniform velocity of *circa* 2 metres per sec. only, was struck and accelerated by five successive ‘ shock ’ waves, each of which abruptly raised its speed to a higher uniform level. In this way five successive uniform flame speeds of 9·2, 71·5, 76·4, 101·1 and 122·0 metres per sec. respectively, were imposed upon it during the first 0·65 metre of its travel. The short white arrows indicate where the four last shock waves overtook the advancing flame.

Phil. Trans. Roy. Soc., **A 228** (1929), Plate 10, No. 37.—No. 2 shows the flame of such a moist $2CO+O_2$ explosion in the ‘ pre-detonation ’ stage traversing the second metre section of the tube after having been accelerated (as in No. 1) by a series of ‘ shock ’ waves. On entering it the flame was rapidly accelerated, but soon attained a nearly uniform velocity of 1,575 metres per second, at which it continued for a distance of about 20 cms. Its velocity then decelerated somewhat to about 1,275 metres per second, and several successive ‘ ignitions-ahead ’ occurred, as the flame rapidly caught up with the ‘ shock waves ’ which previously had passed through it, the last one being as much as 3 cms. ahead. As the flame spread from this last and extremely well-defined ignition point, a sudden and very intensive local combustion occurred, giving rise to both ‘ detonation ’ and ‘ retonation ’ waves. The speed of the ‘ detonation ’ thus set up was 1,920 metres per second.

Forthcoming Phil. Trans. Paper, 1931, Photograph No. 4.—No. 3. In this case a moist $2CO+O_2$ medium has been ignited by the minimum requisite high tension spark at a point 2·5 metres from the *closed* end of the explosion tube, a ‘ detonator ’ being simultaneously fired right up against the closed end, the space intervening between the ‘ detonator ’ and the explosive medium being filled with nitrogen. By such device a powerful ‘ shock wave ’ was set up by the detonator behind the explosive medium at the moment of its ignition. A feebly luminous flame started off from the igniting spark with a uniform velocity of 38·6 metres per sec. ; after about 4 millisecs. it was overtaken, and its velocity abruptly accelerated to 308 metres per sec., by the

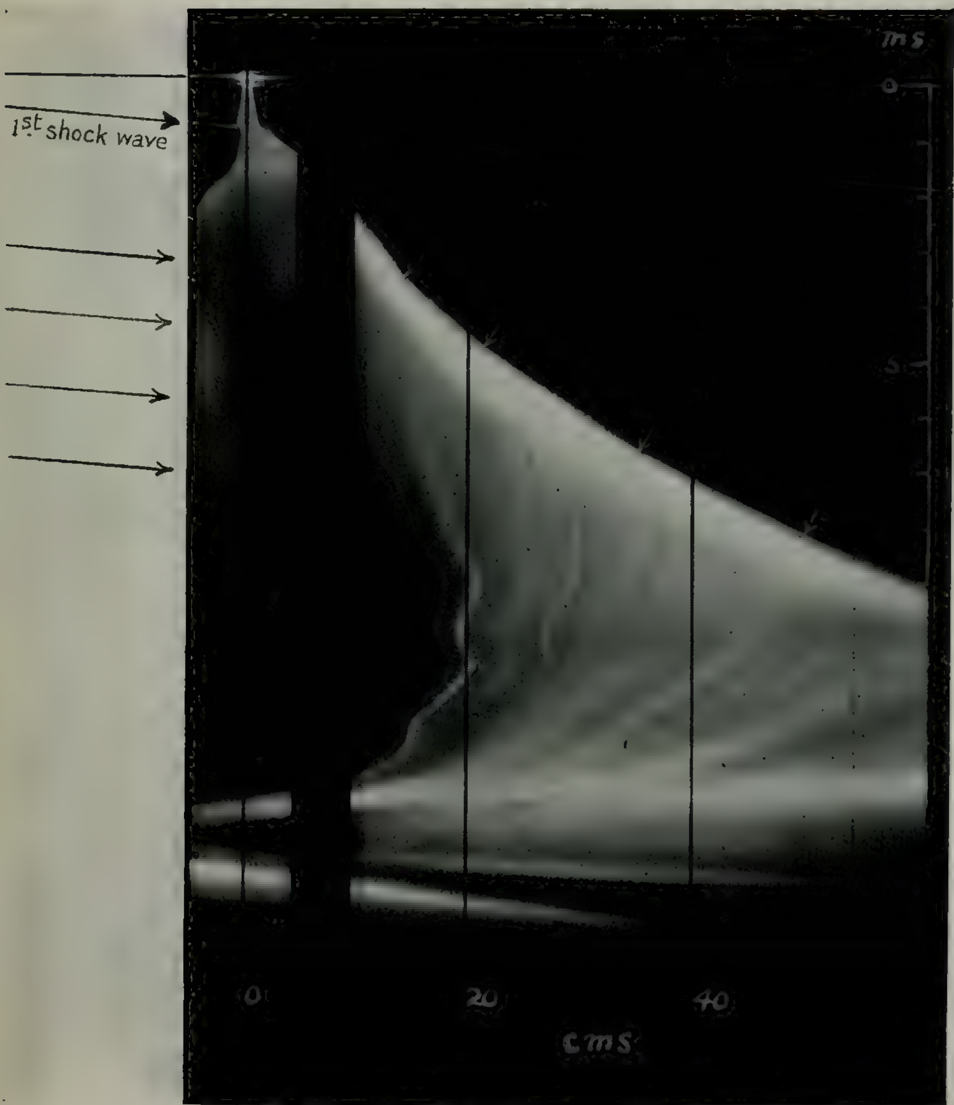
'shock wave' referred to, which, after passing through the flame, emerged from the front thereof at a point about 0·2 metres from the firing point, and continued ahead of the flame along an invisible track indicated by a white dotted line in the photograph. Meanwhile, the flame, following hard after it, with further rapid self-acceleration, soon all but overtook it, after a total run of about 1·2 metres, whereupon an ignition occurred at a point in the track of the shock wave a little (4·25 cms.) ahead of the flame, whereby 'detonation' was set up, a strong 'retonation' wave being sent back through the incandescent medium at the coalescence of the two flames.

Forthcoming Phil. Trans. Paper, 1931, Photograph No. 10.—No. 4 shows two such 'ignitions-ahead' during the 'pre-detonation' stage of a moist $2CO+O_2$ explosion, at the second of which 'detonation' was set up at a velocity of 1,940 metres per sec., a strong 'retonation wave' being simultaneously sent back through the incandescent medium with a velocity of 890 metres per sec.

Ibid., Photograph No. 14.—No. 5 is an excellent example of the phenomenon of 'spin' and its persistent uniformity in the detonation of a moist $2CO+O_2$ medium in a tube of 2·55 cms. internal diameter. The forward velocity of detonation was 1,795 metres per sec., the frequency of the helical rotation causing the 'spin' 24,000 per sec. and its 'pitch' 7·5 cms.

Phil. Trans. Roy. Soc., **A 228** (1929), Plate 8, No. 33.—No. 6 shows very beautifully a 'compression wave,' originated in the firing spark, being reflected backwards and forwards at the boundaries between the flame front and the unburnt medium beyond them, in a moist $12\ C_2H_2+88\ (2CO+O_2)$ explosive medium which had been ignited by a weak condenser-discharge spark (1 microfarad at 200 volts) at a point midway along a *closed* horizontal explosion tube 35 cms. long and 2 cms. diameter.

Proc. Roy. Soc., **A 114** (1927), Plate 33, Photo No. 3.—No. 7 is of an explosion similarly initiated in a CH_4+O_2 medium by a condenser discharge of 3·75 microfarads at 1,000 volts midway along a closed horizontal tube 35 cms. long and 2 cms. internal diameter. Here ghost-like flames were propagated in each direction along the tube, being marked by *striae* of enhanced luminosity whose forward projection was reversed as soon as the initial continuous acceleration of the flame front was checked. These *striae* ultimately coalesced near the middle of the tube, just as the two flame fronts reached the ends of the tube; whereupon a sudden outburst of intense luminosity occurred, the whole tube instantly becoming filled with an intensely luminous flame (the main combustion) traversed by compression waves, a condition which lasted 0·0025 sec.



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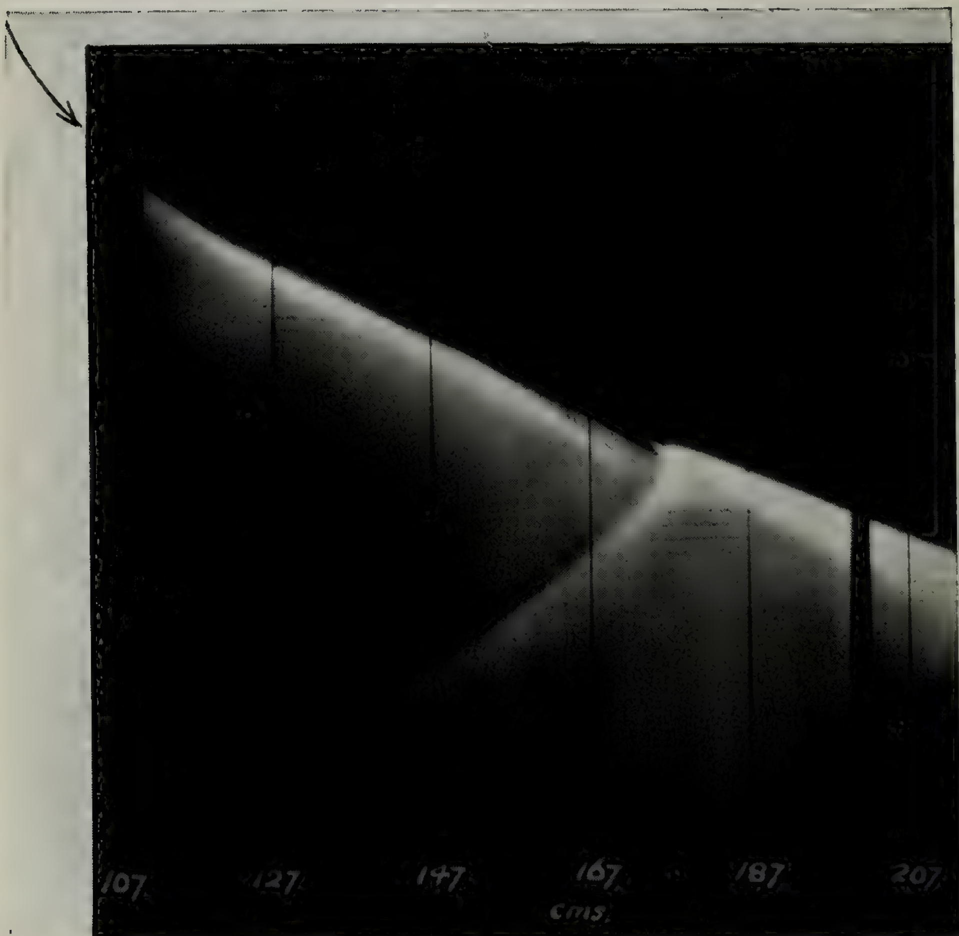

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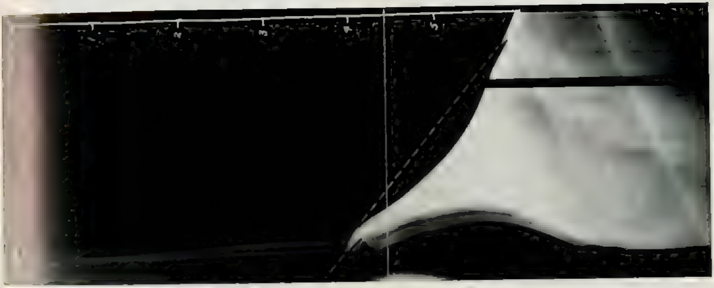

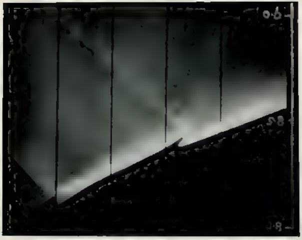

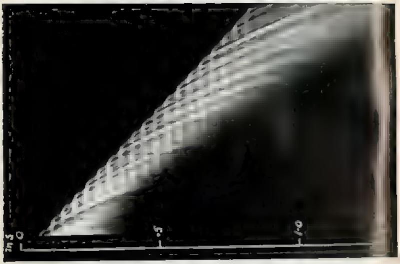

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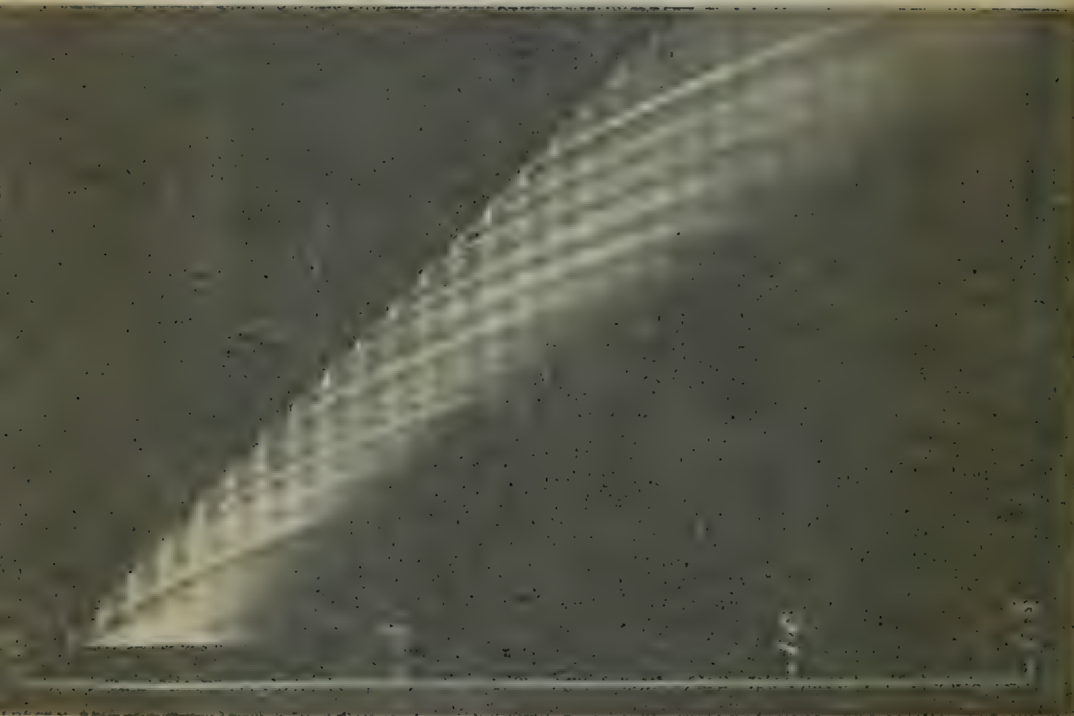

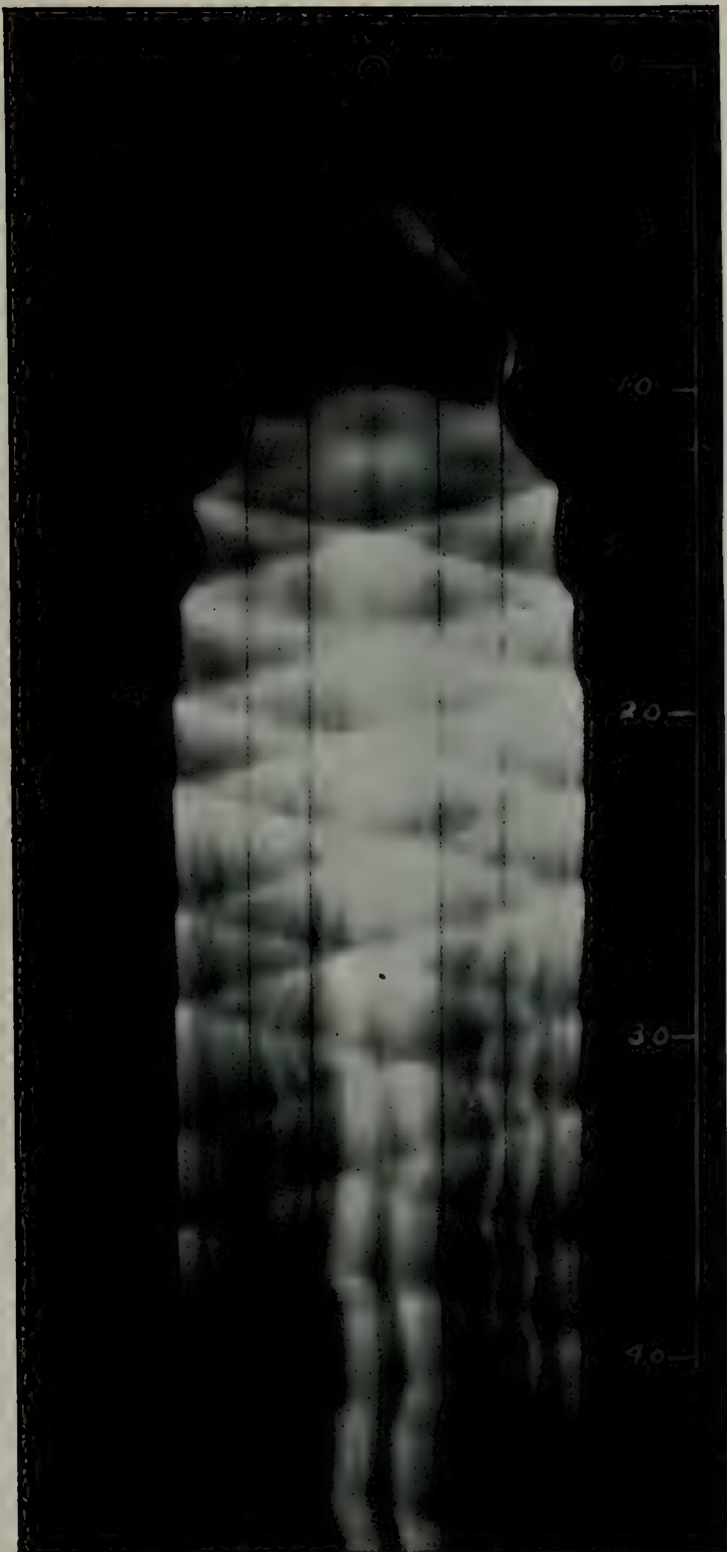

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'ZOOS' AND NATIONAL PARKS.

BY

SIR PETER CHALMERS MITCHELL, C.B.E., F.R.S.

THERE are three types of institutions for the display of living animals, each with specific functions and opportunities. The Zoological Garden in a city is, so to speak, a 'close-up' of the animal kingdom, endeavouring to present a picture of the richness and variety of the groups of animals, bringing its subjects from the tropics and the snows, from forests and plains, rivers and seas, so far as it is possible to reproduce artificially on a small scale the conditions in which they can live. The country Zoological Park has a larger area and simpler arrangements, and although the attempt is made to display creatures from many different environments, the choice must be restricted to animals which can prosper under conditions sometimes widely different from those of their natural homes. The National Park, using the term not in the wide sense which troubled Dr. Addison's Committee, but in the restricted sense of a large tract of country, as nearly virgin as possible, dedicated for all time as a sanctuary for its native animals and plants, should contain no animals, not at present native to the locality, except such as had recently been exterminated and were now reintroduced for their preservation, as for example the American bison in the great national park of the United States and Canada.

In the city zoo, as visitors had the opportunity of getting very close to the animals, it was necessary to provide barriers to protect both animals and visitors and houses, cages, glass screens and cases recalling those of a museum rather than of nature. In the country zoo the barriers need be only of the simplest kind, preferably sunk ditches, so that nothing comes between the eye of the visitor and the animals. In the completely developed national park of the future the ordinary arrangements in collections of living animals might have to be reversed, and wired walks or travelling cages made for the visitors, not for the animals.

With regard to finance, in time each of the three should be self-supporting by revenues drawn from visitors. But the three should be interdependent. The country zoo will become a breeding ground and recuperating station for many of the animals of the town zoo, and the surplus stock of the national parks will become more and more the chief source of supply for the zoos of the town and of the country.

There were now many well-managed zoological gardens and parks in different parts of the world, and an increasing number of large areas for the preservation of the native fauna and flora in the United States of America, Canada, Australia and New Zealand, Asia and Africa, but to avoid diffuseness Sir Peter took the London Zoo, which celebrated its centenary two years ago, its new country annexe at Whipsnade, opened this year, and the Parc National Albert in the Belgian Congo, as examples of his theme.

He then described in detail the relations of animals in the London Zoological Gardens to the primary requirements of light, temperature and fresh air, general sanitation, diseases and parasites, and said that the Zoological Society had been pioneers in adapting the conditions to the new physiological knowledge of hygiene. He directed special attention to the experimental work done in the use of vita glass and artificial sunlight, and to the far greater importance of fresh air than artificially maintained equable temperatures. He paid a tribute to the research work done by his colleague, Dr. Joan B. Procter, who had died on the morning of September 20.

Sir Peter then explained the reasons which induced the Zoological Society to acquire a country estate at Whipsnade, and described the progress that had been made in developing the new scheme.

Zoological gardens in cities and in the country added to zoological knowledge and stimulated public interest in the beauty of wild animals. On the other hand, there remained the very important problem of preserving the wild fauna of the world. In 1912, when he had been President of the Zoological Section of the British Association at Dundee, he had devoted the greater part of his address to that subject. As instances of the rapidity with which wild animals might disappear he had then taken the examples of the American bison and the American Passenger Pigeon. In 1867 there were still millions of bison roaming over the plains and forests of North America, but in that year the building of the Union Pacific Railroad, the first great trans-continental line, had cut the herd in two. The southern half, consisting of several million

individuals, was wiped out between 1871 and 1874, and the practical destruction of the northern half was completed between 1880 and 1884. During the recent great war in Europe more complete destruction had fallen on the remnants of the European bison in the forests of Lithuania and the Caucasus and in the private preserves of Russian magnates. The American bison had been saved by collecting individuals from the remnants of the wild herds and from examples in zoological gardens and placing them in the national parks of North America and Canada. A European Bison Society had begun a similar task, but the difficulty was much greater as, so far as could be ascertained, only a few dozen individuals were still in existence. An instance of complete extinction of a species was the Passenger Pigeon. A little more than a century ago these birds existed in countless millions, and Audubon relates that for four days at a time the skies were black with the streams of their migration. In 1906 only five individuals were alive, all of them bred in captivity, and before 1912 the last survivor died in the zoological gardens at Cleveland. A similar destruction was going on all over the world, even at increasing rates, as the wilder regions of the globe were being opened up, and as the means of transport were quicker and more abundant.

Fortunately certain measures were being taken in most parts of the world to arrest this process. Close seasons were instituted, shooting licences insisted on, and individual species were placed under the protection of the law. Moreover, sanctuaries were being formed in increasing numbers. Without doubt much good was being done. But the efficacy of all such measures depended on the strictness with which they were carried out, and this was the more difficult to secure the more remote the regions were. A special difficulty arose in connection with game reserves such as had been established in many parts of Africa. Settlers coveted the land or complained that their farms and plantations were being damaged by the presence of the sanctuaries. There were many cases in which existing reserves were thrown open, suspended or abolished. The only safe policy for the permanent preservation of wild animals and plants was to dedicate areas absolutely for all time. The areas should be sufficiently large and should be surrounded wherever possible with a neutral zone, in which settling was not permitted and the wild animals protected.

In 1912 the best examples of such national parks were in the United States and Canada. Much more recently the Union Government of South Africa had turned certain reserves into the Krüger National Park of about 12,000 square miles dedicated for all time to the preservation of plants, animals and natural scenery. Still more recently, by the decree of the King of the Belgians, an area of 400,000 acres in one of the wildest and most beautiful parts of Central Africa, as yet unspoiled by man, has been made the Parc National Albert. Adjoining territory of still larger area has been turned into a buffer state, a game reserve under very strict regulations. The Parc Albert contains three of the finest volcanoes in the world, lakes, forest-clad mountains and valleys with a very rich fauna. Above all it contains gorillas, which, unlike those of West Africa, have not been persecuted by human beings, and still remain undisturbed in a territory they have occupied for hundreds of thousands of years. Gorillas are the anthropoid apes nearest to man in structure and psychology, and it is of supreme zoological importance that they should be protected for all time. Marching with the Parc Albert, but across the frontier in Uganda, is a small area of a few square miles of the same natural character, and occupied occasionally, or perhaps permanently, by the same race of gorillas. For several years the Zoological Society of London, the Society for the Protection of the Fauna of the Empire, have urged that this area be turned into a national park on the same lines as those arranged by Belgium. It is true that gorillas are protected by the Uganda Government and that recently the regulations have been made more strict. But if the shooting of other animals is permitted, the gorillas will be disturbed; if settlers should invade the area or prospectors find minerals in it, not only will the protection for gorillas inevitably cease, but by the destruction of a neutral area the sanctity of the Parc Albert will be prejudiced. The Governor of Uganda sees no objection to the transformation of the area into an inviolable park; the Belgian Government, through the Ambassador in London, has made a formal request that the step should be taken. But in spite of appeals from these and other sources the Colonial Office, on one pretext or another, has postponed giving the necessary instructions. Let us hope that before long this humiliation to our country will be removed.

(Published in *Journ. Soc. for Protection of the Fauna of the Empire.*)

THE CONSTRUCTION OF MAN'S FAMILY TREE.

BY

SIR ARTHUR KEITH, F.R.S.

SOMEWHERE about the year 1865 Ernest Haeckel, the young and enthusiastic professor of zoology in the University of Jena, did an unprecedented thing; he drew a family tree to represent man's relationship to the rest of the animal kingdom. It will repay us to review the circumstances which led Haeckel to make this bold attempt—the first of its kind—to represent man as a sprig on the great tree of Life. The essential circumstance was the appearance of Darwin's *Origin of Species* at the close of 1859; Haeckel was then a young medical man, aged 25, investigating marine forms of life in the zoological station at Naples. A subsidiary circumstance, still a very important one, was the appearance of Huxley's *Man's Place in Nature* in 1863. By then Haeckel was established in the University of Jena. Neither Darwin nor Huxley were so daring as Haeckel. Even in 1871, five years after Haeckel had published his *Generalle Morphologie*, with its wealth of pedigree tables, Darwin, in his *Descent of Man*, went no further than to infer that 'a member of the anthropomorphous sub-group gave birth to man.' As to the date of that event, he would not commit himself because of lack of evidence. It might have been 'as early as the Eocene,' he admitted; all he was assured of, on the evidence then available, was that 'in the upper miocene the great and small anthropoid apes had already diverged. Huxley in 1863 was equally cautious. He drew no pedigree. The utmost he adventured was the opinion that man had arisen by 'the gradual modification of a man-like ape or from the same stem.' As to the date of man's emergence he was caution personified. 'Time will tell' was all he said. But he was prepared to find the fossil remains of *Homo sapiens* in strata of pliocene or miocene date, or even in those of an earlier period. Where Darwin and Huxley feared to tread the young and daring Haeckel, pioneer and prince of pedigree-makers, stepped boldly in, and if he blundered, it is not his error which impresses us to-day but the amazing degree of his success. Where in the modern world is there a man with sufficient knowledge and courage to reconstruct the entire tree of life single-handed? Yet that is what Ernest Haeckel did between 1864 and 1866—just after he had attained his thirtieth year.

What made Haeckel so bold? It was this. The doctrine of evolution, as expounded by Darwin, presented itself to Huxley's cool judgment as an acceptable working hypothesis; to Haeckel's glowing and penetrating imagination it came as a revelation of reality. He immediately saw life as a great tree rooted deeply in the geological past with trunk and great branches dead, buried and fossilised, only the end-twigs peering through the surface of the earth into the present as living forms. Convinced of its reality he immediately set to work to reconstruct that tree. Only one branch of Haeckel's tree concerns us to-night—a branch on the extreme right of the tree—a branch which represents the order to which man belongs—the Primates. The terminal twig on the extreme right represents humanity (*Homo sapiens*). Haeckel's tree had a storm-blown appearance; its branches, under the stress of evolutionary winds, tended to grow in one direction—the most progressive branch of all—the human branch, occupying, as it should do, the extreme right. We are surprised to note how closely he has set the twig or branch which gave birth to the African anthropoids—the gorilla and chimpanzee—to that which eventuated in man; all three are made to emerge from the same terminal stem. When we follow this human-gorilla stem towards the left and downwards—that is, against the stream of progress and of time—we find shooting from it the orang twig; then, at a still longer interval, the gibbon branch; then a long way further to the left the old-world monkeys emerge, then those of the new world; ultimately, this primate stem, after moving far to the left, merges with the stem of the prosimiae or lemurs.

What were the facts which guided Haeckel in the construction of his great tree of life? Let us confine ourselves to his terminal primate branch. Why does he represent man and the African anthropoids as springing as terminal twigs of the same stem? It was because of the multitude of structural points they had in common. He presumed there was only one way in which this community of structure could be explained, namely, as an inheritance from a common ancestor. He did not believe that animals pursuing separate evolutionary ascents could come by a complex of identical structural modifications. In constructing his tree he made no allowance for parallelism or convergence. In brief, he fashioned his tree so that it gave a satisfactory explanation of the resemblance and differences which anatomists found when they compared men with great anthropoids, great anthropoids with small anthropoids or gibbons, gibbons with old-world monkeys, old-world monkeys with new-world monkeys, and new-world monkeys with the riff-raff which make up the lowest and oldest primates.

He found it necessary, in order that his tree might give a rational explanation of anatomical fact, to make another very important assumption, namely, that the evolutionary stages through which man has ascended from the lowest place to the highest place amongst primates were still represented by living forms. The human was the final and highest stage; it had evolved from a great anthropoid stage, now represented by the gorilla, chimpanzee and orang; the great anthropoids had arisen from a small anthropoid stage—the gibbons and siamang—being modified descendants of this ancient stage. Still older in type were the monkeys of the old world; the monkeys of the new world represented a still earlier evolutionary stage.

We must recognise, then, that Haeckel constructed his family tree merely as a working hypothesis to explain the distribution of structural characters in the higher primates. Modern students of culture are familiar with the principles which guided Haeckel in framing his pedigrees. As they trace the civilisation of modern Europe backwards in time they recognise a series of stages which links our culture with that of our palæolithic ancestors. But suppose nothing was known of these buried stages, could they not be inferred by a study of the culture of native and backward living peoples? It was by an analogous process of legitimate inference that Haeckel built up the stages which transferred the lowest into the highest form of primate organisation—stages which led from a half ape to a whole man. At a later date Haeckel counted twenty-six stages in the evolution of man, but at the date of which I speak he was content with ten. I need mention only three—the eighth or prosimian, the ninth or catarrhine, and the tenth or tailless (anthropoid) stage. It was Haeckel who first demarcated man's evolution into stages.

I have spoken of the family tree which Haeckel constructed to explain the structure of man, anthropoids and monkeys, as a mere hypothesis. How could he have converted this, a supposititious history, into a true history? Very simply; by finding the fossil remains of the missing links—many thousands in number; the links which joined man and the great anthropoids to a common ancestral series; the links which joined this upper series to that which gave birth to the small anthropoids; the links which joined the small anthropoids to the ancestral chain from which the monkeys of the old world sprang and the still older series which joined the old-world stock to that of the new world, and the new-world monkeys to the prosimiae. We have now a number of these missing fossil links to guide us in pedigree building, but Haeckel, in the year 1866, could cite very few of them. Neanderthal man had been discovered, but his relationship to races, past and present, was an unsolved problem. Fossil remains of a great anthropoid (Dryopithecus) and of a small anthropoid (Pliopithecus) had been discovered in the miocene deposits of Europe. Such discoveries assured him that both small and great anthropoids were in existence at this phase of the Tertiary history of the earth. Then in the eocene or earliest phase of the Tertiary period there were numerous fossil remains of very early kinds of primates representing prosimian or pre-monkey forms. Scanty as the geological record then was, it assured him (1) that true monkeys had not come into existence at the beginning of the Eocene—the first phase of the Tertiary period; (2) that both small and great anthropoids were in existence in the two later phases of the Tertiary period—miocene and pliocene; (3) there was no certain evidence of the existence of man until well into the Quaternary period, which in modern phraseology is made up of the pleistocene and recent.

Now no pedigree or family tree has any scientific value unless it is dated, that is, drawn against a background of time. Haeckel boldly drew his family tree against the background of time provided by the geological calendar of his day. He depicted

the human stem as breaking away from that leading on to the gorilla and chimpanzee rather more than half-way through the Tertiary period. He did not attempt to estimate the duration of geological periods in years, but in order that we may form some estimate of the antiquity of man or pre-man as depicted in Haeckel's tree—we may apply what is the most acceptable of modern estimates, that given by Dr. H. Fairfield Osborn. On the Osborn scale of reckoning 60 million of years are supposed to have elapsed between the appearance of primates at the beginning of the eocene and the full evolution of man in the present or Quaternary period. On this reckoning Haeckel gives the antiquity of man—that is the date at which the human separated from the anthropoid phylum—as some 20 or 25 million of years. He represents the great anthropoid phylum as breaking away from that of the small anthropoid deep in the oligocene, the small anthropoid parting from the monkey deep in the eocene, and the monkey stem from the basal stem just before the dawn of the Tertiary period of the earth's history. Thus, to transmute an early primate form into modern man has occupied nature's evolutionary processes an æon of time—some 60 million of years if we apply Osborn's reckoning to the family tree drafted by Haeckel. Looking at this first attempt in the light of the facts now at our disposal, we marvel at the accuracy of Haeckel's intuition.

By 1874 (*Anthropogenie*) Haeckel gave his tree of life a new shape. His original tree (1866) was flat topped, its branches streaming in one direction under the stress of the winds of evolution ; it was a lopsided tree. His new tree of life resembled a massive gnarled oak, with a single main trunk giving off many branches from each side and ultimately ending at the extreme top in a crown of twigs—the human family. Just under the top issued the anthropoid branches. Such a tree seems to me to reflect human vanity rather than zoological justice. It conveys a totally wrong impression of man's relationship to the animal kingdom. It leads the spectator to conclude that the tree of life has grown and groaned all through these past geological æons just to produce humanity as its topmost shoots, and that all forms of life are but abortive branches of the great human stem. This anthropocentric tree of life was introduced by Haeckel. It has still its advocates. And if we accept such a tree as a representation of reality, then it is but just to regard these branches which emerge just short of the top and represent anthropoid types of primates as abortive attempts at man-production.

I have dealt thus fully with Haeckel's family tree not only for the reason that it represents the first of human genealogies, but more especially because it has given me an opportunity of discussing the manner in which evolutionary pedigrees are drafted. We must now leave these early attempts of Haeckel behind us and note the chief changes which have had to be made from time to time as new evidence came to light. Haeckel lived to see many of these changes introduced ; he died in 1919 at the age of 85. First let me show you the pedigree which Dr. Eugen Dubois drafted of the highest primates in 1895. He wished to depict the position which he assigned to the fossil form of being he had discovered in Java to which he gave the name *Pithecanthropus erectus*. He adopted Haeckel's anthropocentric scheme and gave man's line of ascent the central place in his diagram. The central stem is represented at first by a Proceropitheque, the prototype or ancestor of the old-world monkeys. Then follows a stage represented by Prothylobates—the ancestor of the gibbons ; then the stem ascends to become the prototype of the great anthropoid apes. This, as it ascends, becomes the Pithecanthropic type, and finally the stem ends in the human form. Dubois originally regarded the fossil being he found in Java as a transitional form between the protoanthropoids and man, and hence gave his discovery a place on the direct line of human ascent. It is a remarkable fact that in the human pedigree which Haeckel drew in 1879 he postulated such an intermediate stage, and gave to this hypothetical ancestor of man the name Pithecanthropus—a representative of speechless man. Dubois was impressed by the many gibbon-like features of Pithecanthropus, and hence he made the stock of gibbons or small anthropoids diverge from the main or human stem high up, just below the emergence of the great anthropoid branches.

Some years before Dubois published his family tree (*Nature* 1895, vol. liii., p. 245) I had been dissecting apes and collecting published records in order to make a more complete census of the anatomical characters of the higher primates than had been done by my predecessors. Altogether I had dissections of some 300 animals at my disposal—gorillas, chimpanzees, orangs, gibbons, old-world monkeys and new-world monkeys. Records of human dissections were already plentiful. I made this extensive

census of anatomical characters to see if I could construct a family tree which would explain how man had come by his share of them, the gorilla, chimpanzee, orang, gibbon and monkeys by their respective allotments. I could find no schematic family tree which gave a clear-cut explanation of the distribution of anatomical characters, but I determined to be guided by the majority of characters, believing that the exceptions I encountered would find an explanation in a fuller knowledge of the laws of inheritance. After collecting data for eleven years I drafted in 1900 the chart which I show you to explain the mass of data I had then collected. Man is what he is because of his brain, but I found then and, what has happened since has convinced me that my surmise was right, that a study of the evolution of posture of body gives the clue, not only to the evolution of man, but to that of all the higher primates. There are an infinite number of ways in which animals with grasping hands and feet can climb trees. There are, however, only two main ways. Such an animal may carry its body prone or horizontal to the branch along which it runs. It is pronograde in its arboreal posture ; for such primates my friend—now the Hon. Patrick Duncan—proposed the name *Pronorachites*—' prone spines.' The other posture is quite different in all its implications. The animal climbs in an upright or orthograde posture. As it runs along a branch the body is held at right angles to the plane of movement. To such primates—or anthropoids—the name Orthorachites (upright spines) may be given. Orthorachites may be of three kinds ; in the act of arboreal progression the arms may play a more important part than the legs ; the gibbon and orang exemplify the brachiating mode of orthograde progression. Or the arms and legs may play a more or less equal part, as in the chimpanzee and as in the gorilla, although in the latter, especially in the adult male, the legs may be more active than the arms. There is the third possibility—where the legs play a more important part than the arms in orthograde life in the trees. It was in this latter way I supposed that an arboreal orthograde anthropoid had first diverged in a human-ward direction in specialisation of spine, leg and foot. It was on the trees, not on the ground, that man came by the initial stages of his posture and carriage.

I held then—and what has happened in the past thirty years has but confirmed my conviction—that a study of the gibbon provides the key to those who wish to understand the evolution of the great anthropoids and man. Haeckel was right when he gave the gibbon a separate position, placed between the old-world monkeys and the great anthropoids, yet well apart from both. The gibbon is a primitive old-world monkey whose body at an early date became transformed to serve the needs of the orthograde arboreal posture. But I reject altogether the Lamarckian explanation of how the transformation of the gibbon's body was brought about. It was not brought about by one generation of Catarrhine monkeys after another seeking to climb in an orthograde manner. Adaptation to posture implies fundamental changes in the whole organism, changes in the development of all parts connected with locomotion, changes in reflex nerve centres, in development and arrangement of muscles and in the shaping and growth of bones and joints. How these evolutionary changes we, in our present ignorance of developmental processes, cannot explain, but in estimating relationship of ape to man we must regard these postural modifications of structure as of the highest value.

Thirty years ago, when I was constructing a family tree of man and of the higher primates, the oldest fossil evidence we then had of the existence of a small anthropoid or gibbon-like anthropoid was in deposits of the upper miocene, and as the great anthropoid type was already in existence and was clearly derivable from the small anthropoid type, I saw that the origin of the orthograde posture as manifested in the gibbon must be ascribed to pre-miocene times, that is, in the upper or later part of the Oligocene. In 1910 a discovery was made in the deeper or older Oligocene of Egypt which caused me to extend the antiquity of the orthograde posture. Prof. Max Schlosser described from that horizon the teeth and jaws of a very primitive and small anthropoid ape—apparently an ancestor of the gibbons which he named *Propliopithecus*. Its teeth and jaws being so like those of the gibbon, we presume that the posture of this early oligocene ape was also orthograde. So I had to modify my original scheme by carrying the first stage in the evolution of man's posture backwards a good few million years, for on the Osborn scale 16 million years are allotted to the Oligocene period.

Now every serious student of human origin accepts the discovery of Propliopithecus, the forerunner of the gibbons, as an event of the highest importance. Propliopithecus provides a fixed basis for our speculations concerning the evolution of the higher

orthograde primates. Authorities such as Prof. Fairfield Osborn and Prof. Wood Jones, who exclude altogether the anthropoid type from the ancestry of man, are prepared to accept Propliopithecus as ancestor from which both the chimpanzee and man may have arisen. They carry the separation of the human from the anthropoid stock back to early Oligocene times, giving man, on the scale of reckoning I have adopted for this discourse, an antiquity of some 35 million of years.

The census which I made of anatomical characters showed me that man and the three living great anthropoids—the gorilla, chimpanzee and orang—besides agreeing in great bulk of body, shared so many anatomical features of a kind peculiar to themselves that I had to postulate a common ancestor for the big-bodied or giant group. It seemed to me then, and it still seems so to me, most improbable that man and the great anthropoids and man came independently by such a complex of characters. Increase in size of body, which in many orders of mammals has no deep significance, was, so far as concerned the higher primates, symptomatic of an extensive series of structural changes, one of which was an increase in size and complexity of brain. The great anthropoids had bodies which were eight times, or even twenty times, heavier than those of the small anthropoid. Great increase of weight entails changes in the manner of arboreal locomotion as well as changes in structure of body. In my scheme of 1900 I represented the great orthograde primates as being evolved from the small-bodied at the very beginning of the miocene period. The discoveries which Dr. Fourtan made in the lower miocene deposits of Egypt (1920) supported my inference. In deposits from this horizon he found jaws and teeth of two orthograde primates; one was of the size of a gibbon, the other had reached a size which must have been about half-way between that of the gibbon and of man. I therefore suppose that the great-bodied pre-troglodytes which afterwards diverged into anthropoid and human types came into existence in earliest miocene times.

This great increase of size of body which we infer affected the anthropoid stem in early miocene times was of critical importance. It indicated that a branch of the higher primates had entered a period of evolutionary plasticity and was undergoing profound functional and structural changes. It was in this plastic period that I suppose the human line to have separated from that of the great anthropoids. I cannot suppose that a change in a humanward direction could have taken place before miocene times. If we attempt to carry man's separation to an earlier date then we cannot account for man's structural resemblances to the great anthropoids unless we suppose that such resemblances to have been independent acquisitions. Dr. W. K. Gregory, in making a family tree of man and ape, constructed quite independently of mine and based on an altogether different mass of data, also brings the human stem from the anthropoid at the beginning of the miocene—practically the same date of emergence as I have given. On the Osborn scale of reckoning he and I give the human stem an antiquity of about 20 million years.

How long did it take this early miocene leg-using anthropoid to undergo these transmutations which entitled it to be reckoned no longer anthropoid but human? In 1900 there was only one fossil form which gave assistance in solving this problem, namely, the man of Java, Pithecanthropus. So far as concerns his lower limbs and manner of progression, Pithecanthropus was entirely human. The human posture was completely evolved before the end of the pliocene period. We thus allow two geological periods, the miocene and pliocene, covering a period of 18 million years, for the evolution of the human characteristics of foot, leg, thigh and pelvis. Besides the skull of Pithecanthropus we now know those of two other forms of early pleistocene man, Eoanthropus (Piltdown man) and Sinanthropus (Peking man). By the end of the pliocene period, therefore, the human stem had been in existence long enough to have broken up into many divergent branches, but at present we are still uncertain whether or not they represent the ancestors of any race now living. Hence in drafting our family tree we trace the modern races of mankind to an early pleistocene ancestor not yet discovered. If, however, man's lower extremities and posture and progression were fully evolved by the end of the pliocene period, it was otherwise with his brain. When we compare the brain casts of those early pleistocene representatives of man with their successors in the latter third of the pleistocene period, we see that in this apparently short period the human brain must have undergone a great increase of size, complexity and inferentially, in power. The pleistocene period, to which the Osborn scale allows a million years, saw a marked increase of man's brain power. In making my original draft of man's family tree I adopted, quite unconsciously, Haeckel's method of indicating evolutionary progress by carrying the main line of

advance towards the right-hand side of my chart. The line of advance culminated in the human stem in the extreme right. Like Haeckel, I recognised certain definite stages in the advance towards the human form—a pronograde stage, still represented by old-world monkeys; an early orthograde stage, still perpetuated by gibbons; a later orthograde stage, represented now in the bodies of the great anthropoids; and lastly the final orthograde (or plantigrade) posture represented by man. If we accept such a succession of evolutionary stages then we can give a rational explanation of man's structure and posture of body; we can explain the existence of great anthropoids, small anthropoids and pronograde apes and man's structural relationship to them. We can also explain, so far as we have discovered them, the geological records of extinct types of apes and man. In brief, a family tree represents a working hypothesis; it must explain—and be the only possible explanation—of how man, anthropoid and monkey, have come to be constituted as we now find them.

Now I do not claim that my formula explains all the facts. Prof. Wood Jones has drawn attention to the persistence of many primitive features in the human body which have disappeared from those of anthropoids and of monkeys. On the other hand, anthropoids and monkeys have retained to even a greater degree a varied assortment of primitive features which have disappeared from man's body. The explanation of these anomalies I would seek for, not by modifying the family tree I have shown you, but by a better understanding of the laws of inheritance. Living animals are structural mosaics: the descendants of a common ancestor may change—or revert—in one set of parts while a cousin form changes in quite another set. The gorilla reproduces in its liver the lobulation of monkeys, while the orang has a liver unified to an ultrahuman extent.

During the past four years the final form which should be given to man's family tree has been very actively and profitably debated by our colleagues of the United States. I can best bring home to you the chief matters now in dispute by showing in quick succession three recently constructed human pedigrees, all of them from America. The first represents the conception which Prof. W. K. Gregory, of the American Museum of Natural History, has based on thirty years of inquiry.[1] Prof. Gregory has introduced an improved method of charting man's pedigree; nevertheless, it is but a modification of Haeckel's original scheme which represents man as the most changed, the most highly evolved of all primates. The great anthropoid stock is depicted as diverging into three branches at the very beginning of miocene times, one branch leading towards man, another towards the African anthropoids—the chimpanzee and gorilla—and the third towards the line which ended in the orang. The extinct anthropoids of the upper miocene and lower pliocene are placed on or near one or other of these three divergent branches. We are particularly interested in the place given in Prof. Gregory's scheme to that extinct primate discovered at Taungs by Prof. Dart and named by him *Australopithecus*. Prof. Gregory makes Australopithecus spring from the chimpanzee stock late in the miocene, and gives this extinct South African anthropoid the nearest place to man, the chimpanzee coming next and the gorilla third in degree of proximity to man. On the whole, I consider the place given in this scheme to Australopithecus to be the best that can be assigned to it in the light of our present stock of knowledge. The idea which underlies Prof. Gregory's pedigree of man is the same as that which guided Haeckel in his first scheme and which influenced me in my attempt. All three of us have found that in order to derive man from one of the many early eocene primates—the tarisoid family is now accepted as the most likely early eocene progenitor—it is necessary to suppose at least five stages in man's ascent—a tarisoid stage, a small monkey stage, a small anthropoid stage, a great anthropoid stage and, finally, an early human stage. Neither Prof. Gregory nor I can conceive how an early eocene primate could arrive at man's estate except by passing through this or a similar series of stages.

Another human pedigree published by Dr. Adolf H. Schultz of Johns Hopkins University at the close of last year (*Human Biology*, September 1930) is well worthy of consideration. For over twenty years Dr. Schultz has been investigating the development and growth of anthropoids and monkeys, and has thus come into possession of a large mass of new data. He has drawn a pedigree of man and ape to explain his observations. The central position in his scheme is occupied by the African anthropoids; their stem emerges from a generalised small anthropoid.

[1] 'How near is the relationship of Man to the Chimpanzee-Gorilla stock?' *The Quarterly Review of Biology*, 1927, vol. ii., p. 549.

Almost from the base of this central anthropoid stem issues the line which led to the gibbon, diverging and ascending to a position far to the right of the African anthropoids. From the same central anthropoid stem, but a little higher up, emerges the human stem ; it diverges and ascends to the left, so that ultimately man takes up a position as far to the left of the gorilla and chimpanzee as the gibbon holds on their right. Dr. Schultz regards the gibbon and man as contrasted forms, possessing evolutionary pedigrees of nearly equal antiquity. The gibbons represent an arm-using or brachiating orthograde primate ; man represents a leg-using or *cruriating* orthograde primate. Big-brained man has undergone many and great structural changes, while the small-brained gibbon has been relatively conservative and undergone specialisation of a minor kind. There is much to be said in support of Dr. Schultz's conception.

There are many other recent schemes showing man's origin and relationship which I should have liked to have discussed with you had time permitted. There is, in particular, an instructive pedigree outlined by Dr. C. Tate Regan,[2] but I must limit myself to a brief discussion of the family tree[3] which Dr. Henry Fairfield Osborn has drawn up to represent his conception of man's origin. Dr. Osborn's tree differs from those I have so far shown to you in one important respect. Its trunk, which is rooted deep in the oligocene, divides almost at once into right and left stems, the right stem breaking up, as it ascends, to give birth to the known forms of anthropoid ape, both extinct and living. The other main stem ascends to the left and, as it rises towards and enters the pleistocene, branches into the known forms of man, living and extinct. Dr. Osborn thus excludes an anthropoidal stage from human ancestry. He supposes that man and ape diverged from a common ancestry in the earlier half of the oligocene period. Dr. Osborn's opinion will carry weight with every serious student ; he has had a richer experience than anyone in tracing the evolutionary histories of the higher mammals The likenesses which man bears to the great anthropoids, in brain and in body, he holds, are to be regarded as independent acquisitions ; he has noted similar instances of 'parallelism' in many lines of mammalian evolution. He has examined anthropoids living and extinct, and nowhere finds a form sufficiently generalised to serve as a starting-point for human evolution, until he passes back to the early eocene primates of the type represented by *Propliopithecus*.

There is no need for me to criticise Dr. Osborn's theory of man's evolution ; that has already been done by Dr. W. K. Gregory. I would merely say this, that the Osborn scheme is framed not to explain the wealth of facts already at our disposal, but to account for certain ideals which his imagination has fashioned out of a wide and ripe experience. For my part I can see no way of accounting for man's structure, his posture and manner of progression, except by supposing that in his evolution he has passed through a small anthropoid stage and then a large anthropoid stage. These stages we know of ; we have no evidence of such stages as must be postulated in Dr. Osborn's theory.

Thus I end by giving my support to that form of Man's Family Tree which was drafted by Haeckel sixty-five years ago. Knowledge has greatly increased since then ; but even when this new knowledge is incorporated as has been done by Gregory, Schultz and many others, the scheme is essentially that which Haeckel drafted under the inspiration of Darwin and of Huxley. We are all agreed that anthropoid and man have a common ancestry ; it is merely the degree of relationship which is in dispute.

[2] The Evolution of the Primates. Annals & Mag. *Nat. Hist.*, 1930, September 10, vol. vi., p. 383.

[3] Published in *Long Island Medical Journal*. October 1927 ; also in *Palæobiologica*, 1928, vol. i., p. 193.

A RETROSPECT OF WIRELESS COMMUNICATION.

BY

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As one gets older people seem to think that one's duty is to be an historian of the times during which one has lived. Unfortunately I have not been trained as an historian, and am therefore incompetent to do more than just trade upon my reminiscences, which are liable to be rather one-sided and not to satisfy the conditions for serious and reliable history without prejudice or favouritism. It has been suggested that I speak on the History of Wireless Telegraphy, under the title 'A Retrospect of Wireless Communication.'

Looking back, then, over my lifetime, the first item to attract my attention was a paper on 'Transient Currents' written by Lord Kelvin (as Sir William Thomson) in 1853, wherein he gave the theory of electric oscillations in a masterly manner considering that the idea of self-induction was not then born. He knew, however, that an electric charge could be stored in a condenser, after the same fashion as energy is stored in a bent or coiled spring, so that the condenser received and stored up electric energy, which it would subsequently give out when released. That was the first step. He knew, moreover, that the discharge would constitute an electric current, and that every electric current was surrounded by a magnetic field, which would confer upon it something akin to inertia or momentum; so that, like a loaded spring, it would not only recoil when released, but would overshoot the zero mark and reverse of itself, swinging like a loaded pendulum first on one side the zero, then on the other; so that the discharge was not a flow in one direction only, but an oscillating or alternating flow, first in the positive, then in the negative direction. The magnetic field would thus prolong the discharge until the energy was finally wiped out; and the spark, if examined in a rotating mirror (as Feddersen examined it twenty years later) would be seen to be not a single luminosity, which would be drawn out into a uniform band, but would be a succession of luminosities or a beaded band, each band corresponding to a half swing. Kelvin did not attempt this experimental verification, but he went on with the theory.

The elastic recoil or strength of the spring varies inversely with the capacity of the condenser. The smaller the condenser, the stiffer the spring; so that with a large condenser the oscillations would be fairly slow; not really slow, but something comparable to a thousand or a hundred a second, something which could be made to give a musical note, if the capacity were very large. I exhibited this musical note at the Royal Institution many years afterwards in what I called a 'whistling spark.' The noise of such a spark, instead of being a crack, was a whistle, whose pitch could be brought down to reach the tones of the voice, and indeed lower still. The rate of swing depends not only on the capacity of the condenser; it depends also on the load or inertia of the discharging circuit. It depends on what we now call self-induction, but which then Kelvin spoke of as 'the electro-dynamic capacity of the discharger.' There was the electrostatic capacity of the charged condenser, and the electrodynamic capacity of its discharging circuit. The two co-operated so as to produce the swing, and the rate of swing depended on both equally, and could be calculated exactly. This theory Sir Richard Glazebrook and I subsequently verified, many years afterwards, in the Cavendish Laboratory, Cambridge, in the 'nineties of last century, the result being published in the Stokes Memorial Volume.

The discharger not only had magnetic induction, it also had resistance, and Kelvin's theory showed that if the resistance was above a certain amount, the oscillations would be quenched prematurely. There was a critical resistance at which they would be

wiped out, so that the recoil would be dead beat, just returning to zero and staying there. That was the quickest possible method of discharge. But there would then be no oscillations. If the resistance were still greater than that, then the discharge would take longer to reach zero ; it would degenerate into a leak, or at first into a sort of intermittent current, returning towards zero spasmodically in jerks all in the same direction.

Now a flash of lightning is the discharge of a condenser, that is, the discharge of a store of electricity in the cloud ; and Dr. Simpson the eminent meteorologist has shown that the resistance to a flash of lightning usually exceeds the critical value which would make the discharge dead beat, and still more exceeds the value which would permit it to be oscillatory, and in fact makes it intermittent ; so that the lightning, if photographed, is seen to be a series of spits, succeeding one another very rapidly, and giving a jerky current all in one direction. Lord Kelvin's theory of 1853 provided not only for the oscillatory discharge and its dead beat condition, but also for the leak likewise. If the resistance was enormous then the charge of the condenser would simply leak away, the law of discharge being then exactly like that of a hot body cooling. For heat has no inertia, and therefore has no tendency to make oscillations. The hotter the body the more rapid the leak, that is to say, the cooling process : and the return to zero is asymptotic, that is to say, the potential falls down an exponential or logarithmic curve, getting slower and slower as it comes nearer zero, and gradually approaching it, taking theoretically an infinite time actually to attain it. That theory of Lord Kelvin's about electric oscillations or what he called transient currents dominated for me the nineteenth century, and was illustrated by innumerable experiments, at different times in the century. In the year 1889 I lectured on the Leyden jar at the Royal Institution, and showed many of these effects.

But we did not know then that there was another reason for reducing the time during which the charge continued to oscillate. It was killed not only by the resistance of the circuit, but by a certain proportion of energy radiated away into space. We did not know that there was any such radiation, nor did Lord Kelvin. We knew, or might have thought, that such radiation was possible, by the analogy of a tuning-fork. A struck or excited tuning-fork gives sound vibrations which die out at a certain rate. They die out partly because of the resistance of the steel of the fork, and partly, indeed chiefly, if the fork is mounted on a sound-board, by reason of the radiation which is thrown out into the air. A genius might have surmised that, as the tuning-fork vibrates in air, so the discharge of a Leyden jar or other condenser, being a vibration in the ether, might possibly carve the ether into waves and emit energy in that way. That is what happens, but no one suspected it for a long time ; they did not know that the conditions for ether waves would be satisfied by an electric discharge. We had no sense for such waves, and could not tell that they were being emitted, even when we made the experiment. We were in the condition of a deaf person striking a tuning-fork or a bell. If you could not hear the sound emitted by the fork you would not know that there was any ; and you would certainly not experiment on the waves, measure their wave-length, and utilise them for purposes of communication.

The first to show that an electric discharge would generate such waves, that is to say that an alternating or oscillating current would lose a certain fraction of energy to the ether at every swing, was George Francis FitzGerald, who in the year 1880 examined the question mathematically, communicating it to Section A of the British Association ; and in 1883 followed it up with a further communication, in which he calculated the actual amount of energy lost in a second by a given condenser and self-induction. This was a great feat, and I will write FitzGerald's result on the board, for it is used to this day. It showed that a short wave oscillator radiated much more vigorously than one that vibrated slower, that the radiation power, in fact, depended on the fourth power of the frequency, other things being equal.

The radiating power of a current i oscillating in a time T in a circle of area πa^2 is

$$\frac{8\mu\pi^4}{3T^4c^3} \cdot \left(\pi a^2 i_0\right)^2$$

which simplifies to this in terms of wave length

$$8\times 10^{12}\frac{(\pi a^2 i)^2}{\lambda^4}.$$

His theory shows why an ordinary alternating dynamo does not radiate appreciably; it does radiate, like every alternating current, but if its frequency is comparable to 100 a second the amount of energy lost is next to nothing. To get at anything like efficient radiation you must have an alternating current of a million a second or more; and if you could only work up the oscillations till they were five hundred-million-million a second (which sounds preposterous) then you would have the means of detecting them. They would be sufficiently rapid then to excite our sense-organ, the eye, and give us the sensation of a strong yellow light; for what we call light is just an ethereal vibration excited by an electric oscillation of this extravagant frequency.

Still we didn't know how to produce these oscillations, and still less how to detect them. FitzGerald virtually said that the oscillations were there whenever a Leyden jar discharged. On what ground was he able to make that assertion? How did he know that an electrical oscillation would generate ether waves just as a tuning-fork generates sound waves? He only knew that on the strength of the work of a great genius, James Clerk Maxwell, who in 1865 communicated papers to the Royal Society, and to the British Association a year or two later, giving the result of his mathematical theory of Faraday's views on electromagnetic phenomena. Maxwell's equations expressing electric and magnetic relations were, and still are, of the utmost importance. They are not expressed in the simplest possible form, but they are remarkably complete. Simplification came later. But as a foundation for all the work that followed during the century, Maxwell's equations are the basis, and shine with undiminished brightness down to the present day.

This leads me to make a digression on the work and methods of mathematical physicists. Their plan in studying any phenomenon is to bethink themselves of what is the fundamental fact or process underlying it. They express that process in what to them is the simple form of an equation, and having written down equations appropriate to each aspect of the phenomenon, they proceed to combine these equations according to certain rules, the rules of pure mathematics, and deduce the consequences. It is not a process that comes naturally to ordinary people; indeed, they find a difficulty in following it. When they do follow it, they are apt to be lost in admiration, first for the insight which enabled them to express the fundamental laws in that tractable form, and next for the skill with which the forms have been manipulated, so that results could be interpreted which might subsequently be put to the test of experiment and thus verified. Verification is always necessary because, though the theory may be accurate as far as it goes, it never goes all the way, and it may fail in not going far enough. A complete theory of any phenomenon would have to take all the universe into account, but no one aims at such a complete theory: they take the most essential features of what is happening and ignore the rest. It takes some genius to perceive what the most essential features are, and to judge whether the other things may be ignored or not. When the theory fails to be verified in practice it means either that some error has been made in the calculation, or, more probably, that something has been ignored which ought not to have been ignored. Thus, for instance, to take a trivial example.

Prof. Tait, the great mathematician of Edinburgh, calculated the trajectory of a golf ball, taking into account a good number of the causes governing it—the impact of the head of the club, the inertia and elasticity of the ball, the resistance of the air, the force of gravity, and so on. His first theory gave him a maximum range which no one, however skilled, could hope to exceed. Then, as is well known, his son, Lieutenant Tait, a skilled golfer, exceeded it. What did Tait do? He didn't abandon the theory; he perceived that something more must be taken into account. What he had ignored as unimportant was the spin on the ball. A ball, when struck except in a line exactly through its centre, will not only move forward but will be set spinning. Everyone knows the effect of spin on a billiard ball. A skilled player purposely puts it on by the way he strikes the ball; it will then rebound from the cushion or from another ball in a way different from what it would if it were not spinning. A spinning ball might move in a curved line. But then a billiard ball is rolling on a table, a golf ball is not. Still, it may be rolling on the air through which it is moving. Tait perceived that the spin must not be ignored, but must be fully taken into account. He remodelled his theory, writing down some more equations to take the spin energy into consideration. He thus made a more complete theory, which led to curious results, most of which have now been verified by experiment. The practice and the

theory agreed to a sufficient approximation, and the theory is then said to be complete. I am only taking this as a typical instance of the way in which mathematics is applied to physics, to secure in this instance a trivial result. Of course, a mathematician might have taken the whole into account at first, and then the theory would have been complete from the beginning, and there would have been nothing to correct afterwards.

That was the kind of way in which Newton proceeded. When he gave his theory of astronomy based upon gravitation he at first took the heavenly bodies first as particles, then as spheres, and thus arrived at a first approximation to the theory of their motions. But he knew that the earth itself could not be a sphere, because it was spinning on its axis; it must be an oblate spheroid. That had never been observed, but Newton predicted that it was so, and proceeded to trace the remote consequences of the shape of the earth. He found that it accounted for the precession of the Equinoxes, which had been known as an empirical fact to the Ancients. Copernicus had said that precession must represent a conical motion of the earth's axis; but no one knew any cause for such a conical motion. Newton with his extraordinary genius perceived that a conical motion, very slow, and taking thousands of years for its revolution, would be the exact result following from the pull of gravitation on an oblate spheroid. A spheroid would not act as if all its mass were concentrated at its centre; it would be more complicated than that. He was not deterred by the complications, but worked them out and completed his theory.

Another thing that had been ignored in the first view of astronomy was the size and plasticity of the bodies; they were treated as particles or rigid bodies, and this gave the first approximation. But, obviously, the earth is not a particle but a body of considerable size, so that some parts of it are appreciably nearer the sun or the moon than are the parts at the Antipodes. Newton took the size into account, and thus was able to show that anything yielding or mobile on the surface of the earth, like the ocean, would have a motion distinct from the rest of the earth to a slight extent, and would go through an oscillation periodically. This oscillation of the water on the earth he perceived would account for a phenomenon that had been known from antiquity, but had never been explained, namely, the Tides. He completed his theory by working out the tides in all their main detail, leaving it to others to show how great an effect tidal phenomena had had on the evolution of the universe.

This has been recently extended and applied by Sir James Jeans to the production of a solar system. He has shown mathematically that if a visiting sun entered our neighbourhood, coming within a reasonable distance of our sun, it would excite tides upon the sun, which might increase to such an extent as to throw out an explosion or protuberance. The history of this he followed up, and showed that it would presently aggregate into round bodies revolving round the earth, the bigger ones in the middle of the protuberance, the smaller ones at either end, and thus provide the sun with a system of planets, on one of which we happen to live. Well, that is a further development of tidal theory, taking into account all the possibilities, or at any rate such of them as a genius is able to consider relative, and giving an idea which at present seems likely to hold against adverse criticism about the origin or creation of the earth and other planets. This again is a digression, and I do not see how the theory is to be verified by experiment. All this development could not be done by one man; the genius who made it possible is Isaac Newton. The further developments of his theory were left to posterity.

So it was also with Clerk Maxwell. He wrote down some equations which expressed what Faraday had long brooded over as the electric field, regarding it as distinct from matter and existing in the ether of space around the charged body. He also wrote down another set of equations expressing the magnetic field surrounding a current. And then he began to combine these, so as to see what an electromagnetic field would be like, that is to say, a field which combined an electric displacement with a magnetic whirl. Do not suppose that this was an easy thing to do, but Maxwell did it, and found (possibly to his surprise, possibly to a satisfaction of his instinct in that direction), that the equation he now got, a differential equation of the second order, was one that was familiar to him and to other mathematicians, namely, the equation to a wave; that is, a disturbance periodic in space and time, which advanced through space at a certain rate. This rate was expressed by an electric and a magnetic constant of the ether, which were immeasurable; no one knows to this day how to measure them. But the rate of propagation of the electromagnetic wave did not

require the separate constants to be measured, nor need we know what each was; only their product entered into the expression. Experiments made abroad, in Germany, had indicated a way of determining this product; Maxwell bethought himself of another method, and proceeded to put it into practice in the laboratory of King's College, London, where he was then a professor. He performed the experiment, worked out the result, and obtained a speed for the transmission of electromagnetic waves very close to the velocity of light. It looked as if ether waves were just what we had been using all along for optical experiments and for arousing our sense of vision. We had been discussing whether ether waves were possible; they were familiar, only we didn't know they were electromagnetic.

All manner of theories of light had been tried in the early part of the nineteenth century—very ingenious theories, depicting the ether as a kind of elastic solid or jelly, in which the vibrations travelled at an immense pace. But none of these theories had been satisfactory. They covered the ground to a great extent, but they failed sooner or later. There were things we could not account for by any elastic solid theory. But Maxwell's theory that light was not mechanical but was an electromagnetic phenomenon, that its laws were ascertainable by electric and magnetic experiments, was a tremendous eye-opener. His book was published in 1873. The president of Section A of the British Association called attention to it, and we young people were all agog to understand this theory better, and if possible to verify it by actually producing ether waves electromagnetically. That was what set FitzGerald to work, and I have given you an indication of his results.

I also set to work experimentally, and tried to produce the waves by the discharge of Leyden jars. No doubt I did produce them—that was easy enough; the thing was to detect them. The eye is useless for waves measured in metres; it can only deal with the excessively rapid vibrations that constitute light. We needed what Lord Kelvin called 'an electric eye.' We worked mainly with closed condensers—that is to say, things of which the opposite plates were near together—and tried to see if there was any sign of waves running along wires which were attached to such discharging condensers. In 1887 or '8 I got the evidence in the form of nodes and loops characteristic of ether waves, reflected back on themselves at the terminal of the wire. But Heinrich Hertz in Germany, though not himself seeking to verify Maxwell's theory, which had not attracted much attention on the Continent, but making experiments on the way in which electric force streamed out from a discharging conductor, arrived at a sensational result. He did not work with closed circuits. He took two plates, like the two coats of a Leyden jar, separated from one another as far as possible, and joined by a wire: in fact, he made what we now call a Hertz vibrator. It can hardly be called a condenser, but still it has capacity and self-induction, that is, the two ingredients necessary for an oscillation, and when the two surfaces were charged oppositely and sparked into one another, oscillations were set up, and waves were generated, as FitzGerald had perceived they would be. They were generated in space, however, because the electric field was spread out in space as well as the magnetic. They had such energy, these waves, that when they fell upon a conductor they caused it to emit little sparks. Such a thing as that we experimenters had not imagined possible. We had never thought that a luminous field would be strong enough to excite sparks when absorbed. FitzGerald might have thought of it if he had interpreted his expression in terms of energy numerically. He had not done that, none of us had done that, but he perceived the strength and beauty of Hertz's result, and in 1888, at the British Association Meeting in Bath, he called the world's attention to the fact that Maxwell's electromagnetic waves had at last been produced; not only produced, but detected, and detected by their extraordinary amount of energy sufficient to emit sparks.

After that, progress was rapid. Hertz's discovery was first understood and made notorious in this country. It never caught the ear of the public, it was not taken up by the newspapers, but it could not fail to arouse attention through the whole of the scientific world. Hertz showed that Maxwell's theory would account for his radiation in every detail; he made a map of the process by which the radiation was generated in an electric oscillator, that is, he mapped out the lines of force during every phase of an oscillation—the beginning, the quarter of an oscillation, half of an oscillation, three-quarters, the complete—and these maps of lines of force were published in *Nature* when I translated his paper into that journal in February 1889. See vol. 39, p. 451. They could be shown in action on a kinematograph.

The only method of detecting them at first was the scintillæ which they produced. I found another means of detecting them by two knobs in loose contact in the circuit of a battery and galvanometer, or again in any form of loose joint and a telephone. This was the coherer principle, subsequently made more practicable by Branly in France, who found that the resistance of the metal coating smeared on paper, or a tube of iron filings, fell suddenly when a spark was taken in its neighbourhood.

Now the era of scientific discovery was nearly complete. The rest was what happens when any application is made of science on an extensive scale. A multitude of ingenious inventors combined their ingenuity and experience to apply the process on an engineering scale and to improve it out of all recognition. In 1894 I showed that Hertz's waves, combined with a Branly detector, could be used for sending and receiving messages in the Morse code by the emission and detection of waves from an electric oscillator, a signal or series of waves being emitted and detected at every spark.

About the same time, or soon after, Prof. Righi took the matter up and, in Italy, Senatore Marconi began applying the same process privately in his father's garden. In 1896 he came over to this country with an introduction to Sir William Preece, chief engineer to the Post Office, aroused his interest and enthusiasm for this method of signalling, and secured his co-operation. No doubt he encountered many difficulties, not only scientific and engineering but also financial; but he persevered and won through, and to him must be attributed the great success of wireless telegraphy. Before his patent was published, I perceived that something more would be wanted; that as stations multiplied there would have to be selection, and that tuning was necessary, not only to give selection, but also to give sufficient sensitiveness. One station could be worked upon its own wave length, by a receiver with capacity and self-induction attuned to that rate of vibration. Such a receiver would be very sensitive to one length of wave, and would exclude all other waves. This was patented in 1897, and was regarded afterwards by the Courts as the bottom patent for Tuning; it was extended by Lord Parker for a total period of 21 years, and for the last seven years was purchased by the Marconi Company. Other improvements were made, too numerous to go into. Marconi's special kind of radiator was an elevated aerial connected through a spark gap to the earth. He was thus able to reach great distances, because the waves oscillated in a vertical plane, so that the electrical vibrations were not wiped out, as they might have been if they had been horizontal, by the resistance of the earth and the sea-water over which they went.

Then came another striking discovery, which must be credited to Mr. Marconi working with unexampled energy on an extensive scale. He arranged for a large sending station in Cornwall, and travelled across to America to see if he could hear the signals in Newfoundland. I imagine that the scientific world must have been against him in this enterprise, since the waves could not penetrate the substance of the earth, and could not apparently travel round it to reach America. They would apparently travel in straight lines. But enterprise was rewarded, and the signals were heard: only three dots signifying the letter S; that was the arranged thing to be sent. It was enough: it began the series of Transatlantic communication. There was something in the earth's atmosphere, an upper layer postulated by Heaviside, as an effect due to the solar radiation in ionising the upper air, which caused the upper air to act as a kind of mirror, and make the whole earth into a whispering-gallery, so that waves impinging on the layer no longer went straight, but curved round until they reached the Antipodes; so that waves sent out in this country could be heard ultimately all over the surface of the earth. When I say 'heard,' the waves could not be heard; they make no impression on our senses until they are received by a suitable and attuned apparatus, when the high frequency electrical disturbance is transmuted into the low frequency mechanical disturbance that we call sound. No sound travels from the distant station to the receiver, nothing but ether waves which travel with the velocity of light; so that they reach the whole earth simultaneously. But at the receiving station they are converted into sound energy, and then operate on a telephone.

So far all signalling had been of a spasmodic or discontinuous character. A short series of waves was emitted by each spark, and it was by a succession of sparks that messages were sent. There was no continuity in the waves themselves. But many people perceived that it would be an improvement if, instead of generating a jerky series of independent trains of waves, we could generate a continuous wave at the

sending station, so that the signals might consist of modifications or modulations in its amplitude, which modulations might recur with a frequency of their own after the fashion of group waves. To use an acoustic analogy, the method hitherto in vogue had been analogous to signalling by strokes on a bell ; whereas what was aimed at was to get something more like organ notes, which would be continuous except when modulated by some kind of keyboard.

So far as I know, the first method of achieving this was the outcome of an experiment by Duddell with an electric arc as part of a tuned circuit. The circuit had a capacity and self-induction in series with the arc. The arc acted very like the blast of air on the lip of an organ pipe, giving an irregular flutter or disturbance which might be made periodic by a suitable resonant cavity, the resonant cavity or organ pipe itself being represented in the electrical case by the self-induction and capacity, that is, by the inertia and elasticity of the circuit. So long as the circuit was not interfered with, the note emitted was of a uniform tone ; but by means of a key the self-induction or capacity could be varied, and thus the note emitted could be changed. In that way with a few keys Duddell was able to carve the continuous wave into an imitation of the National Anthem or any other well-known tune. Duddell's arc, however, only responded to slow vibrations or long waves ; a big self-induction and capacity were necessary, and the arrangement was not applicable to the extremely high frequency needed for an effective sending station.

Poulsen, however, improved the arc by immersing it in hydrogen gas under various pressures, until he had got an arc of really high frequency, which he patented in 1903, and thereafter used it for generating continuous waves of a frequency such as would generate radiation.

Then came the question of receiving such waves. The thermionic valve was known as a rectifier, that is to say, it transmitted a current in only one direction, because it conveyed the current by the flight of electrons in a partial vacuum. Every vacuum tube acts as a rectifier, since it only transmits the current in one direction. Fleming perceived that this rectifying action of a partial vacuum could be employed as a detector of ether waves, and so patented the vacuum valve as a detector in 1904. It was used at first for the discontinuous system of spark signalling. The unrectified pulses were far too rapid to affect a galvanometer, or even a telephone ; but when rectified, so that only half of each wave was employed, the pulses could act in groups, and each group could cause a sound in the telephone, so that if the groups followed one another in regular succession they could cause a musical note whose pitch depended on the frequency with which the groups succeeded one another.

To get the groups out of the continuous wave Fessenden devised the heterodyning system of receiving, that is to say, he superposed on the received wave another wave of nearly the same frequency, so that it would beat with the first. The actual vibrations of neither wave can be heard, but the beats, which represent the difference of the frequencies, are much slower than the generating waves, and therefore come within the range of audition. The beats may, for instance, succeed one another at a thousand a second, whereas the generating waves had a frequency of a million. By this combined plan of heterodyning and rectifying a modulated continuous wave to be received, no matter how complex the modulations were, it was possible to superpose upon a carrier wave the modulations of a human voice applied to it by a microphone ; and then these complicated modulations could be received at the distant station, and the tones of voice reproduced. This was the beginning of wireless telephony, which depends upon emitting and receiving the modulations of a continuous wave.

But the amount of energy received at a distant station was small, and accordingly the voice was very feeble. But De Forest introduced a grid into the valve, giving it three electrodes instead of two, and supplied the valve with a local high tension battery, so that a stream of electrons travel from cathode to anode, passing the grid on the way. If the grid was now supplied with the pulsations of the received wave it would sometimes be positive and sometimes negative in potential. When it was positive it would help the electrons up on their way ; when it was negative it would repel them and beat them down. Thus it stood in the middle of the traffic like a policeman and regulated it, sometimes helping it on, sometimes stopping it. But the energy of the traffic does not depend on the policeman ; he merely regulates it. So it is with the grid. The energy of the triode valve is determined by the local battery, and may have any value you please. But the regulation of it, that is, the

modulation on which signals and voice depend, are determined by the grid, which carves the otherwise continuous traffic into an intermittent stream, in accordance with the fluctuating potential of the grid. So that, although the grid is supplied with very small power, its influence in regulating the traffic is considerable ; and the power derived from the valve is thereby regulated in accordance with the received wave, but has a power depending only on the local battery. It acts, in fact, as a relay, putting fresh energy into the disturbance, but not otherwise interfering with it. The electrons are so docile that they follow every fluctuation with precision, so that every feature or tone of the voice is accurately reproduced, however much it be magnified. That is the beauty of the three-electrode valve : it magnifies without appreciable distortion. There is no distortion in the Ether of Space, and accordingly the feeble fluctuations imposed on the wave by the voice at the sending station can travel any distance across space without injury, except that they become weaker ; and then these residual fluctuations can be magnified up in amount, still retaining their features, until they are capable of actuating a loud-speaker and being heard all over a hall. If the magnification is being pressed to extremes, a little distortion does enter in, not because the ether has any deleterious effect, but because a certain amount of matter is introduced into the circuit, which has the usual imperfections associated with matter ; and thus, though the magnification that could be used is very great, it is not infinite.

Next it was found that the valve could also be used for generating continuous waves, at the same time enhancing their power enormously ; so that a talker at the microphone could have the modulations of his speech magnified till they were represented by great electrical energy, which then emerged as ether waves from the aerial, and travelled to a distance, where some trace of them was picked up by another aerial, again magnified, rectified, and transmitted to the ear. In that way it became feasible to transmit speech or music to great distances. This was in 1913.

Four years later the remarkable property of quartz was utilised. A crystal of quartz, if squeezed, becomes positive at one end and negative at the other, this being the kind of electrification produced by pressure applied to the peculiar structure of the crystal. This effect is reversible, so that when it is electrified it constricts as if squeezed. Quartz thus furnished an intermediary between mechanical and electrical vibrations. The pressure applied to quartz was mechanical ; the electric effect resulting was ethereal. The two being reversible, there was a possibility of transmuting an ether wave into a sound wave, or *vice versa*. Quartz could be used either for sending or transmitting. A thin plate of quartz would have a high rate of mechanical vibration in the longitudinal direction. An electrical vibration of the same frequency applied to it would call out this very high frequency sound vibration. It could not be heard, it would be supersonic, but it could produce various effects. These effects have been studied of late years by R. W. Wood of Baltimore and other people. The facts are astonishing, and have introduced a fresh department into science called Supersonics.

Eccles, however, proceeded to use the vibrations of quartz as a transmitter for electrical waves of steady frequency. He regulated the frequency by a tuning-fork, adjusted once for all to the frequency desired. He then caused the fork to operate on a piece of quartz so as to generate electrical vibrations of the same frequency. These could then be magnified up, transmitted to the aerials of the Rugby station, and so send out waves of enormous power all over the earth, with a precision of tuning hitherto unequalled, so that an operator at a distant receiving station with an accurately tuned receiver could do anything he liked with them.

Broadcasting on a large scale now became possible. In 1920 it was initiated in the United States, and in October 1922 the British Broadcasting Company started its career in this country. Later it became a Government institution, the British Broadcasting Corporation, and under the management of Sir John Reith throughout made itself responsible for transmitting speech and music from a number of stations on different wave-lengths to all the houses in the British Isles (and far outside them) who would pay for a licence.

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BEYOND THE MILKY WAY.

BY

SIR JAMES H. JEANS, F.R.S.

Our earth is one of a system of nine planets which, together with millions of smaller bodies, asteroids, comets and meteors, circle round the sun; our sun is one of a system of millions of stars which circle about one another: this star-system is one of millions of star-systems—and here so far as we know, the sequence ends abruptly. These star-systems are the biggest objects known to science; there is nothing beyond them except the great universe itself. They form the largest subdivision of the universe, and it is from this circumstance that they derive their special importance to science.

The star-system which is best known to us is, of course, our own; we call it the Galactic system, because it is bounded by the Milky Way. We get the best picture which modern science can give us of this system if we think of it as being shaped like a cart-wheel, with the sun perhaps a third or a half-way along one of the spokes, and rotating like a cart-wheel. We still do not know the size of the wheel with any approach to accuracy, but its diameter is probably of the order of two hundred thousand light-years. Still less do we know how many stars there are in the whole wheel. It is almost certainly greater than a hundred thousand million, and may quite well be two, three, four or even five times this number. The band of light we call the Milky Way is formed by the combined light of all the stars at great distances from the sun, including those which form the rim of the wheel. The wheel is held together by the gravitational attractions of the different stars of which it is composed. As a consequence, the outermost stars move most slowly and take the most time to perform a complete revolution, just as in the Solar System the outermost planets move most slowly and take longest to describe their orbits round the sun. The sun probably moves round the hub at about 200 miles per second, and takes something over 200 million years to perform a complete revolution.

In the early days of astronomy this was assumed to be the only system of stars in the sky. Then it began to be conjectured by Kant and Herschel that it was only one of innumerable systems. Recent research has confirmed this conjecture very fully. Something like two million such systems can now be observed; they are the objects we describe as extra-galactic nebulæ.

A random collection of extra-galactic nebulæ seems at first to show a bewildering variety of size, shape, brightness and constitution, but a scientific study soon reduces them to law and order. It soon transpires that size and brightness go together, and that variations in both originate in differences of distance. The nebula which appears small also appears faint; and does both, merely because it is far away. Thus, the faintness of a nebula gives us a measure of its distance; and we can easily get rid of effects which arise from mere differences of distance.

Also we can avoid all complications caused by the different orientations of the nebulæ by a very simple artifice; we reject all those which are not seen edge on, and confine our attention to those that are. We can do this quite recklessly, as some two million nebulæ can be seen in all.

We have now eliminated all the purely geometrical effects arising from differences of distance and of orientation, and are left with real physical differences of shape and constitution. We find that by far the greater number of our edge-on nebulæ can be arranged in a single continuous sequence; it is, in brief, a sequence which begins with spheres and ends with flat discs, although other features besides shape change as we move along it. The nebulæ at one end of the sequence consist solely of round, fuzzy masses—even the most powerful telescope shows no stars in these. About half way along, stars begin to appear in the outer edges of the nebulæ. Then, further along, come nebulæ such as the great Nebula in Andromeda, which consist of a comparatively

small, central, fuzzy mass, surrounded by vast crowds of stars. At the extreme end of the sequence we have pure clouds of stars such as our own system. The comparison of the cart-wheel remains quite a good one, throughout the second half of the sequence, because the nebulæ here generally have a thick central projection, which we may describe as the hub of the wheel, while the rest of their structure is flat. In brief, our sequence is one of nebulæ arranged in order of flatness, and this suggests a simple theoretical interpretation of the sequence.

We know how increasing the speed of rotation of a body results in a flattening of its shape. The ordinary Watt governor provides an obvious instance—as the engine runs faster, it flattens out. The sun rotating only once every 26 days, is an almost perfect sphere. The earth, rotating more rapidly, but still very slowly, is slightly flattened, so that we usually describe it as orange-shaped. Jupiter rotates more rapidly—once every ten hours—and is much flatter in shape. Finally, astronomical bodies which are rotating very rapidly may be almost completely flat. It is natural, then, to try to interpret our sequence of nebulæ as one of bodies which are rotating at different speeds. And as we know that the speed of rotation of a body increases as it shrinks, it seems likely that we may interpret this sequence of nebulæ as one of different stages of development or evolution. If this conjecture is sound, a nebula starting with little rotation at first and shrinking in size, would gradually increase its speed of rotation as it shrank, *and would move steadily along the sequence as it did so.*

The way to test this conjecture is to calculate for ourselves how a mass of rotating gas would change in shape as it condensed and shrank. Although the mathematical analysis is not simple, and cannot be absolutely precise, it is, I think, fairly conclusive; we find that the evolution of a mass of rotating and shrinking gas would be represented exactly by passage along the sequence.

It is not worth enumerating all the detailed changes which, as theory predicts, would accompany this evolution, but one is worthy of attention. The more highly developed nebulæ do not show a uniform distribution of gas in their outer fringes, but an uneven distribution which first forms condensations or knots, and finally develops into separate stars, such as we have already seen in the Great Andromeda Nebula. Mathematical theory not only predicts this, but enables us to calculate the amounts of gas which would go into each of these condensations; in other words, we can calculate theoretically what the masses of the stars ought to be if our theory is sound. It is gratifying to find that these theoretical masses agree pretty well with the masses of actual stars. We may, then, be fairly confident that this is the way the stars come into being; our sequence of nebular configurations is, in effect, a sort of cinematograph film of the birth of the stars.

We may, then, feel on fairly safe ground in tracing the evolution of the universe back from stars to nebulæ, but how did the nebulæ themselves come into being? The conjecture which at once jumps to the mind is that the nebulæ may have been formed by the same process as the stars; just as the stars came into being as condensations in a tenuous, uniformly spread gas—the outer fringes of the nebulæ—so the nebulæ may themselves have previously come into being as condensations in an earlier mass of uniformly spread, tenuous gas. This can never be more than a conjecture, but there are strong arguments in its favour, as we shall now see.

We have already seen how differences in size and brightness between nebulæ of the same shape are almost entirely due to a distance effect. In other words, the faintness of a nebula gives us a measure of its distance. This makes it possible to estimate the distances of all nebulæ, even the very faintest, with fair accuracy. The faintest which can be observed photographically in the 100-inch telescope prove to be at the amazing distance of about 140,000,000 light-years. Dr. Hubble finds that the two million nebulæ which lie within this distance are fairly uniformly spaced at about 1,800,000 light-years apart. We can construct a model, by taking apples and spacing them at about 10 yards apart, until we have filled a sphere a mile in diameter. This will use about 300 tons of apples. This sphere is the part of space we can see in the 100-inch telescope; each apple is a nebula containing matter enough for the creation of several thousand million stars like our sun; each atom in each apple is as big as Betelgeux with a diameter equal to, or slightly larger than, that of the earth's orbit.

The circumstance that the nebulæ are fairly uniformly distributed through space certainly supports the conjecture that they may have originated out of a primæval gas spread uniformly through space. Moreover, it can be proved that such a gas would not stay uniformly spread through space but would break up into condensa-

tions; and that each condensation would have something like the same mass as the observed nebulæ. Or to put the same thing in another way, the primæval gas would break up into condensations at distances apart comparable with the 1,800,000 light-years which Dr. Hubble finds for the average distance of the actual nebulæ. Thus, although, from the nature of things, we can never know the truth for certain, there are good reasons for conjecturing that the nebulæ came into existence as condensations formed by a primæval gas which was spread uniformly, or at least with some approach to uniformity, throughout space.

Cosmogony presents us with a picture of the evolution of the universe—a cinematograph film—in which big bodies continually break into smaller; the film shows the one for ever changing into the many. One primæval gas produces millions of nebulæ, each nebula produces millions of stars, each star may, perchance, change into a solar system producing millions of planets, comets and meteors. Even this is not the end of the story, for planets may break up and form satellites, satellites may break up and form rings of miniature moons, such as we see encircling Saturn. The extra-galactic nebulæ illustrate the first two chapters in this general break-up—

I. Primitive chaos into nebulæ.
II. Nebulæ into stars.

The general breaking-up process of which this is the beginning is in operation throughout the universe. It might be thought that the attractive forces of gravitation would continually draw all the broken pieces together again.

The exact reverse appears to be the case. Not only is the substance of the universe for ever being broken into smaller pieces, but these pieces for ever tend to scatter further and further apart.

To take the example nearest home, the earth is forever driving the moon further away, by the agency known as tidal friction. When we watch the waves of the sea being checked by a sea-beach or a headland, we may reflect that their impact is not only slowing down the earth's rotation and so lengthening the day—it is also lengthening the month by driving the moon further away from the earth. Incidentally, it is also, through the solar tides, driving the earth further from the sun, and so lengthening the year as well.

Again, every ray of sunlight that enters our eye carries mass with it; eight minutes previously this mass was part of the mass of the sun. Every second the sun loses more than four million tons of mass, in the form of sunlight and sun-heat. As the result of this continual loss of mass, the sun's gravitational hold on its family of planets for ever weakens and these are driven further off into space. The earth's orbit round the sun is not so much like a circle or ellipse as like a coiled watch-spring—a spiral for ever receding into the cold and dark of space.

The same tendency affects the galactic system as a whole. The stars of which it is formed continually scatter their mass broadcast in the form of radiation. As they do so, their gravitational hold on one another weakens, so that the whole galactic system for ever expands.

And it must be the same with the other star-systems in space. Throughout the universe, all the smaller broken pieces, satellites, planets, stars, are scattering away from one another in apparent opposition to the laws of gravitation.

Still more surprising and sensational is the recent discovery that the largest pieces of the universe—the great extra-galactic nebulæ we have been discussing—are to all appearances engaged in a similar scattering. They, too, appear to be running away from us and from one another. Until recently, it was thought that on the whole the nearer nebulæ were approaching the galactic system, while the more remote were receding. We now know that the nearer nebulæ appeared to be approaching merely because they happen to lie mostly in the direction towards which the sun is being carried by the rotation of the galaxy; actually we are approaching them. After the sun's motion in the galaxy has been taken into account, all, or nearly all, of the nebulæ appear to be receding from the galaxy. The nearer nebulæ have small speeds, and the more remote nebulæ have greater speeds; in general, speed is approximately proportional to distance. This simple law seems to hold to the very furthest of the nebulæ—Hubble finds that for every million light-years of distance, there is a speed of recession of about 105 miles a second. The last nebula to be investigated at Mount Wilson shows a speed of recession of 12,300 miles a second; its distance, as estimated from its faintness, being about 105 million light-years.

On the face of it, this looks as though the whole universe were uniformly expanding, like the surface of a balloon while it is being inflated, with a speed such that it doubles its size every 1,400 million years.

One of the great puzzles of astronomy at the present moment is whether these apparent motions of recession are real or not. They are deduced from spectroscopic observations; the nebular spectra show displacements to the red, which, interpreted in the most obvious way as Doppler effects, give the speeds already mentioned. Yet every spectroscopist knows that many factors besides motions of recession are capable of reddening light.

There is one strong theoretical argument in favour of regarding the apparent speeds as real. Einstein's original cosmology regarded the universe as being as full of matter as a universe of its size could possibly be without violating the theory of relativity. Recently, Lemaître, of Louvain, has shown that a universe of this type would not be static—there would be an unstable quality about it. The condensation of the primæval gas into distinct nebulæ, and the imprisonment of a large part of the free energy of the universe in these nebulæ would cause it to start expanding, in which case it would continue to expand, its radius finally increasing exponentially with the time, until it ended up as an empty universe—finite matter spread through infinite space. Throughout the motion, the relative speed of recession of any two nebulæ would be exactly proportional to their distance apart, so that, at first glance at least, this theory seems exactly to fit the observed facts. It not only provides a suggestion as to why the nebulæ may be receding. It goes much further and predicts that they must be receding; if Einstein's relativity cosmology is sound, the nebulæ have no alternative—the properties of the space in which they exist compel them to scatter.

Yet various circumstances suggest a need for caution. For one, the speeds of the nebulæ are not strictly proportional to their distances, and it is not easy to explain the discrepancies, which do not look like mere random motions. Again, the very magnitude of the apparent speeds casts doubt on their reality; they would reduce the whole existence of the universe to a mere flash—at any rate in comparison with what we have recently believed. If they are real, Eddington has calculated that the universe must have started from a radius of about 1,200 million light-years, and that its total mass must be about $2{\cdot}3 \times 10^{55}$ grammes, which is the mass of $1{\cdot}4 \times 10^{79}$ protons and an equal number of electrons. So far as we can tell from the masses of the extra-galactic nebulæ, the present average density of matter in space appears to be not less than 10^{-30} grammes per c.c., which, with the same amount of matter, would assign a radius of 13,200 million light-years to the universe—only eleven times its initial value. If, then, the motions are real, the universe is only at the beginning of its career; it cannot have doubled many times since it started. And as it appears at present to be doubling in size every 1,400 million years, the few doublings which these figures permit cannot have occupied more than 10,000 million years at most.

General calculations on the ages of astronomical bodies point to far longer periods of time than this. The mere act of condensation of the nebulæ—the occurrence which seems most likely to have started the expansion—was probably a matter of hundreds of thousands of millions of years. Perhaps there is no real difficulty here; it might well take this long to get the doubling process really going—indeed, Lemaître has calculated that it would take some such time. The real difficulty is that the stars carry intrinsic evidence of having lived through far longer periods than this. Both single stars and binary systems show an approximation to equipartition of energy which must have taken far longer than this for its establishment.

Spectroscopic binaries provide further evidence. These consist of pairs of stars revolving round one another. Observation reveals a complete sequence; it begins with systems which appear to have just broken into two as the result of rotation—pairs of stars describing circular orbits, and almost in contact—and ends with systems in which the two stars are far apart and describing elliptical orbits. Theory suggests that this observational sequence exactly depicts the evolution of a star which has broken into two as the result of excessive rotation. The outstanding importance of this sequence to our present discussion lies in the fact that the stars at the beginning of the sequence are undoubtedly many times more massive than those at the end. It seems likely that those which are now at the end must have begun at the beginning and lost the greater part of their mass in the form of radiation, and to do this would take millions of millions of years. Considerations such as these make it very difficult to believe that the universe can be such an ephemeral concern as the apparent speeds

of recession of the nebulæ would suggest. There is, I think, every reason to hope that within a very short time we shall know the truth about this puzzle, and, whatever the solution may be, there seems to be a considerable chance that it may provide us with a clue, perhaps even with a key, to the structure of the universe as a whole. Until quite recently, the scientist, like the ordinary man, accepted the fundamental ingredients of our experience—space, time, matter and energy—more or less at their face value. The most obvious and superficial interpretation suggested by everyday experience was assumed to correspond fairly closely to ultimate reality. The theory of relativity has shown that we were utterly wrong about space and time, and we are beginning to suspect that we are still just about as far wrong about matter and energy. The concept of an expanding universe may prove after all to be a false scent, and the truth may lie in some other direction. In either case the observed phenomena must mean something, and their true interpretation, when it is found, may carry us a step on towards the solution of the greatest mysteries of the external world—the nature of space, and of time, matter and energy, and of the combination of all these which constitutes the physical universe.

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OCEANOGRAPHY IN THE ANTARCTIC.

BY

Dr. S. W. KEMP, F.R.S.

The discourse gave a general account of the research work that is now being carried out in the Southern Ocean by the Discovery Committee. The objects of this work are numerous, for it has been undertaken with the intention of exploring the resources of the Dependencies of the Falkland Islands; but the principal object is to obtain information which will have a bearing on the great southern whaling industry.

Dr. Kemp first spoke of the work which has been done at a Marine Biological station erected in South Georgia. Here the Blue and Fin whales caught at the adjacent whaling station have been systematically examined during the past six years and many facts relating to their life-history have been elucidated. It is now known that these whales are sexually mature at the age of two, and that the females normally produce one young one in every alternate year. Whales are physically mature in six to eight years, after which no further growth is possible, and their normal duration of life probably does not greatly exceed twenty years. It has recently been found that the age of female whales up to ten or twelve years can be estimated with some accuracy from the numbers of *corpora lutea* which remain as vestiges in the ovaries. With the information which has been gathered it is possible for the first time to obtain an insight into the constitution of a stock of whales, and problems such as the rate of deterioration of the stock by intensive hunting can now be attacked. Experiments have been made in marking whales in order to trace their migrations, but these have hitherto failed, and new methods are under consideration.

The research ships employed by the Discovery Committee are mainly engaged in a study of the environment of whales, and it is hoped that in course of time it will be possible to explain the great fluctuations which exist from season to season in the abundance of whales. Whale food, which consists exclusively of a prawn-like animal, *Euphausia superba*, has been the object of close investigation. It appears that the natural habitat of this animal is along the edge of the Antarctic pack-ice from which it spreads northwards each spring, and is sometimes found in great concentration in areas which whales frequent. The reproduction of the prawn and its dispersal in the Southern Ocean afford problems of considerable interest which at present are not completely solved.

The microscopic plant life of the sea, on which all marine animals are ultimately dependent for nourishment, is unusually abundant in the south. This, in general, is to be explained by the abundance of phosphates and nitrates in solution in the sea water, but there is another factor—at present unknown—which may limit the production of plant life in certain areas.

The circulation of nitrates and phosphates has been examined in connection with a general study of the hydrology of the South Atlantic and Southern Oceans, and in the course of this work the principal mass movements of the water have been elucidated.

Attention was directed to the extreme importance from a biological point of view of certain lines where sudden changes of hydrological conditions occur, for these lines separate distinct faunas in the upper layers of the sea just as effectively as land faunas are separated by mountain ranges.

Dr. Kemp concluded his lecture by an account of the survey work undertaken with a view to improving the charts of these little-known parts of the Empire. The recently introduced method of echo-sounding has been employed with very great success, and the bearing of the results on geological views regarding former land connections was discussed. Echo-soundings have been successfully taken to a depth of 4½ miles. In the course of the work new charts have been made of South Georgia, Bouvet Island, parts of the South Shetlands, and of the South Sandwich group, the last-named a chain of volcanic islands, which had not been surveyed for over a hundred years.

For the Evening Discourse by Mr. H. E. Wimperis, C.B.E., on 'High-speed Flying,' see *Journ. Roy. Aeronautical Soc.*

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APPENDIX.

DISCUSSION ON THE EVOLUTION OF THE UNIVERSE.

(Sir JAMES JEANS, F.R.S.; Prof. E. A. MILNE, M.B.E., F.R.S.; Prof. W. DE SITTER; Prof. Sir A. S. EDDINGTON, F.R.S.; Prof. R. A. MILLIKAN; Rt. Rev. the LORD BISHOP OF BIRMINGHAM, F.R.S.; Gen. the Rt. Hon. J. C. SMUTS, P.C., C.H., F.R.S.; M. L'ABBÉ LEMAÎTRE; Sir OLIVER LODGE, F.R.S.)

Held in Section A (*Mathematical and Physical Sciences*) *on Tuesday, September* 29, 1931.

Sir JAMES JEANS, F.R.S.

With so large a subject, and so short a time, I can only hope to sketch out a sort of skeleton in its barest outlines, leaving it for the other speakers to fill in details according to their individual interests and opinions.

We are, of course, discussing only the physical universe. Here strict determinism reigns, because even if there is no determinism in the behaviour of individual atoms, there are so many atoms in even the tiniest bit of matter that we may take an average. The laws of probability provide something which is, for our present purpose, equivalent to a strict physical determinism.

It follows that the final state of the universe is inherent in the present state, just as this present state was inherent in the universe at its creation. The physical universe never has any choice—it must inevitably move along a single road to a predestined end. What we are calling evolution is like the rolling of a train along a single-track line, with no junctions of any kind.

The various possible lines of development for the universe are like an enormous number of single-track lines. We can imagine these running through a diagram in which all possible configurations of the universe are mapped out, just as all points in England are mapped out in an ordinary map. As we do not know which track we are on, it is futile to discuss at what particular spot it ends. But we may be able to discover in what kind of country it ends, and this is really the information we want.

Imagine that we suddenly waken up from a state of unconsciousness to discover we are on a British railway. We have no means of knowing where our journey will end. Yet if we have a physical map of Great Britain with us we may notice that only a few hundred acres out of 55 million lie more than 4,000 feet above sea-level. Although we cannot say where our journey will end, there are obviously very long odds that it will end at less than 4,000 feet above sea level. If a barometer in our

compartment indicates that we are already 4,000 feet above sea level, there are very long odds that the general trend of our journey will be downhill.

It is this kind of consideration rather than exact knowledge that guides us in our efforts to study the evolution and final end of the universe. We can have no certainty and must be guided entirely by probabilities. Yet the odds we encounter prove always to be so immense that we may, for all practical purposes, treat long odds as certainties. The number of particles—electrons and protons—in the universe is of the order of 10^{79}. As a consequence, high powers of 10^{79} enter into all our odds, and, this being so, we need not trouble to differentiate too carefully between long odds and certainties.

In our diagram of the universe there is a quantity—the entropy—which plays much the same part as height played in our imaginary railway map, except that *small* entropy corresponds to *great* height, and *vice-versa*. Thus entropy corresponds rather to depth below the top of the highest mountain, whose height is 4,400 feet. As most of Great Britain is only a little above sea-level, most of it is at a depth, in this sense of the word, of nearly 4,400 feet—the maximum depth possible. In the same way, most of our map of the universe is at the maximum entropy possible—all, indeed, except for tiny bits proportional to inverse powers of 10^{79}.

At the moment we cannot prove this statement because we have not yet defined 'entropy.' And there is no need to prove it, because the best definition of 'entropy' makes the statement true of itself and automatically. We may define maximum entropy as specifying the condition which is commonest in our map of the universe; we define entropy in general so that the more common condition is always of higher entropy than the less common. Entropy gives a measure of the 'commonness' of a given state in our map. Actually, if W is the 'commonness,' of a certain state, the mathematician defines the entropy of this state $k \log W$, where k is the gas-constant.

Just because such immense numerical factors are involved, conditions of 'maximum' entropy are incomparably more common than those whose entropy is less, and so on all down the ladder. Thus it is practically certain that the universe will 'evolve' through a succession of states of ever-increasing entropy, until it ends in the final state of maximum entropy. Beyond this it cannot go; it must come to rest—not in the sense that every atom in it will have come to rest (for maximum entropy does not involve this) but rather in the sense that its general characteristics cannot change any more.

Yet, if someone asserts that this will not happen, and that the universe will move to a state of lower entropy than the present, we cannot prove him wrong. He is entitled to his opinion, either as a speculation or as a pious hope. All we can say is that the odds against his dream coming true involve a very high power of 10^{79}—in his disfavour.

The question of discovering the final state of the universe is merely that of discovering how far its entropy can increase without violating the physical laws which govern the motions of its smallest parts—the physical properties of matter come into account as soon as we try to discover the state of maximum entropy. Let us take two simple instances. I pour

red ink into water and leave them to diffuse into one another. The final state, as we know, is one in which they are uniformly mixed to form a homogeneous pinkish fluid; thus this state of uniform mixture must be the state of maximum entropy. Again, I put a kettle of cold water over a hot fire. The final state is one in which the water turns into steam. Just as the red ink diffused itself equally through all parts of the water, so the heat of the fire tends to diffuse itself equally through coal, kettle and water. There is a possible state of this miniature universe in which the water is turned to ice and the fire is even hotter than before through having abstracted heat from the water. This is one of the configurations on the map, and we cannot know for certain that it will not be the end of the journey—it is perfectly possible in theory for our kettle to freeze when we put it on a hot fire. Yet it is almost infinitely improbable that it will do so, because the entropy of the frozen configuration is lower than that of the starting-point.

These two simple instances illustrate a very wide principle—the final state of maximum entropy avoids concentration, whether of special substances (as with the ink) or of energy (as with the heat of the fire). The 'commonest state' is one in which both substance and energy are uniformly diffused, just as the commonest state in which we find an audience is that in which tall people and short, dark and fair, and so on, are uniformly diffused.

Considerations of this kind show that the universe still has a long way to go. It can increase its entropy by distributing its radiant energy more uniformly; at present this is still very far from being uniformly distributed. Out in the furthest depths of space, the density of radiant energy corresponds to a temperature of less than one degree above absolute zero: in the interstellar spaces of the galactic system, three or four degrees only; near the earth's orbit about 280 degrees; at the sun's surface about 6,000 degrees; at the sun's centre perhaps 40 or 50 million degrees. The entropy is increased by equalising these temperatures: that is why energy flows from the sun's hot centre to its cooler surface, and why it then streams out into space, past the earth's orbit, into the cold and dark of interstellar and intergalactic space. There can be no end until all these regions are at the same temperature, with radiant energy diffused uniformly through space.

Most radiant energy has its origin in atomic disturbances. Each atom is a sort of storehouse of energy or mass—we now believe the two to be identical—and at intervals, either spontaneously or through interference from outside, an atom may discharge some of its energy or mass into space in the form of radiation. This wanders through space like a sort of bullet of radiation travelling with the velocity of light; we call it a photon. We see the encounters of these photons with free electrons in the Compton effect, and with complete atoms in the more ordinary phenomena of absorption of radiation. By studying these we can deduce the masses of the photons concerned.

Ordinary spectroscopy tries to correlate the masses of the photons with the atomic changes which gave rise to them, the mass of the photon being precisely the mass which the atom lost when it ejected the photon —the process of ejection is, in fact, a splitting up of the atomic mass into

two parts, one of which remains with the atom, while the other goes off with the photon. Generally, the former part is many millions of times greater than the latter. But the so-called ' cosmic radiation ' which falls on to the earth from outer space is found to consist of photons whose masses are comparable with those of complete atoms. The two most massive types of photon so far detected in this radiation are found, to within errors of measurement, to have precisely the masses of the helium and hydrogen atom respectively. The simplest interpretation is that these photons originate out of the complete transformation of atoms of helium and hydrogen into radiation. But this is not the only interpretation, nor, I think, the most probable. All atoms consist of protons and electrons in equal numbers, so that their masses are (approximately) exact multiples of the mass of the hydrogen atom, which contains one of each. If an electron and a proton were to neutralise one another in any atom whatever, the atomic weight of the atom would be reduced by unity, and the atom would eject a photon of mass equal to the hydrogen atom. In the same way, if an α-particle were to neutralise itself by combining with two electrons in any atom whatever, the resulting photon would have a mass equal to the helium atom.

Although not everyone agrees, this seems to me the most plausible interpretation of the two most penetrating constituents of the cosmic radiation. In any case, these photons are too massive to originate in any less drastic transformation than annihilation. I think they provide evidence that matter can be annihilated, and point to a general degradation of complex atoms in the direction of simplified structure and decreasing atomic weight.

On this view, electrons and protons must be regarded as concentrated stores of energy, which are capable of being set loose and dissipated in the form of radiation. The entropy of the universe is increased by this process, and the final state of maximum entropy is one in which every electron and proton which is capable of annihilation has been annihilated.

The question at once arises as to how many of the 10^{79} or so electrons and protons which form the universe are subject to annihilation. They may all be, in which case the final state of the universe will be one in which all matter is dissolved into radiation, and nothing remains but radiation traversing empty space. Or again, only special types of atoms may be liable to annihilation, just as only special types are liable to radio-active disintegration. In this case, the final state will be one from which these atoms, like the radio-active atoms, will all have disappeared. There will be a universe of *cold* inert matter and of *cold* radiation.

Amongst a mass of theoretical possibilities, the one certain fact is that if the atoms of our earth are undergoing annihilation, their rate of destruction must be exceedingly slow. Their average life must be of the order of 10^{18} years at least, otherwise the energy generated by their annihilation would make the earth too hot for human habitation. If we restrict our range of vision to some 10^{17} years or so, the atoms of the earth may be treated as permanent—whatever happens to sun and stars, the solid earth will endure.

This suggests the possibility that annihilation can only occur in types of atoms which are not found on earth—possibly atoms of higher atomic

weight than terrestrial atoms. Many considerations suggest that the stars produce their radiation by the annihilation of their substance. Perhaps the strongest, and certainly the most clear-cut, is that the stars which appear to be the youngest (such as binary stars describing close circular orbits) are statistically the most massive. It looks as though the stars lost a large part of their mass in the course of their lives, and this can only mean that a large number of their atoms, or a large part of these atoms, undergo annihilation. If so, our terrestrial atoms may well be a sort of indestructible ash, the relics of more complex atoms after all that can be annihilated has been—just as lead and helium are the relics of radio-active atoms of greater atomic weight.

There is, however, an alternative possibility. If matter is capable of annihilation, the general principles of the quantum theory show that the rate of this annihilation must consist of two parts. The first is an annihilation which goes on steadily regardless of external conditions of temperature and pressure, just as radio-active disintegration does. The second is an annihilation which is incited by high temperature, and whose rate increases with an increase of temperature. Calculation shows that this will only come into play when the temperature begins to approach a million million degrees.

Clearly, if the cosmic radiation we receive on earth proceeds from annihilation, it must be annihilation of the first kind; if it had been produced by annihilation of the second kind, and so at a very hot place, it would have all been absorbed by matter before it could climb down the very long temperature gradient and emerge into outer space. But it is possible that it is mainly radiation of this second kind which, after innumerable absorptions and re-emissions, or repeated softenings, appears as the ordinary light of the sun and stars. Thus, many astronomers think that matter is annihilated in appreciable amount at high temperatures only, and that the annihilation at low temperatures is negligible. In their view, terrestrial atoms escape annihilation, not because they are specially indestructible, but because they are specially cool.

Yet this view is confronted by many difficulties. The cosmic radiation cannot be produced by this high temperature annihilation, and yet this is of vast amount—comparable at least with the radiation of all the stars. Also, for the heat of the stars' interiors to produce annihilation, their temperatures must approach a million million degrees. This requires what so far seems to be an impossibly high temperature-gradient inside the star. Also such high temperatures, and such a mode of generation of energy, would, so far as we can see, make the stars unstable, and indeed explosive, structures. The best simple analogy to such a structure would be a keg of gunpowder, with its centre raised to the flash-point of the powder. Nevertheless, it must be admitted that we are far from definite knowledge on these questions, which are still in the stage of very heated discussion.

There is yet another possibility, which seems far more likely to-day than even a year or two ago. It is that stellar light and heat do not result from annihilation of matter at all, but from some less fundamental change in atomic structure. If so, unduly high temperatures are no longer needed at the centre of the stars; this difficulty disappears. And if the

atomic changes are spontaneous, like radio-active change, the difficulty as to stability also disappears. But these milder changes cannot provide enough energy for the long lives—millions of millions of years—through which the stars have to all appearances existed. The equipartition of energy in the motion of the stars, as well as in the orbits of binaries, and also the small masses of what appear to be the oldest stars, all point to extremely long stellar lives. To provide adequate total radiation throughout these, we need annihilation of matter, and nothing less will serve. If we cannot have this, we must conclude that the universe of stars is still quite young, in spite of looking so old ; its appearances of great age must all be deceptive.

Recent developments of the theory of relativity certainly give much support to this possibility. These suggest, somewhat strongly, that the whole universe may be expanding, while recent astronomical observations, if they have been rightly interpreted, indicate that it actually is expanding, and this at so rapid a rate that it becomes a mere transitory and ephemeral structure compared with what we recently thought ; the spectra of the great extragalactic nebulæ seem to indicate that these bodies are running away from one another so fast that they cannot have been running for long. This reduces the whole life of the universe to a matter of hundreds of thousands of millions of years at most, and, incidentally in so doing, brings almost complete chaos into the already chaotic problem of stellar evolution.

Another interpretation of the observations—nearly but not quite identical with the foregoing—is that the universe retains its size while we and all material bodies shrink uniformly. The red shift of the spectra of the nebula is then due to the fact that the atoms which emitted the light millions of years ago were larger then than the present-day atoms with which we measure the light—the shift is, of course, proportional to distance. The final end here is a universe in which all matter has shrunk to nothing.

On the other hand, if the universe is expanding, the stars are merely pouring out their radiation into a bottomless pit, since the space to be filled with radiation is for ever increasing in amount. The total energy of the universe is for ever decreasing in amount, because radiation does work in pressing out the boundaries of the universe—just as a gas loses energy and so cools when it expands, and presses back the boundaries of its ‘universe.’ Thus, the mass of the stars is continually changing into energy, while this energy in turn changes into mere additions to the size of the universe. There is conservation neither of mass nor of energy. Nor, if the evidence of the cosmic radiation is to be trusted, is there any conservation of matter. Matter turns into energy and energy into mere bigness of space.

Such a universe can never attain a state of maximum entropy ; such states abound in our map, but no railway line on which we can possibly be leads to them. The universe can never come to rest ; it is destined to go on changing for ever—continually swelling in size, and continually dissolving into nothing but size, yet never attaining either complete dissolution or truly infinite size. It seems likely that after a sufficient time, the different galaxies or star-systems will be scattering away from one another with speeds greater than that of light, so that radiation will be

unable to bridge the gap between them; it will have become impossible to see any one galaxy from any other, even by light which left it at the very beginning of time; only the mathematician will be able to deduce the existence of the other galaxies in recondite ways—and probably no one will believe him. Then at last these will have justified Herschel's name for them—'island universes.'

When this time comes, each individual galaxy will make its own individual ripple of radiation; these ripples will for ever increase in size and weaken in intensity, but cannot intermingle to form a uniform distribution. The evolution of the universe will be nothing but a rapid spreading of ripples in a still more rapidly-spreading space.

Even the most confirmed optimist could not, I fear, claim that any of the possible lines of development or final ends is of a particularly exhilarating nature; the most consoling thought I can offer is that when the worst comes to the worst, we shall none of us be there to see.

Suppose some infallible oracle offered to give a 'Yes' or 'No' answer to two scientific questions for each of us. Personally, I think I might choose as my two questions:

1. Does the main energy of stellar radiation come from the annihilation of matter?
2. Is the universe expanding at about the rate indicated by the spectra of the nebulæ?

Prof. E. A. Milne, M.B.E., F.R.S.

It has sometimes been asked whether the universe is evolving at all. Certainly its future evolution is a matter of speculation and its past evolution a matter of inference. But that it is evolving at this present time is not a matter of speculation at all; it is a matter of observation. We actually see stars undergoing drastic changes—changes of organisation so fundamental that they can almost be described as mutations. I refer to those outbursts which we call Novae.

From time to time in the heavens a faint star is seen to blaze up for a few hours or days, increasing in brightness perhaps some 25,000 times, then irregularly fading and after a few years returning to its pre-outburst brightness. Such outbursts are by no means uncommon. The first quarter of the current century has witnessed five novae visible to the naked eye. Many present will remember Nova Aquilae of 1918, which was visible even before dark; and Nova Cygni of 1920. These, from their brightness, were comparatively near-by stars—Novae Aquilae, the more distant, was only some 300 parsecs away. But many more have been observed in the most distant regions of space. Up to 1929 over eighty novae had been recorded in the great nebula of Andromeda, and were used by Hubble to determine its distance. Thus, Novae are distributed throughout space. They are also distributed fairly frequently in time. Hubble has estimated that some thirty novae occur each year in the Andromeda Nebula. It has also been estimated that in our own galactic system there is at least one nova per year. Such estimates are conflicting, but, as many have pointed out, novae are sufficiently frequent to imply that on the average every star 'becomes a nova' at least once

in its lifetime. We thus have in each nova an evolutionary event characteristic of the stars as a whole. Let us examine the event more closely.

The spectrum of a nova shows that the outburst is accompanied by the emission of streams or jets of gas, in various directions, at the large velocity of some 1,500 kilometers per second. There is little doubt that this gas is a portion of the atmosphere driven off by the greatly increased radiation pressure. Some years ago when I was engaged in discussing the sun's chromosphere, I found that the hotter stars would have difficulty in retaining certain elements such as calcium, since the pressure of the outflowing radiation, greedily absorbed by the atoms in their spectral lines, would be strong enough not only to balance gravity at the star's surface, but further to repel these atoms in a kind of upward rain. Later I examined the probable limiting velocity that such accelerated atoms would attain, and I found that it was of the order observed for the gases expelled from novae. Thus, one of the characteristics of a nova outburst is that the huge increase in brightness blasts off, almost explosively, portions of the star's atmosphere, and that these clouds of gas travel outwards through space. We shall see the evolutionary significance of this later.

This driving off of atoms probably has its maximum effect when the star is at its brightest. What of the subsequent events as the star fades ? The atmospheric layers, or some of them, continue their journey outwards, but what of the mother star itself ? Let us re-examine such a star a few years after the outburst. We find that it has returned to the same undistinguished brightness as it possessed before the outburst—but with a difference. Its spectrum is now of what is called type O, or Wolf-Rayet, indicating a very high surface temperature or surface brightness. From this we can make a most important deduction. If the *total* brightness of the star is the same as before the outburst, but the *surface* brightness—the brightness per square mile—is much greater than before, the total area of the star must be much smaller than before. And if the area is smaller, so must be the radius and the volume. The reduction in radius as observed may be as much as 10 : 1 or even more. Thus, after the outburst the star is found to have shrunk. We must resist the temptation to describe it as the shadow of its former self. It is smaller in dimensions but much brighter per square mile, and as bright as before in the aggregate. We have the paradoxical situation that the outer parts expand, forming an advancing outward-moving cloud, but the inner parts contract.

We can only see the shrunken star when the expanding clouds have moved out of the way. During the outburst itself the expanding atmospheric gases obscure our vision of what is happening. We can only conjecture what goes on behind the expanding screen. But there can be little doubt as to what is actually happening. The expulsion of the atmospheric clouds is a consequence of the brightening, but the shrinking of the rest of the star is not just an accompaniment of the brightening—it is the actual origin of the brightening. When the star contracts it must lose gravitational energy, which is set free as heat and light. Somehow it must get rid of this energy. If it got rid of it slowly there would be little visible effect. Actually we see the star, in its period of

maximum brightness, getting rid of an enormous amount of energy very quickly indeed.

Since the rate of brightening is very rapid, we infer that the process of shrinkage is very rapid—in fact cataclysmic. The process of shrinkage is a veritable collapse. In a nova outburst the star is seen to be collapsing on itself; and the suddenness of the collapse, and the resulting enormous amount of gravitational energy that must be got rid of in the short time available, conspire to produce the huge brightening of the star as observed. This sudden liberation of energy produces enormously increased radiation, which in turn expels the outer layers of gas. Such is the probable explanation of the origin of novae or 'new stars.'

These outer layers, thus expelled, constitute an exceedingly small proportion of the whole mass of the star. Thus, the mass of the star, after the outburst, is practically the same as before, yet it occupies a much smaller volume. Hence, its mean density must be much larger than before. The increase in density as observed varies from one hundredfold to ten-thousandfold. The gases expelled from the star during the outburst are chips from the old block, but the star itself does not remain an old block, it becomes very much of a new block—a very dense block.

We can now link up this widely spread phenomenon with our general views on stellar evolution. A star may be supposed to fade, very slowly but quite definitely, as it ages. At some particular luminosity it collapses, giving rise to the appearance of a nova. This must be due to some structural weakness in the star, some insecurity in its foundations. It falls on itself like a house of cards.

Samson, in the Old Testament story, lost his strength when he was shorn of his locks. The star also loses its strength when it is shorn of its locks. But the shearing of the stellar locks—the liberation of wisps of atmospheric gases—is a consequence, not the cause, of the internal weakening.

We know other stars which are very dense—the stars which are known as 'white dwarfs' and the nuclei of the planetary nebulae. Planetary nebulae are probably exceptionally large ex-novae in which the expelled gases linger round the star as a permanently accompanying nebula. It is reasonable to assume that the white dwarfs have also undergone the process of collapse—that every white dwarf has been at one time a nova. Though few white dwarfs are known, all those with which we are acquainted are close to the sun, whence it is concluded that they are very common in nature. This is in general agreement with the ideas I am describing, since if novae are common the heavens must be littered up with ex-novae, and these remain in the form of white dwarfs. We must now endeavour to explain the origin of the weakening which causes the cataclysm.

It is probable that all stars possess a degenerate core, a central region of matter in the degenerate state. As the luminosity of such a star decreases the core will vary in size. The behaviour of the core is very complicated, and I have not yet been able to trace it in full detail. But it appears that at a certain critical value of the luminosity the core increases *suddenly* in radius, due to collapse of the gaseous envelope. This is a consequence of the very low

opacity of degenerate matter. Degenerate matter obstructs the passage of radiation far less strongly than gaseous matter, and so experiences a smaller radiation pressure; accordingly (at a certain luminosity) as soon as more degenerate matter is deposited on the core, radiation pressure weakens still further, further collapse occurs, more degenerate matter is deposited, and so on. The star has a canker at the core. Or, to put it another way, degeneracy is an internal disease which propagates itself with startling rapidity under favourable stellar conditions. The outward and visible symptom of the disease is the nova-phenomenon; and, as is not unknown in medicine, a high surface temperature is developed. The existence of a discontinuity in core radius can be established mathematically, but I only provisionally identify this particular discontinuity with the observed discontinuity in Nature. We at least obtain a hint as to the character of one of the most important events in the evolutionary history of a star.

But collapse alone is not the only possibility. If the collapsing star is in a state of rotation the increased angular velocity consequent on collapse may cause the star to divide into two, in the manner described by Jeans. Something of this kind has been observed in the case of Nova Pictoris. The two fragments so produced may remain collapsed (and thus very dense), or in certain circumstances, provided as they are with an internal source of energy, one or both may re-expand and re-form even giant stars. This may be the origin of the great frequency of occurrence of double stars in nature. In the fable the mountains gave birth to a mouse. In the heavens the dwarf may give birth to giants.

The reduced opacity of degenerate matter, for which indirectly the nova phenomenon is evidence, has an important bearing on the source of stellar energy. It has usually been concluded that the white dwarf stars, which are amongst the least luminous of stars, have the hottest interiors. This has always been a difficulty, for if temperature has any influence whatever on generation of energy, we should have then that the hottest stars were the worst generators of energy. But this difficulty now disappears. For if the opacity of degenerate matter is very small, the interior of a white dwarf will be almost isothermal, and its temperature will depend on the thickness and opacity of the gaseous fringe or envelope which encases the degenerate interior. This will be modified by conductivity, but it is at least clear that the interiors of white dwarf stars must be amongst the coolest, not the hottest, of stellar interiors. In other words, we find what we should expect, that the coolest interiors are the worst generators of energy. For complete transparency of its interior Sirius B would have a temperature of at most 15 or 20 million degrees.

Sir James Jeans recently drew my attention to certain aspects of hot-water engineering—or, as he put it, 'what every plumber knows.' He pointed out that every plumber knows that a thin layer of asbestos is inadequate for keeping a hot-water container very hot. But the point is that if the hot-water container is surrounded by only a thin film of asbestos, we do not thereby infer that the asbestos is of no importance; we infer that the water in the container is cold! The Celestial Plumber (if Dr. Barnes will permit me the phrase) knows this also; and we see the result in the internal state of the white dwarfs.

To summarise, we see a twofold evolutionary process at work amongst the stars, both parts of the process occurring in this same phenomenon of the nova. On the one hand we observe the stars to be collapsing and giving birth to white dwarfs, possibly also planetary nebulae and even double stars. On the other hand we see nebulous clouds driven off to space, there to reinforce the already existing cosmic cloud. Reinforce? Possibly we have struck the real origin of the cosmic cloud [1]—possibly the cosmic cloud is the totality of the *débris* of the expelled atmospheres of novae and other hot stars. The universe is simultaneously condensing into hard blocks and evaporating to form a tenuous nebula. I repeat, this is what we observe. Sir James Jeans has suggested that the universe began as a widely extended nebula, which condensed into stars in virtue of gravitational instability. The picture I have attempted to draw is that of a concourse of stars on the one hand condensing into denser stars in virtue of instability under a waning light-pressure, and on the other hand simultaneously expelling their atmospheres and generating a nebula.

Prof. W. de Sitter.

I have been asked to make a short contribution to this discussion about the evolution of the universe. I hope you will find it short, although its length will, I fear, exceed the diameter of the earth's orbit round the sun. This way of expressing a time in a unit of length is perhaps somewhat unusual. To have a distance expressed in a unit of time is, of course, very familiar. A lightyear has become an accepted unit of distance. I have purposely used the inverse method to call attention to the fact that the corresponding units of time and of space are so very widely different in relation to actual phenomena. The diameter of the earth's orbit enormously exceeds all lengths that we ever come across in our daily life, whilst a quarter of an hour is considered a short interval. Similarly, a thousand million lightyears is a long distance, in fact many times greater than any actual distance that we have certain cognisance of, but a thousand million years is a short time in the evolution of the universe. It is only a third or a quarter of the accepted age of the earth, and I do not think geophysicists will be ready to take off even one single zero. We believe, and, so far as I can see, on good grounds, that the age of the sun and the majority of the stars is at least a thousand times greater. The time needed for the development of a double star, or even a quadruple or sextuple system, and that required for the evolution of a stellar system or spiral nebula are at least of the same order. In the farthest galaxies that we can observe things appear to be very much the same as in our immediate neighbourhood. There seems to be no indication that these far-away systems are in an appreciably earlier state of evolution than our own. Although, of course, the trustworthiness of this statement decreases very rapidly with the distance, still it seems to show that the lapse of time corresponding to the distance of these systems is only an entirely inappreciable fraction of their whole lifetime. All these considerations consistently lead to the conclusion that the time elapsed since

[1] This has been suggested also by Vorontsov-Velyaminov, *Observatory*, August 1931, and by others.

the beginning of the evolution of the universe is to be measured in billions of years, or perhaps in thousands of billions. I do not propose to discuss the question whether there ever was a beginning. It suffices for my argument to define the 'beginning' as that state of the universe and its constituent parts which we are with our present knowledge and theories content to use as a starting-point, beyond which we do not wish, or are not able, to extend our investigations.

It is in accordance with this slowness of the evolution of the universe—or I should rather say of the constituent parts of the universe, galaxies and stars—that the universe was, until very recently, assumed to be practically in a state of equilibrium. Accordingly, a theory of the universe, in order to find popular favour, had to be static. Some may have felt inclined to disagree with this generally adopted attitude, but if they ventured to confess their dissenting views at all, they did so rather diffidently. A static solution of the fundamental equations of the theory of relativity was what was asked for.

The choice was between two possible statical solutions, which I have been in the habit of calling the solutions A and B, but which may be better distinguished by the names of the 'static' and the 'empty' universe. Einstein's solution, the 'static' universe, was a genuine statical solution. The other one was essentially non-static, but, as a consequence of its emptiness, it could parade in the garb of a static one. Both of these, however, failed to represent the observed facts in the actual universe, which is neither empty nor static, since it is full of galactic systems, which are all moving with large and systematic velocities. The way out of this dilemma has been shown by Prof. Lemaître, whose brilliant discovery, the 'expanding universe,' was discovered by the scientific world about a year and a half ago, three years after it had been published.

It is not necessary to explain this theory in detail here. It has become widely known by technical and popular expositions in many periodicals. The principal point of it is that it shows the observed radial motions of the spiral nebulae to be in accordance with an isotropic, homogeneous, but non-static solution of the field equations of the general theory of relativity, or, we can say, to be a pure effect of the inertia of these bodies. The static solution, in which inertia leaves the relative distances of different material bodies (statistically) unchanged, is shown to be unstable. The actual universe must therefore be represented by one of the non-static solutions. In these, the change at a certain moment may be either an expansion or a contraction. Observation shows that at the present moment we are on an expanding branch. The solutions are arranged in families. In some, the universe contracts to a finite minimum radius and then starts to expand again; in others, this minimum is zero, in other solutions again the radius oscillates between zero and a finite maximum value in a, rather short, period. These oscillating solutions, however, require a density exceeding that which is indicated by our knowledge of the distribution of matter in our neighbourhood.

I must lay stress on the fact that, in using the words 'universe,' 'radius,' 'expansion,' etc., we are really speaking metaphorically, putting an interpretation on the equation of the theory, which is by no means imperative. There occurs in the equation a certain quantity, which may

be either positive, negative, or zero, and which is interpreted as the reciprocal of the square of the radius of curvature. But both this interpretation, and the assumption tacitly made that it is positive (thus making the three-dimensional universe closed) are entirely gratuitous, and not demanded by the theory. I will, however, go on using this convenient metaphorical speech.

Lemaître's theory not only gives a complete solution of the difficulties it was intended to solve, a solution of such simplicity as to make it appear self-evident, once you know it (like Columbus's famous solution of the problem how to stand an egg on its small end)—it also incidentally contains the answer to some questions of long standing, such as the question : What becomes of the energy which is continually poured out into space by the stars ? It is, in fact, used up by the work done in the adiabatic expansion of the universe. There can be not the slightest doubt that Lemaître's theory is essentially true, and must be accepted as a very real and important step towards a better understanding of nature.

Now if we adopt this theory of the expanding universe, it is very tempting to seek a connection between this expansion and the evolution of the material bodies constituting the universe, and to identify the beginning of the expansion with the beginning of that evolution. But the time elapsed since the beginning of the expansion is only a few thousand million years—an interval that we have learned to consider as very short from the evolutionary point of view. There is no escape from this. We can, in fact, make the interval logarithmically infinite, but that is only a mathematical trick : we call zero minus infinity, but that does not make the interval during which anything really happens any longer. It is a consequence of Lemaître's equations that the time taken by the universe to increase its radius from anywhere near its minimum to its present value is of the order of magnitude of the radius itself, if we adopt corresponding units of space and time, *e.g.* years for time and lightyears for space. The real origin of the difficulty is that the ratio of the natural units of space and time is determined by the velocity of light, whilst the velocities which determine the rate of progress of the evolutionary process, say in the case of a stellar system, are those of material bodies (stars) which are of the order of one ten-thousandth of that of light. Consequently, if we wish to construct a causal connection between the commencement of the expansion and events which are supposed to have happened at a very early stage of the evolution of the stellar systems—such as the first formation of condensations, or the imprisoning of free energy inside matter, called ' stagnation ' by Lemaître—we unavoidably meet with the difficulty that the time elapsed since these two beginnings is some thousands of times longer in the one chain of events than in the other. I do not think it will ever be found possible to reconcile the two time scales.

We thus, however reluctantly, come to the conclusion that the expansion of the universe on the one hand, and the evolution of stellar systems and stars on the other hand, are two different processes, taking place side by side, but without any apparent connection between them. The expansion has only been going on during an interval of time which is as nothing compared with the duration of the evolution. Leaving the

oscillating universes, and those that start from a zero radius, out of account, the universe may have been practically stationary at or very near its minimum size for an infinite time before starting to expand, or it may have contracted during an infinite time and after passing through a minimum a few thousand million years ago started to expand again. In both cases there appears to be no causal connection between the change of size of the universe as a whole and the evolution of the systems which it contains.

It should not be forgotten that all this talk about the universe involves a tremendous extrapolation, which is a very dangerous operation. I have sometimes called the part of the universe of which we know anything with certainty 'our neighbourhood.' The limits of this neighbourhood have been enormously extended in the last ten or fifteen years by the results derived from observations with the large telescopes at Mount Wilson and elsewhere, but still it is presumably only a very small portion of the whole of the universe. All assertions regarding those portions of the universe which lie beyond our neighbourhood either in space or in time are pure extrapolations. In making a theory of the universe we must, however, adopt some extrapolation, and we can choose it so as to suit our philosophical taste. But we have no right to expect it to be confirmed by future observations extending to parts now outside our reach. The extrapolation that is at the base of the theory of the expanding universe is that our neighbourhood is just an ordinary point, or small area, in the universe, not differing from any other small area in any essential property.

This is, of course, a very natural hypothesis to make, and I do not see how we can avoid making it, but it remains a hypothesis and an extrapolation. It involves, of course, the assertion—the axiomatic truth of which can hardly be doubted by any physicist—that the laws of nature remain the same at all times and all places. The observed fact is that the spectral lines in the light which reaches us from bodies at a great distance are displaced towards the red, or, in other words, that *light is reddened by age*. Now the only interpretation of a reddening of light consistent with the accepted laws of nature in a homogeneous world is a receding 'velocity' of the source. This is the observational evidence of the 'expanding universe' in a nutshell.

The dilemma that we are in has some similarity with that by which atomic physics was confronted some twenty years ago, when the necessity became apparent of ascribing to the atom properties which in a finite material body would be contradictory. The concept of the universe as a closed entity is, so far as I can see a hypothesis, an arbitrary addition to, and extension of, the observed phenomena by our imagination. We must be prepared to allow this 'Universe' the freedom to have contradictory properties, like we have been forced to grant to the atom, and in particular we must try to accustom ourselves to the idea that the velocity of change of the quantity of the dimension of a length, which occurs in the equations, and is interpreted as the 'Radius of the Universe,' has nothing to do with the rate of evolutionary change of stars and stellar systems.

It seems to me that the current interpretation, and the consequent model of the universe as an expanding closed hypersphere, may be found to be too simplistic, and may be displaced in time by one in which the

apparent contradictions are more satisfactorily hidden from view. But this does not affect the theory, which will retain its value independent of the interpretations put upon it.

Prof. Sir ARTHUR EDDINGTON, F.R.S.

Discussion of detailed theories of stellar evolution is overshadowed by the fact that the time-scale is once again in the melting-pot. I think it will be agreed that if Prof. de Sitter is right in the facts he has put before us indicating a rapid expansion of the universe or scattering apart of the galaxies, the very long time-scale of billions of years which has been fashionable of late becomes exceedingly incongruous; we should have to accept an age of the order 10^{10} years for the galaxies, and presumably also for the stars. But the theory of the expanding universe is in some respects so preposterous that we naturally hesitate before committing ourselves to it. It contains elements apparently so incredible that I feel almost an indignation that anyone should believe in it—except myself. I have had a special reason for believing it which I have referred to from time to time; but it was not until last month that I was able to put it into definite shape. I believe that from pure physical theory we can not only predict that this phenomenon of expansion will occur, but also predict the actual rate of expansion; and the calculated result agrees with the observed recession of the nebulae. This result comes out of the wave equation for an electron—the fundamental equation of modern quantum theory. When I adapt the wave equation to take account of the curvature of space I find that it ought to contain a term $\sqrt{N}/R$, that is to say, the square root of the number of electrons in the universe divided by the radius of the universe in its equilibrium state.

I do not suppose that this is a new term to be inserted as a correction to the ordinary equation; it is already in the equation in disguise. It is the term attributed to the mass of the electron and ordinarily written mc^2/e^2. I think that the President of our section (Sir J. J. Thomson) was the first person to measure the mass of an electron. It is safe to say that he did not realise in 1897 that the thing he was after—the constant which was responsible for the effects in vacuum tubes attributed to mass—was the square root of the number of electrons in the universe divided by the radius of the universe. Really he was poaching on astronomical preserves. He was finding the rate of recession of the spiral nebulae, or at least a very little calculation will derive it from his measures.

I take the value of $\sqrt{N}/R$ (or, as he mysteriously called it, mc^2/e^2) according to measurements by him and his successors, and combine it with well-known formulae of the relativity theory which Prof. de Sitter has been describing; then I can find at once the principal data about the size of the universe. For example, its original radius was 1,070 million light years before it started to expand. Also $N = 1{\cdot}29 \times 10^{79}$. And, what is of special interest, the rate of recession of distant objects can be calculated; the result in the usual units (km. per sec. per megaparsec) is 528. This is the whole expansion effect which will be reduced a little by the attraction of the galaxies on one another, but the reduction is not likely to be large. The value found from astronomical observation by various investigators ranges from 430 to 550.

Naturally, this close accordance of theory and observation has made me believe that both are right, and that the observed motions of the nebulae are genuine ; so that we must accept this alarmingly rapid dispersal of the nebulae with its important consequences in limiting the time available for evolution.

Prof. Robert A. Millikan.

Anyone who knows me is quite aware of the fact that I have no qualifications for participating in a discussion of the evolution of the universe, unless perhaps it be because of my interest and activity in the development of our knowledge of the cosmic radiation. Since, however, results in this field now seem destined to exert a profound, if not a determinative, influence upon all theories of stellar evolution, it may not be out of place for me to outline the present status of our experimental findings in it, and to do what I can to show whither they point.

I note first, however, that the opening up of this amazing new field of knowledge is the work solely of the experimentalist. Plentiful as theorists have always been, especially in astronomy, and confident as they have always been in their conclusions, not one of them, so far as I know, who speculated about the nature of the universe, ever predicted cosmic rays, or even dreamed of their existence—certainly not sufficiently definitely to suggest any experiments actually to bring them to light. Prior to 1910 not a trace of evidence had appeared that such rays existed. They had not even been seriously proposed. Apart from a passing suggestion by O. W. Richardson,[2] in 1906, that electroscope-discharge effects observed on earth might possibly have something to do with solar influences—a suggestion quickly negatived by the fact that these effects are as strong at night-time as in day-time—I can find no record of the existence anywhere up to 1910 of any ideas even remotely related to those of the cosmic rays. Indeed, all the work that had been done prior to 1910, even on rays capable of discharging electroscopes through metal walls centimetres, or even inches, thick (so-called penetrating rays), was generally interpreted in terms of earth-rays, or of radioactive emanations getting from the earth into the lower atmosphere, and these are, in fact, responsible for much the greater part of the then observed penetrating rays.

In 1909 all the work that had appeared in this field up to that date was reviewed by Kurz,[3] and careful consideration given to each one of the only three possible origins of the observed effects, namely : (1) the earth, (2) the atmosphere, and (3) the regions beyond the atmosphere. The last two were definitely discarded, and the conclusion drawn that there was not the slightest evidence for the existence of any penetrating rays other than those produced by radioactive substances in the earth—this with full knowledge, too, dwelt upon at length in this very article, that half a mile of the earth's atmosphere would absorb all such radioactive radiations.

When, therefore, in that same year the experimentalist, Gockel,[4] took

[2] Richardson, *Nature*, **73**, 607 ; **74**, 55 ; 1906.

[3] Kurz, *Phys. Zeit.*, **10**, 836 ; 1909 : see also Millikan, *Nature*, **126**, 14 ; 1930, for historical studies.

[4] Gockel, *Phys. Zeit.*, **11**, 280 ; 1910 : also **12**, 597 ; 1911.

an electroscope three different times in a balloon to heights which reached four and a half kilometres and found that its rate of discharge was there even higher than on the earth, he had discovered something new and important, namely, that although there are penetrating rays that do originate in the earth and are indeed abundantly given off from practically all the elements of the earth's crust, as Kurz and the other workers prior to 1910 had rightly concluded, yet there must be other rays, abundant at high altitudes, that come in from above, originating either in the remoter regions of the atmosphere or else coming in from outer space; in other words, that one or the other of the two alternatives which Kurz had explicitly considered and definitely discarded had been incorrectly set aside for at least some rays that actually exist. Which one of these two, namely, upper atmosphere or outer space, it took a great deal of work by Hess, by Kolhörster, by v. Schweidler, by Bowen, Otis, Cameron, myself and others, from 1910 to 1925 definitely to determine, and even in 1927, at the Como conference, one of the most distinguished of living physicists declared himself still a believer in the theory of an upper atmospheric origin.

To-day, however, I think the cosmic origin has been generally conceded, and with that concession it follows from the measurements themselves, not only that in the particular portion of our galaxy immediately around us, the energy carried by the cosmic rays is at least a tenth[5] of that existing in the form of radiant heat and light, but also, since these latter radiations must be diminished greatly in intergalactic space, that the energy carried by the cosmic rays throughout the universe is of the same order of magnitude as, possibly greater than, all other radiant energies combined. In the light of this fact, when one reflects that the second law of thermodynamics, which has, strangely, been thought by some so determinative for theories of the origin and destiny of the universe, and which may be roughly said to be merely a generalisation of the fact always observed *here on earth* that all forms of energy tend to become converted into heat and then to be radiated away from the earth and hence lost to us, one sees *how prone we are to make sweeping generalisations upon insufficient knowledge.* This is why the experimentalist has played and always will play so important a rôle in the progress of science. From the very inception of the experimental method he has continually been bringing to light facts which were not within the theorist's ken even when that theorist had got observational phenomena pieced together, *as he thought,* into a beautifully consistent and 'necessarily related' scheme. With perhaps the largest source of radiant energy as yet unconsidered, may it not possibly be that the thermodynamic theorist has gone too far in his dicta about the origin and destiny of the universe? This is my excuse for forgetting, at least for the moment, all about theories, and asking first: What are the experimental facts in the field of cosmic rays?

There are three main facts that now seem to be quite well established, though discussion is still rife about some of them. I hope, however, that some of the new data that I am now able to present will help to bring about better agreement.

[5] Millikan and Cameron, *Phys. Rev.*, **31**, 930; 1928.

(1) The first fact is the complete uniformity in distribution of the cosmic rays within the present limits of experimental error in the measurement of their intensity. This has been disputed by some, but I should like to show how I have been testing it this summer. I have the results of a continuous month of observation of the intensities of the cosmic rays taken through four daily six-hour periods as follows: 6 a.m. to noon, noon to 6 p.m., 6 p.m. to midnight, and midnight to 6 a.m. None of the means of intensities in these four periods differ by so much as one-half of one per cent. The barometer which, as Cameron and I first showed in 1925,[6] influences markedly these intensities, was extraordinarily constant during this summer month in Pasadena, scarcely varying 3 mm. either way from the mean throughout twenty-five days; the barometer readings go through a small minimum each afternoon, due to currents set up by the sun's heat, and a small maximum early each morning. The cosmic rays, on the other hand, go through a small afternoon maximum and an early morning minimum, thus showing quite conclusively, I think, that the minute variations in cosmic ray intensities are not due to radiations from the sun, which is shining during both maxima and minima, but rather to slight variations in the atmospheric blanket induced by the alternate heating and cooling of the gaseous matter through which the rays reach the earth. Further evidence for the correctness of this conclusion is found in the fact that the period from midnight to morning, during which the atmosphere is more quiet than is the case in any of the other three periods, shows the smallest variations in the individual readings, thus appearing to indicate that these, too, are due to atmospheric disturbances. The conclusion is consonant, too, with the latest, very exact measurements of Hoffmann[7] at Halle, though the conditions in central Europe are less favourable for testing the especial points brought to light in my own work.

This independence of the cosmic ray intensities of the positions of the sun, the Milky Way, or other celestial objects is the most amazing property of these rays, since it seems to show quite definitely that those portions of the universe—better, those directions in the celestial dome—in which matter is most abundant, such as the directions of the sun or the Milky Way, have no advantage over those directions in which matter is very scarce. I can see no possible escape from the conclusion that the conditions existing in those portions of the universe where matter is abundant, that is, in and immediately about the stars, are inimical to the formation of cosmic rays. This in itself bars out the likelihood of their being due to the annihilation of protons, provided such annihilation is to be called upon, in accordance with the demands of most modern astronomers, for maintaining the temperatures of the stars. In making this statement I am eliminating as scientifically unacceptable the hypothesis that in bygone ages these rays were created by processes no longer existing and have since been wandering like lost souls about a universe from which there is for them no possibility of escape. Further evidence on this point will be presently given.

[6] Millikan and Cameron, *Phys. Rev.*, **28**, 856; 1926.
[7] Hoffmann, *Zeit. für Physik*, **69**, 703; 1931.

If, then, the cosmic rays are forming *now*, there is no place of origin left except *outside the stars*, in the interstellar spaces, where both temperature and pressure are exceedingly low. This is not so unlikely a place of origin as it was a few years ago, before Bowen[8] at the Norman Bridge Laboratory had solved the seventy-year-old riddle of the nebulium lines and proved that the common elements oxygen, nitrogen, carbon, and sulphur, as well as hydrogen and helium, exist out there, giving rise to these nebulium radiations *in regions which may be lightyears away from the exciting stars.*

(2) The second most significant cosmic ray fact is that which, after some less precise tests by Cameron and myself in 1926, was brought to light most unambiguously just a year ago when, by taking very careful and very exact readings with the same sensitive electroscope, first at Pasadena, lat. 34° N., then at Churchill, Manitoba, lat. 59° N., the nearest settlement on earth to the north magnetic pole, I proved that the incoming cosmic rays are not influenced at all by the earth's magnetic field, and drew from that fact the inevitable conclusion that when these rays enter the earth's atmosphere they have not previously traversed any amount of matter which is comparable with the thickness of that atmosphere, or else they would of necessity be mixed with secondary beta rays which would be directed into the earth most abundantly along the earth's magnetic lines and therefore enter it in the neighbourhood of the magnetic poles. These experiments furnish another independent and, I think, very beautiful proof that the rays must originate in interstellar space, for if they came from even the remotest exteriors of stars, they would have to be appreciably mixed with magnetically deflectable beta rays. The fact that they are not so mixed seems to me to hit the annihilation theory a second fatal blow, for there is no sort of reason for assuming that annihilation takes place *only* in interstellar space. Such an assumption would destroy the whole purpose of the annihilation hypothesis, which up to the present has been to furnish the requisite energies to the stars.

Before I leave this point (No. 2), let me combine the experimental fact of uniformity of distribution (No. 1) with this other fact that the rays enter the atmosphere unmixed with deflectable beta rays. The uniformity of distribution could indeed be reconciled with the annihilation hypothesis if it could be assumed that, in looking out from the earth into the celestial dome, one were always looking through a quantity of matter equivalent to, say, a hundred metres of water, which is a thickness sufficient to absorb 99 per cent. of all incoming cosmic rays. That one would actually encounter one-thousandth part as much matter as this in going out to infinity through interstellar space seems to be contrary to all the other astronomical evidence. But let us forget this and see what the cosmic rays alone have to say about this point. If annihilation is going on in all matter independently of temperature, then so far as the cosmic rays coming to the earth's atmosphere are concerned, every element of the celestial dome would be like every other element, even if the sun were in one of these elements. This is the only way the annihilation hypothesis of the origin of cosmic rays can be reconciled with their uniformity of distribution. But in this case the whole of the cosmic ray beam entering the earth would

[8] Bowen, *Astroph. Jour.*, **67**, **1**; 1928.

be a beam in complete equilibrium with its whole train of secondaries, including the deflectable beta rays, and these latter would of necessity spiral about the earth's magnetic lines and thus enter the earth only near the earth's magnetic poles, a behaviour which clashes with fact No. 2. Since, then, no trace of such an effect is actually found, the cosmic rays alone deny the validity of this hypothesis and with it of this form of the annihilation hypothesis as to their origin. It seems to me, therefore, that as an explanation of facts about cosmic rays the annihilation hypothesis fails at every point at which one can test it. I shall comment upon still another of its failures in the next section.

(3) The third well-established and most significant fact of cosmic rays is that *they have a banded structure.* Cameron and I first brought this fact sharply to light in 1925,[9] but I do not think the evidence is yet clearly understood or its significance fully appreciated. The evidence goes back to the high balloon flight[10] with self-recording electroscopes which Bowen and I made in 1922, a flight in which we reached an altitude of 15½ km. and got 92 per cent. of the distance to the top of the atmosphere as measured by the weight of the earth's atmosphere left beneath our instruments. *In other words, remembering that the atmosphere is the equivalent of* 10 *metres of water, we were within* 80 *centimetres of the top.* Now, 80 centimetres of water allows at least 3 per cent. of rays as soft even as those of thorium C″ to pass through it. We made this high flight on purpose to determine whether the rate of increase of ionisation within a closed electroscope continued exponentially to the top of the atmosphere as Kolhörster's earlier balloon flight to a height of 9 km. had indicated was the case up to that altitude. Further, Cameron and I have since, with precision instruments, obtained quite as high ionisation readings up to 5 km. as those given by Kolhörster, thus checking sufficiently his exponential curve in the lower stretches of the atmosphere ; but the 15½ km. balloon flight not only failed to do this, but it showed definitely and altogether unambiguously, since all possible sources of error would have increased, not decreased, our readings, that with increasing altitude the ionisation fails to maintain its rate of increase but drops back toward lower values, just as it should do if a band of about 25,000,000-volt pure photons enters the atmosphere and requires a considerable distance of penetration into it before it gets into equilibrium with its secondaries. This furnishes a third bit of independent evidence that the cosmic rays do enter the atmosphere as practically pure photons, and hence that they originate in interstellar space.

The high balloon flight shows much more than this. It proves conclusively that neither gamma rays of energy 2,500,000 volts, like those from thorium C″, nor rays of intermediate penetrating power up to that of the softest cosmic rays, which pass through about five times as great a thickness of water and have an energy of about 25,000,000 volts, enter the earth's atmosphere in amounts appreciable in comparison with that of this softest cosmic ray band ; for any such abundant rays would have

[9] Millikan and Cameron, *Phys. Rev.*, **28**, 851 ; 1926 : see also *Phys. Rev.*, **37**, 244 ; 1931.

[10] Millikan and Bowen, *Carnegie Institution Year-book*, **21**, 385 ; 1922 : also *Phys. Rev.*, 22, 198 ; 1923, and **27**, 353 ; 1926.

very rapidly discharged our electroscope when it got within 80 cm. of water of the top of the atmosphere. In other words, there is a definite and strong cosmic ray band of penetrating power about five times (energy ten times) that of the hardest gamma rays of thorium. The analysis of this band on its other side, that is, on its short wave-length side, into much lower intensity bands but of energies of the order of 100,000,000 volts and 180,000,000 volts respectively, has been made by Cameron and me,[11] though these results are less certain. *The existence, however, of a very strong cosmic ray band at about* 25,000,000 *volts, fairly sharply limited on both the long wave-length side and the short wave-length side, is altogether definitely established by our experiments.* This band carries of the order of 90 per cent. of the total cosmic ray energy entering the atmosphere.

Now, the experimental fact of the existence of such a band is only interpretable, so far as I can see, by the assumption of some kind of an energy-emitting atomic transformation of definite energy-releasing value taking place continuously throughout interstellar space. The other suggestions which have been made to account for a band having such energy can, as it seems to me, be quite definitely eliminated. There are but two of them. First, some kind of a cosmical electrical field has been postulated of sufficient total potential difference to impart the necessary 25,000,000 volts to electrons falling through it. There is indeed known to be a symmetrical field surrounding the earth, *but its value is definitely known to be about one million volts and its direction is such as to drive electrons out, not in ; in other words, it has one twenty-fifth the requisite strength and is of the wrong sign.* No potential difference of 25,000,000 volts, symmetrical with respect to the earth and positive at its surface, can possibly be assumed without, as I think, colliding head-on with other well-established facts ; and even if such a field existed, it would produce a more or less continuous spectrum, like the general X-ray spectrum, instead of a well-marked band. I cannot see any way of making this suggestion reproduce even remotely the experimental situation.

The second effort has been to find a way by which rays due to the annihilation of protons (which are at least thirty-five times too energetic) could be softened down to the observed values. The most interesting of these suggestions has been made by Dr. Zwicky, who seizes upon the nebular red shift recently discovered by Hubble to suggest that if at the creation, which he estimates from the maximum possible amount of matter in interstellar space would have been about 10^{19} years ago, annihilation rays began suddenly to be formed, and if these same rays are still hurtling about in a spherical universe and are subject to the red shift, these rays, with all that have been created since, would now constitute a spectrum having a definite limit to its frequency on the long wave-length side, but falling off very slowly in intensity on the short wave-length side, where the limit would be 25,000,000 volts. In my opinion, this is the only conceivable mechanism by which annihilation rays could become reduced in energy to that observed on the long wave-length side of the band which carries nine-tenths of the energy of the cosmic rays ; but the shape of the spectrum on the other side is completely wrong, since it represents a

[11] Millikan and Cameron, *Phys. Rev.*, 37, 235 ; 1931.

very slowly falling intensity with increasing frequency, instead of a fairly sharp, well-marked band such as experiment reveals.

No other known mechanism can soften an original beam of photons of the gamma ray type. If such a beam is in the least inhomogeneous, our universal experience is that it is hardened, not softened, by passage through matter. If, on the other hand, the original beam is monochromatic, it is also at first apparently hardened in the process of getting into equilibrium with its secondaries, and when that condition has been attained it has regained its original absorption coefficient. If annihilation rays maintain the temperatures of the stars at all, the mechanism by which they do it is not that of a continuous degradation of wave-length until the wave-lengths corresponding to heat and light waves are reached. The process is rather that the penetrating power of the beam of photons is maintained *until all of these photons have been picked off by Compton encounters* and complete absorption of the beam has thus been brought about, the temperature of the matter traversed having been thus slowly raised by this straight attrition process. Our third fact, then, of a banded structure in the particular region in which the chief cosmic ray band is found, appears to me to be the last arrow that pierces the heart of the already twice fatally wounded annihilation hypothesis of their origin.

Also, I should like to present one more reason why the hypothesis of cosmic electrical fields as an agency for directly imparting energies of from 25,000,000 volts to, say, 400,000,000 volts to electrically charged particles cannot be admitted. It is not only that the most fantastic assumptions would have to be made to make such fields produce cosmic ray bands of the type observed, but also, more than that, to make fields of any such intensities symmetrical with respect to the earth and thus account for the uniformity of distribution of cosmic rays would involve, as it seems to me, something very like a return to the geocentric theory of the universe—a return scarcely acceptable to any scientific worker who has lived since A.D. 1500.

With cosmic electrical fields and the annihilation of protons both completely unacceptable, what is then left to furnish the energy, first, of the great cosmic ray band which carries 90 per cent. of the energy of these rays? The answer seems to me to be as follows:—If the Einstein equation $E=mc^2$, and the actual facts of isotopes, as accurately worked out first by Aston, are taken as guides, then this answer is unambiguous. But first, how dependable is this Einstein relation? Note first that it is a purely thermodynamic equation, stating merely energy relations. In using it, therefore, we are completely independent of any assumption as to the nature of the cosmic rays. Whether they are photons or high-speed charged particles is, so far as it is concerned, quite immaterial. Again, as to its dependability, I think that most physicists would say that it is about as safe a guide as any theoretical equation which we now have in physics. It not only rests on excellent theoretical foundations, but also it has predicted quantitative relations which have stood the tests of all the careful experiments which we have as yet been able to apply to it. *It states with entire definiteness that there is no atomic transformation whatever that can furnish the necessary energy except an atom-building process.*

I have thus far reached, merely by a process of exclusion, the conclusion that the cosmic rays are the wireless signals of the building in interstellar space of some at least of the heavier elements out of the lighter. The evidence, however, is very much better than that. It is reasonably quantitative in the case of the main cosmic ray band. Here again theory and experiment support one another. For, if atom-building takes place at all, there is one such atom-building act that is more fundamental than all others, and that also must take place more frequently than all others, namely, the formation of helium out of hydrogen, because we have abundant evidence that all the elements are actually built out of hydrogen and helium, and that helium is built out of four atoms of hydrogen, so that the hydrogen-to-helium transformation should take place much more frequently than any other. The energy of this transformation is computed from Einstein's equation and Aston's measurements at 25,000,000 volts: just what I stated above to be the energy of the large cosmic ray band that carries the great bulk of the cosmic ray energy entering the atmosphere. But the way in which I arrived at that figure requires some explanation. Most simply stated, the method used was to compare directly in the waters of high altitude lakes the penetrating power of the cosmic rays there found with the penetrating power of the hardest known monochromatic gamma rays, namely, those of thorium C″, which have an energy of about 2,500,000 volt-electrons. *This I have done directly, the observed ratio being between* 6 *and* 12.

The comparison cannot be made directly with great precision, because the cosmic rays are not homogeneous, but when the inhomogeneities are sifted out into bands (and this does not need to be done with great precision, almost any reasonable kind of sifting being satisfactory, since the softest band carries so large a fraction of the total energy) the penetrating power of this softest band comes out very close to five times that of thorium C″. Further, at altitudes between 6 km. and 9 km., where the harder components should exert almost no influence, according to the observations of both Hess and Kolhörster in their manned balloon flights, the directly measured penetrating power of the cosmic rays was six times that of the hardest gamma rays. This checks most satisfactorily with our analysis of our curve. The best formula we now have (the Klein-Nishina) connecting penetrating power and energy then makes this energy come out ten times that of thorium C″, or about 25,000,000 volts. This Klein-Nishina formula has been directly proved by Bowen and myself, as well as by others, to fit the facts up to rays of the hardness of those of thorium C″ reasonably well, and the extrapolation from there up to the softest cosmic ray band is not likely to be very badly in error. Indeed, this whole procedure may be looked upon merely as the extrapolation, to a not unreasonable distance, of an *experimental* curve, and thus as largely independent of the Klein-Nishina formula or indeed of any theory. At any rate, the foregoing is, in my judgment, the only quantitative test of the energy of any portion of the cosmic ray spectrum that has any sort of significance. For to use the Klein-Nishina formula to compute the energies of the rays, not five times but 200 times as penetrating as those of thorium C″, as some who have sought to make the cosmic rays proton-annihilation rays have done, seems to me to be extra-

ordinarily rash. If my extrapolation is not significant, theirs must represent that lack of significance raised to a very considerable power.

Let me not, however, overstress the precision of this quantitative comparison. *It is scarcely necessary to do so, since there is no other energy-releasing act of this order of magnitude that can produce this soft component of the cosmic rays.* There are, to be sure, other *atom-building* acts which might produce as soft or softer rays, and some of these may well be taking place, such, for example, as the formation of carbon out of helium, but a consideration of the *abundance* of the elements involved shows that such rays would in general be of negligible intensity. The fact that there are but some five *abundant* elements is here a powerful guide.

So far I have dealt only with the formation of helium out of hydrogen. Cameron and I have shown that our complete cosmic ray curve is consistent with the existence of three other bands corresponding to the three other abundant groups of elements which we have called the oxygen group, the silicon group, and the iron group, but the evidence was here necessarily qualitative in view of two recently demonstrated cosmic ray facts. The first is that referred to above, namely, the evidence that the rays come into the earth's atmosphere as practically pure photons. For this means that we may not assume that the harder rays have reached a condition of equilibrium with their secondaries at the points at which we measure them.

The second is that interesting new fact discovered by Bothe and Kolhörster[12] a couple of years ago: that the penetrating power of the secondary beta rays may apparently, with increasing energy, rise up to, or possibly surpass, the penetrating power of the original photons. This seems to me to furnish the most simple explanation of the fact brought to light by Regener, as well as by Cameron and myself in our work at great depths in water, namely, that there is a very small component of the cosmic rays of intensity of the order of 1/500 of that of the whole cosmic ray beam as it enters the atmosphere, which has a penetrating power thirty-five or forty times that of the main cosmic ray band of which we have been speaking. The hardest rays of appreciable intensity that should be formed by the atomic building process are those of iron, and their energy should be about seventeen times that of the helium rays. There are, indeed, more penetrating atom-building rays that might be formed, but the abundance of the elements corresponding to them is so small that these rays should be exceedingly feeble—probably too feeble to make the formation of these heavy elements out of hydrogen a good theory as to the origin of the hardest component of the cosmic rays.

There is, however, already some little evidence, which we hope soon to complete and extend, that the penetrating power of these very hard rays increases more rapidly than does their energy, and in the present state of our knowledge this is the best working hypothesis to account for this one point which might raise doubts about the completeness of the atom-building explanation of the cosmic rays. If, however, it might be assumed that proton-annihilation does not take place in the stars, but does take place, *with* atom-building, in interstellar space, then proton-building might

[12] Bothe and Kolhörster, *Zeit. für Physik*, **56**, 751; 1929.

perhaps be called upon to account for this 1/500th part of the cosmic rays which possess this great penetrating power. In other words, if one prefers to go no further than to seek an answer to the question, What processes are able to take place in Nature that are appropriate for supplying the energy actually found in the cosmic rays? then the answer is, atom-building taking place in interstellar space for supplying more than 99·5 per cent. of the rays, and proton-, or nucleus-, annihilation for supplying the remainder. If we assume them both, then the energy-conditions represented in Einstein's equation can easily be satisfied.

The reasons, however, for such proton-annihilation in interstellar space are not so cogent as are those for atom-building. These latter reasons, apart from the direct experimental evidence, may be stated thus. We know that all the atoms are actually built up out of hydrogen, and it is therefore natural to assume, even apart from direct experimental evidence, that somewhere or other the process is going on now. Secondly, Bowen has shown that the reason so-called forbidden spectroscopic lines appear in the nebulae and not on earth is that, given a long enough time free from collisions, an atomic change will take place that cannot take place where atomic collisions are frequent. It is not a dissimilar hypothesis, and one concordant with modern wave mechanical thinking, too, that a cluster of four hydrogen atoms *free from collisions for a long enough time* will jump over a potential wall and find themselves together in the nucleus. The reason this does not take place in the atmospheres of the stars, or even on earth, is presumably that temperature and density, that is, energy and frequency of collisions, destroy the clusters or prevent altogether their formation. But at low enough temperatures, under the influence of ordinary molecular forces, these clusters *must* form; for what is liquefaction other than the process of formation of such larger molecular groups? It is such clustering, combined with freedom from energetic collisions, which, according to the hypothesis herein presented, provides the necessary condition for occasional atom-building. It might conceivably take place in the very remote regions of our atmosphere, but in that case it should also take place in the remote regions of the sun's atmosphere; and the cosmic rays should then be much stronger during the day than at night, a result contrary to fact. Atom-building *in interstellar space* is therefore a natural enough hypothesis. Proton-annihilation, on the other hand, must take place, if at all, either within the nucleus or through the rushing of an enormously energetic electron into the nucleus. These acts ought to be either independent of temperature or else facilitated by it. The first cannot be true if cosmic rays are to be explained by annihilation, else the sun would be enormously influential. The second may be true, and annihilation therefore confined to the interiors of stars as has been generally assumed. It is therefore very unnatural, if not quite impossible, to assume annihilation to take place in interstellar space and *not* in the atmospheres of the suns. This hypothesis, therefore, I should not wish to make until every other avenue of escape from our difficulties has been closed. If the ideas presented herewith prove correct, they will obviously influence strongly not only present theories but also all future theories of the origin and destiny of the universe.

Rt. Rev. the Lord Bishop of Birmingham, F.R.S.

I will begin by briefly recapitulating the theory of the evolution of the universe in the form in which, as I understand, it at present exists.

In the beginning a large unbounded finite three-dimensional universe with space of very small positive curvature was filled with highly diffused matter of very small density. The matter began to aggregate into masses of approximately equal size, spread fairly uniformly throughout the space; and the whole space began, at some epoch or another, to expand. The masses attracted neighbouring matter and somehow acquired velocities of rotation which increased as the matter in them condensed. Finally, incredibly vast bun-shaped aggregates, spinning too quickly for the stability of their outer edges, began to throw off drops, as it were. The drops congealed into suns, and ultimately the aggregates became the spiral nebulae which now exist. Each of them, apart from possible central regions of diffuse matter, consists of thousands of millions of stars. Our sun is a star of no particular importance, belonging to a spiral nebula or group of nebulae called the Galactic universe. That universe came into existence some five million million years ago.

Since they first existed, stars in the various universes have moved aimlessly under the influence of their initial velocities and mutual attractions. Periodically, but rarely, at intervals of tens or hundreds of millions of years, collisions between suns in the various universes have taken place and planetary systems have been born. So the earth came into existence some two or four thousand million years ago. Thus ours is quite possibly one of the youngest planetary systems in *our* universe. On the cooling earth primitive forms of life appeared at least a thousand million years ago; and gradually, by a slow evolution, more highly developed living organisms arose. Finally, about a million years ago sub-men emerged from a group of anthropoid apes.

If I personally am critical of this picture, I plead that we must not confuse speculative possibility with satisfactory demonstration. I am concerned that we do not give arguments to obscurantists, who claim that the scientific theories of one generation are usually repudiated by the next. So I would begin by emphasising that in the group of possibilities and probabilities just outlined there is much less certainty than, say, in the facts upon which Darwin rested his conclusions when the *Origin of Species* was published seventy-two years ago.

I need not refer to the prejudiced opposition by which Darwin was assailed. Of course, his triumph has been signally complete. But others before him had put forward theories of the evolution of terrestrial animals. What Darwin did was so to accumulate and arrange biological facts, that experts were convinced that evolution by the mechanism of natural selection had produced from primal organisms the vast range of living things upon the earth, including man himself. Since Darwin wrote, further investigation and discovery have confirmed his insight. Some of his subordinate opinions were erroneous; but his main scheme stands intact because his facts were correct. All fresh geological and embryological investigation confirms the conclusions on which he rested. The scanty remains of primitive man that are discovered from time to time

accord with expectation ; and such statistical investigations as those in R. A. Fisher's recent volume are a triumphant vindication of the potency of natural selection.

It is worth while recalling these facts when we consider the picture of the evolution of the universe which has emerged from recent work. I personally doubt whether the time has yet come for an astrophysicist of genius to write a book which shall in its own sphere rival the *Origin of Species*. We can point to a few new facts and to considerably more new (and occasionally discordant) theories. Out of them there has emerged the present picture, immensely exciting, but by no means certain.

What of it can we regard as certain ? First of all, there is no reason to doubt the existence of island universes. Such form that vast, fairly regular distribution of spiral nebulae through the depths of space which is revealed by photographs taken in the great telescopes. We can say with fair certainty that our own galactic universe is either a single spiral system or an aggregate of several such, each analogous to millions of others with which space is strewn.

Secondly, I would say that it is fairly certain that our space is finite, though unbounded. Infinite space is simply a scandal to human thought ; and, though we must not assume that the universe was made that man might understand it, the alternatives to finite space are incredible. We cannot accept the idea of island universes succeeding one another indefinitely as we pass in imagination through the depths of space. Such a distribution does not accord with a Euclidean-Newtonian gravitational scheme, for it would lead to infinite gravitational potentials. Neither can we with equanimity think of a vast finite group of universes forming a sort of island in empty space. Ultimately such a group ought, one would surmise, to aggregate into a single mass. But in Riemannian spherical space we can have a finite and uniform distribution of universes, inasmuch as such space is unbounded so that every point is related to the whole as is every other point. Finally, there is no fact of observation to set against the belief that space has a very small positive curvature.

Thirdly, I think we must accept as highly probable Jeans's hypothesis that in the stars matter is actually destroyed as protons and electrons unite to form radiation. To this conclusion we are driven by failure to find any satisfactory alternative explanation of the vast output of energy by the stars. If, however, their lives were not to be measured by so long a period of time as millions of millions of years, the necessity for assuming the actual destruction of matter would not arise.

Consider now some of the uncertainties and difficulties which belong to the present scheme. First of all, of course, there is the insoluble difficulty of infinite time. No man of science will postulate a supernatural intervention, a stirring of the uniformly distributed matter filling space with which in imagination the present scheme begins. Yet, in default of such a beginning, we must imagine an infinite regress, a never-ending sequence of alternate periods of world-building and world-destruction, the rise and fall of universes without end. In comparison with such a past the future is perhaps less perplexing, though it is not very satisfying, because the second law of thermodynamics seems to necessitate an end when all energy will have so run down that nothing happens anywhere

to break a deadly uniformity. Of course, we can escape from the difficulties caused by infinite time if we accept the opinion entertained by some philosophers that time is not real. With them we may hold that the notion of time is due to the nature of our perception. If we accept such an idea, we can assert that consciousness in its passage through the space-time continuum meets but does not cause events. It then follows, however, that all our fancied activities are an illusion. Against any such belief we must put what surely is our constant and invariable experience. We *have* a measure of freedom of will. Labour and struggle *are* real. We *can* get nearer truth and overcome evil as we strive for goodness. Thus I am forced to conclude that the time-process must be real. Yet, unless time is an illusion, or unless alternatively the cosmos had a beginning in time, any picture of the evolution of the universe must fail to satisfy.

Let us take it, however, that the primal mist that filled all space in the beginning aggregated into masses of roughly equal size in a finite universe and that these masses slowly began to condense and revolve. Whence came their rotation ? To this question I can find no satisfying answer. Let us ignore the difficulty. Out of condensation and rotation came the spiral nebulae, the universes of thousands of millions of stars with which space is strewn. Obviously, with relatively few exceptions, the great nebulae are built to a common pattern. They are results of rotation, and the picture of vast rotating lenticular masses throwing off stars, like drops at the edge of a fly-wheel, satisfies the imagination. But why are the arms of the spirals so short ? We should expect arms which curl repeatedly round the central nucleus. None such exist ; and yet there is apparently no ejection into space of early-born stars to account for the disappearance of the coils of stars that ought to be visible.

The different chains of reasoning by which Jeans has been led to assign a period of millions of millions of years for the age of our universe seem to me to demand respect. I doubt whether they are conclusive. But I wish that there were certain knowledge of the development and decay of typical stars. Theories abound. Some are magnificent in the ingenuity and in the intellectual power which have gone to their making. But the final theory of stellar evolution has not emerged. I confess, moreover, that I am by no means happy with regard to the expanding universe ; and I doubt whether Doppler's principle is rightly applied to measure the velocities of recession of the great nebulae. It is not improbable that the reddening of their light is due to other causes. If the great nebulae are moving away as fast as is suggested, we are lucky to be living at an epoch when we are able with our telescopes to see them at all. Moreover, the Friedman-Lemaître equations give contraction as an equally possible alternative to expansion. May it not be that a velocity of approach is masked by some other effect ? A universe that was continuously contracting would have a snug end. Steady and continued inflation, either of a currency or of a universe, is disquieting.

It is, however, when we come to the formation of planetary systems that I feel especially uneasy. The current theory is, as I have said, that the earth and its associated planets arose from the encounter of our sun with a wanderer which came so near as to disrupt it some few thousand

million years ago. If the theory be true, planetary systems must be rare and therefore consciousness, as men know and possess it, is rare. In fact, the existence of consciousness, when it occurs, will be the by-product of an accident. We are then apparently forced to conclude that the universe was not created with the primary object of producing beings in whom mind should lead to spiritual excellence.

There is, of course, no reason why consciousness should be associated with animals such as ourselves who represent transformations of carbon compounds at temperatures between the boiling and freezing points of water. It might, for all we know to the contrary, be associated with changes in the ionisation of atoms or with the disintegration of their nuclei at temperatures of hundreds of millions of degrees. But of any such bases for the appearance of mind we have no knowledge. What we do know with certainty is that throughout the universe the raw material of which it is made is fairly uniform. The matter in distant stars is the same as that which exists in our own sun. We must then assume that there are planetary systems in distant island nebulae, and that on some of them conditions resemble those which exist on our earth. So life, as we know it, must be distributed throughout the universe; but, if the collision theory of planetary origins is correct, the distribution is astonishingly sparse.

I do not, of course, suggest that there are human beings on other planets. The direction of the physical and physiological evolution of living things upon our earth would seem, if we can judge by the geological record, to have been somewhat erratic. Particular mutations coincided with particular conditions of environment to determine the direction of change at any instant. But, throughout the known geological process there has been large-scale progress, a possibly unsteady but quite definite development of mind. In the possibly very different living organisms of other planets there will have been a progressive development of mind. Our physical structure matters little in comparison with the kind of consciousness which it carries.

We can then, as it seems to me, assume the existence throughout the universe of conscious beings. If it be true that our earth and all planetary systems similar to our own originated in a chance collision of suns, life elsewhere must be as a rule unimaginably more developed than with ourselves. Also planets carrying living organisms must be incredibly rare. After a life history of five million million years our galactic universe will have but one sun in a hundred thousand with satellites which can carry life. Such extravagant world-building for such meagre results leaves one dubious as to whether the theory is correct. In defence of such doubts as are forced upon me I might point out that the origin of our moon, with its exceptional density and massiveness, has not been finally settled. The theory that it was broken from the earth when the latter was mainly liquid owing to a chance 'resonance' phenomenon Jeffreys, in a recent paper, deems untrue. Even present estimates of the age of the earth, in so far as they depend on the rate of disintegration of uranium, puzzle us because we do not know why there should have been any uranium in the earth at its birth. Thus I, personally, should not be surprised if new facts were forthcoming to give some other explanation

of the existence of planetary systems. I suspect that such systems are much more numerous than is at present believed.

I need scarcely emphasise that the issue raised by the relative frequency of planetary systems is of great philosophical importance. From Judæa, through the Christian Church, has come a belief in ethical theism which has been a strong and ennobling influence in European civilisation. To-day such belief rests upon the conviction that we can only explain the universe by assuming that it is due to creative thought and will, associated with purpose and plan. Such purpose appears most clearly on earth in the progressive development of mind, which has ended in the recognition of moral values by humanity; and the religious outlook of many of us is determined by our belief that God has thus created man for His service. But, if consciousness should be proved to be but a rare accident of a vast, otherwise aimless, universe, such belief in God would be encumbered by a new perplexity. I may add that the belief would not disappear, since we should still have to explain why man has been created with the conviction that he must be loyal to goodness and truth.

Such philosophico-religious speculation is, however, premature. We need more facts and, that we may obtain them, we need new instruments of greater power and precision. The interferometer, we may hope, points the way to instrumental triumphs in the future. If only an instrument could be invented which should enable us to determine whether stars, within, say, a hundred light-years' distance, have planetary systems attached to them! We should then know whether any of the few thousand stars near the sun have planets on which life may conceivably exist. If even one such system were found, the present theory of planetary origins would collapse. Failing any such invention of a super-telescope, there remains the possibility of wireless communication. As I have already indicated, I have no doubt that there are many other inhabited worlds, and that on some of them beings exist who are immeasurably beyond our mental level. We should be rash to deny that they can use radiation so penetrating as to convey messages to the earth. Probably such messages now come. When they are first made intelligible a new era in the history of humanity will begin. At the beginning of the era the opposition between those who welcome the new knowledge and those who deem it dangerously subversive will doubtless lead to a world-war. But the survivors, when they extricate themselves from the economic consequences of the peace treaty, will begin what we may correctly term a strenuous correspondence course. I should like to be living then. We might get a true understanding of the evolution of the universe.

Gen. The Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S.

I have been kindly invited to say a few words in this debate, with special reference to the philosophical bearings of the subject. I could have wished that a really competent philosopher had been selected for this task, as I am equally innocent of philosophy and mathematical physics. In a sense my Presidential address dealt with some aspects of the evolution of the universe, but a good deal more could be said, if there were time, about the philosophical issues which are raised by this subject.

I do not agree with those who say that the recent advances in physics have no important value for philosophy. The most creative philosophical thinkers in the past have as a rule been saturated with the science of their time, which gave substance and body to their philosophy. And it is only to be expected that the recent revolutionary advances of physics are bound to have the most profound effect on our world-view and on our philosophical outlook. How, for instance, could philosophy, which has for thousands of years speculated on the nature of time and space, be unaffected by the fruitful integration of the two which has been effected by the physicists in our time? Again, the concept of the quantum, with its peculiar behaviour, its holism, its indeterminacy, is bound to be far-reaching for philosophy no less than for physics. Of course, it is extremely difficult at this early stage, when we are at the beginnings of these changes, and physical theory is still in a state of flux, to say what exactly will be the philosophical outcome of the new physics. But I have little doubt that the revolution in physics will yet be followed by a revolution in philosophy, and that in their joining of forces a new era will open up for both science and philosophy.

So far as philosophy is concerned, one is at present perhaps more impressed with the difficulties and perplexities, arising from the new concepts in science, than with its solid results. Thus, one is inclined, from the relativity standpoint, to attach the greatest importance to the new space-time concept. If the old forces of nature, like gravitation, and perhaps even electro-magnetism, are (as Einstein teaches) nothing but curvatures of space-time, if matter itself is really only such a curvature, one feels inclined to look upon space-time as the basic structure of the world, and as no mere mathematical symbolism. Space-time becomes something like the old ether, a substratum or matrix from which all the physical differentiations have taken place. To such a view one is inevitably led by the Relativity theory and its results. And yet the next moment, when one considers the behaviour of the ultimate physical units, especially the electron and the quantum, one meets with phenomena in flagrant contradiction with the idea of space-time, as if for the electron and quantum space and time really do not exist, as if space-time is a macroscopic result rather than an ultimate basic feature of the cosmos. If space-time is merely a statistical macroscopic result, there seems to be some flaw in the fundamental relativity treatment which makes it basic to everything in the world of existence. We seem to be making for a real clash between the relativity and the quantum concepts, unless we have to admit that both are still provisional, and that a wider, truer unification or reconciliation is still to come.

I could refer to other difficulties and perplexities arising from the new physics, but as time is short I pass on to another point which has perhaps a closer bearing on our topic of discussion. I wish to refer to the peculiar character of the ultimate physical units and their bearing on the evolution of the universe. Many physicists, including even a profound thinker like Sir Arthur Eddington, maintain the view that exact science, and physics in particular, is confined to the metrical aspects of the world and has nothing to say as to the nature of the universe. If this is really so, then how could Sir J. Jeans say in a recent famous book of his that the universe

as viewed by the new physics is more like a great thought than a great machine ? This seems to go far beyond the metrical characters of the world. If physics were confined simply and solely to the metrical aspects of the world, then how can Sir A. Eddington himself find intimations of freewill in the law of indeterminacy ? Truth is a whole, and the truth of physics will be found to link on and to be but part of that larger truth which is the nature and the character of the universe.

When we speak of the evolution of the cosmos we are faced with a series of questions in regard to the ultimate physical constituents of the world and their interplay, the routes they follow, the structures they form, and the resulting characters, size, mass and properties of the universe as a whole. These are problems for physics. But we find that that evolution also comprises the emergence of life and mind, of the human soul and human personality, and a whole new world of values of all sorts. Truth, Beauty, Goodness and Love are as much structures of the evolutionary universe as the sun and the earth and the moon. But when we come to scrutinize the ultimate foundations of this vast superstructure we naturally look for the characters which would in the long run render such enormous developments possible. This would be in particular the attitude of the philosopher, with his wider outlook over the realms of existence. And it is for him that from this point of view the new quantum physics has a peculiar fascination. For he finds that the physicists, working merely with their own tools and their own incomparable technique, and looking for no more than the metrical units which subtend this universe, have indeed, like the man who looked for asses and found a king, found much more than they have looked for. The units they have discovered will constitute not only a world of physics but, in the end and at far removes, also a world of life and spirit.

For these units, particularly the electron and the quantum, have an almost metaphysical aspect ; they are physics infected with thought ; they are not in space-time, they behave as wholes, they are indeterminate in their behaviour, so that the law of chance rather than the ordinary casual law applies to them. Of course, it is possible that this ambiguous character is due to infection from the mind of the researcher, as if the mental instrument of research has affected the result. But it is far more likely that in this dual character of the physical units we reach an ultimate basic fact, that we transcend the mind-matter distinction and reach the bedrock where no such differentiation has yet arisen. Thus it comes that the ultimate units are not purely physical or material, but point to an undifferentiated primitive world matrix, which includes both the physical and the thought characters of the world.

When from these units we start our process of world building, we find at first what is apparently merely a physical universe. But gradually the suppressed vital and mental elements, inherent in the universe from the start, begin to emerge. Cosmic evolution is thus found to include organic evolution, and this again gives rise to the evolution of spirit, of social and spiritual values, which form our own human phase of this cosmic process.

The origin of life from matter, of mind and personality from both, has always seemed an unresolvable mystery. The philosophy of

emergence which has recently arisen, and which purports to explain the creative rise in evolution, the coming of life and mind and spirit—this philosophy has appeared to be based on a miracle or a series of miracles. The quantum physics has come to the relief of this school, which surely teaches a significant truth. According to quantum doctrine the roots of life and mind lie imbedded deep down in the ultimate structure of this universe, and they are not mere singular apparitions of an unaccountable character, arising accidentally in the later phases of evolution.

To me the significance of the new physics for philosophy seems to lie particularly in this linking of the deep down beginnings with the most advanced achievements of the universe. The apparent huge gaps in evolution, requiring a miracle of leaps at more than one stage, are thus shown to be a gross exaggeration. We see the universe gradually pushing to the front certain features and characters which have been there all the time from the very beginning, although in most primitive and scarcely recognisable form.

One concluding remark. We cannot say that the universe has been built up from these units or any ultimate units in the course of its evolution. We can only say that these are the ultimate elements which we find on analysis. The universe may for ever have been a complex affair, a closely knit structure of some sort or other as the law of Entropy would seem to indicate. It may even have been one vast quantum, as the Abbé Lemaître suggests. But in the evolution of this mass there has been relative movement of parts or features; some have pushed more to the fore with time, and there is no doubt that life and mind are features that have thus emerged from the mass, with the increase of entropy.

M. l'Abbé G. Lemaître.

I propose to give some answer to the two questions raised by Sir James Jeans, which so clearly summarise the present state of the problem of the evolution of the universe. I will begin with the second question, because I think that its solution may throw some light on the first one: "Is the universe expanding at about the rate indicated by the spectra of the nebulæ,' the atomic constants not being modified by some artificial change of gauge? I add these words, because it is clear that any artificial expansion could be provided by arbitrarily varying the units of length, time, and mass. Expansion of the universe is in some sense relative: it is relative to the whole set of essential properties of matter being assumed to be constant.

The expansion of the universe is a matter of astronomical facts interpreted by the theory of relativity, with the help of assumptions as to the homogeneity of space, without which any theory seems to be impossible. I shall not discuss the legitimacy of this interpretation, as I do not know any definite objection made against it and this is not the place; and it is not necessary to give a new popular version of the leading principles of the theory of relativity. I shall rather try to show that the universe must be expanding, or rather that the most necessary processes of evolution are contradictory to the view that space is and has always been static.

It has been pointed out by Sir Arthur Eddington that a static universe is unstable, and he proposed the problem of finding the possible causes of its expansion. He suggested that such a cause might be the formation of condensations. I obtained recently a solution of this problem, and the main results are as follows:

When the expansion is already started, the effect of kinetic energy or pressure of radiation is quite negligible. On the contrary, pressure is the chief factor in the question of instability of a static universe. If the pressure were rigorously zero, the expansion could never appear. But, if the pressure (or kinetic energy) is not zero, any diminution of pressure must start the expansion. For example, a world full of radiation starts expanding as soon as the radiation can transform itself into matter.

When condensations exist or are formed, the problem is complicated by gravitational effects; but it can be shown that the general expansion of the universe depends entirely on the density of kinetic energy or of pressure at the places where the gravitational influences of the condensations cancel one another. I call these places (for brevity) 'neutral zones.' Condensation in itself has no direct effect whatever on the stability of the universe: but condensations would necessarily induce a rarefaction at the neutral zone and so a diminution of the density of kinetic energy at the neutral zone; and this must induce expansion.

We can conclude that any general process of condensation, occurring in a world where the kinetic energy does not vanish, must induce expansion. Therefore, practically, the expectation of Sir Arthur Eddington is fully confirmed. For example, formation of stars out of a primeval gas starts the expansion; formation of extra-galactic nebulæ out of a uniform mass of gas or of stars starts the expansion. I think that these results add much weight to the fact that the actual velocity of expansion fixes a limit to the time scale of the evolution, as we must rule out of our speculations every process which would start a premature expansion of space.

Even if we had no experimental evidence of the expansion of space, considerations of stability would fix a limit to the time-scale of evolution. The reason is that, if the universe has existed for too long a time, any general process of condensation would be contradictory to the actual value of the density of matter. Although this quantity is not known with great accuracy, its value may give some idea of the maximum scale of evolution. I find that any general process of condensation, even of very moderate intensity, cannot have happened earlier than a few hundred thousand million years ago.

As stated by Sir James Jeans, this brings almost complete chaos into the already chaotic problem of stellar evolution. A complete revision of our cosmological hypothesis is necessary, the primary condition being the test of rapidity. We want a 'fireworks' theory of evolution. The last two thousand million years are slow evolution: they are ashes and smoke of bright but very rapid fireworks.

I think that the key of the problem is afforded by the discovery of the cosmic rays. Cosmic rays are of enormous energy. Their intensity is estimated to be about one-tenth of that of the light coming to us from the stars. If these rays are really cosmic, their energy is much bigger than that of the light of the stars, because it must be of equal intensity all over

space. Simple computation shows that the energy of cosmic rays is comparable in amount to the whole energy of matter, being possibly one thousandth, and at least one hundred thousandth, of the total energy of matter.

If the cosmic rays originated chiefly before the actual expansion of space, their original energy was even bigger, and it has been reduced by the expansion in the ratio of the change of the radius of the universe during the time of their transmission through free space. We get photographs of nebulæ at a distance of about one hundred million light years; light from these nebulæ travelled through space during about one hundredth of the time of expansion. It does not seem improbable that the cosmic rays have travelled around one hundred times longer and were really produced by the process of the formation of the stars. This may give the solution of the puzzle. The only energy we know which is comparable to the energy of the cosmic rays is the matter of the stars. Therefore it seems that the cosmic rays must have originated from the stars.

Now the stars are surrounded by an atmosphere, and an atmosphere would altogether prevent any escape of cosmic rays from the inside of a star. The explanation seems to be that the cosmic rays went off from the stars at a time when the stars had no atmosphere. The stars are born without atmosphere; the atmosphere evolved after the escape of the cosmic rays.

We are thus led to the conclusion that the stars were born some ten thousand million years ago without atmospheres, and that the cosmic rays are outstanding features of the formation of a star.

How could we explain such a phenomenon as that? Sir James Jeans has given strong reasons for admitting the existence of atoms of considerably higher atomic weight than our actual dead atoms. Cosmogony is atomic physics on a large scale—large scale of space and time—why not large scale of atomic weight? Radioactive disintegration is a physical fact, cosmic rays are like the rays from radium. Have they not escaped from a big scale super-radioactive disintegration, the disintegration of an atomic star, the disintegration of an atom of weight comparable to the weight of a star?

The birth of a star would be an atom of weight somewhat greater than the actual weight of the star, and the star would be formed by the super-radioactive disintegration of its original atom. It is conceivable that the greater part of the products of disintegration would be kept back together by the gravitational attraction of such a massive atom, although a considerable part, say one thousandth, should be able to escape into free space at the beginning of the evolution, before the products of disintegration are numerous enough to form an atmosphere. Cosmic rays would be glimpses of the primeval fireworks of the formation of a star from an atom, coming to us after their long journey through free space.

The frequency of cosmic rays is, of course, very high; nevertheless, it may be thought to be too low to be the by-product of such a tremendous disintegration of matter. However, it must be realised that the observed frequency of the cosmic rays is not the original frequency. The original frequency has been reduced in the ratio of the expansion of space, and was at least twenty times greater than the observed frequency.

I think that a possible test of the theory is that, if I am right, cosmic rays cannot be formed uniquely of photons, but must contain, like the radioactive rays, fast beta rays and alpha particles, and even new rays of greater masses and charges. I have shown that the momenta of such rays must be reduced by the expansion in about the same ratio as that of the photons.

The mass of a star should be determined by the weight of its original atom, and it is conceivable that stars might be born with different masses. If the mass of the original atom is too big, the star must be finally broken up by radiation pressure, and the original atom must give birth to a cluster of stars, chiefly formed by stars of maximum mass. If the star comes from an atom, both masses and luminosity are determined by the weight of the original atom. Thus this theory accounts for a mass-luminosity relation.

The actual theory does not completely bring order into the chaotic state of cosmogony imposed by the fact of the expansion of space. Explanation of the approximate equipartition of energy between the stars, or evolution with loss of mass along the Russell diagram, might be dismissed with regret. But I do not see any way to retain these processes of evolution, because they are altogether too slow.

If I had to ask a question of the infallible oracle alluded to by Sir James Jeans, I think I should choose this : 'Has the universe ever been at rest, or did the expansion start from the beginning ?' But, I think, I would ask the oracle not to give the answer, in order that a subsequent generation would not be deprived of the pleasure of searching for and of finding the solution.

If the total time of evolution did not exceed, say, ten times the age of the earth, it is quite possible to have a variation of the radius of the universe going on, expanding from zero to the actual value. I would picture the evolution as follows : At the origin, all the mass of the universe would exist in the form of a unique atom ; the radius of the universe, although not strictly zero, being relatively very small. The whole universe would be produced by the disintegration of this primeval atom. It can be shown that the radius of space must increase. Some fragments retain their products of disintegration and form clusters of stars or individual stars of any mass. When the stars are formed, the process of formation of the extra-galactic nebulæ out of a gaseous material, proposed by Sir James Jeans, could be retained for the star-gas filling the space. The numerical test works out equally well for this case.

Whether this is wild imagination or physical hypothesis cannot be said at present, but we may hope that the question will not wait too long to be solved.

We want two things. First, a theory of nuclear structure sufficient to be applied to atoms of extreme weights. For these atoms, the problem cannot be separated into a nuclear problem and a problem of surrounding electrons ; because it is easily seen that the K ring would merge into the nucleus. We must wait, but we may trust the physicists that we do not have to wait too long. The second thing we want is a better knowledge of the nature of the cosmic rays. The correlation of the theory of nuclear structure with further observations on the cosmic rays must answer yes or no to our question ; and we shall prefer this answer, however incomplete it may be, to any answer of any infallible oracle.

Sir Oliver Lodge, F.R.S.

It is well known that a physical theory which ignores some of the elements of the problem is incomplete, and is therefore liable to break down when confronted with the facts. A physical theory cannot take the whole universe into account; but if it is to be complete enough to be satisfactory and to make trustworthy predictions, it must take all relevant factors into account.

Sir James Jeans began his discourse by saying that in this Section we were concerned only with the physical universe, that is with material bodies and the forces that act upon them. I suppose that is true. But the fact that it is true seems to make it impossible for this Section to enter upon a philosophical discussion of such a subject as the universe as a whole, and to decide its fate upon purely deterministic lines. For the universe certainly contains more than we deal with in this Section. We must remember that there are Sections D and K and I and J; that is, we must realise that the universe is not solely inorganic. Some of the matter is animated; and although it is still obedient to the laws of physics and chemistry, an animated body behaves in a spontaneous manner not predictable by those laws. When a thing behaves as if it were alive, physics loses interest in it, and hands it over to another Section; for it is incompetent to deal with motions attributable to spontaneity and free will. Live things are excluded from our instruments, and charwomen from our laboratories. Wherever life has entered in, the predictions of physicists and astronomers and mathematicians are liable to be spoilt. Laplace's calculator might reckon the behaviour of every particle in the universe so long as it was not interfered with by life and mind. But purely physical evolution would not anticipate or predict the occurrence of life and mind.

I have looked sometimes at the ripples coming over the sand on a sea beach and leaving a deposit of foam. I have thought whether a mathematician, given sufficient data, could predict every ripple and every line of foam. Yes, he could, theoretically, provided there were no boats, nor any fish. The splash of a fish, the ripples of a boat, would put his calculations out. Given even a spark of free will, there are no data that can be supplied. It may be said that our sense of free will is an illusion. Well, that is a philosophical question that can be raised. But it cannot be settled in this Section. So I venture to think that before we can philosophise upon such a theme as the ultimate fate of the universe, we must be able to take everything into account, and philosophise with a very wide and comprehensive knowledge of reality.

Maxwell showed how the effect of mind could be introduced into the scheme of physics without contravening any of the laws of energy, except the purely statistical second law of thermodynamics. His demon, by dealing with the particles individually and selectively directing them, could interfere with and neutralise the consequences supposed to be deduced from that law. I claim as a physicist that too much attention has been paid to this second law of thermodynamics, and that the final and inevitable increase of entropy to a maximum is a bugbear, an idol, to which philosophers need not bow the knee.

It is doubtless instructive to learn from high and competent authorities what the unadulterated or rather unvivified laws of physics applied to the universe will lead to. We are faced with a steady running down or degradation of energy to a predetermined end: without hope of novelty introduced at any stage of the process, all settled and dull, events just going through the hollow form of taking place. But that is all on the assumption that there is nothing or no one to wind it up or to guide it to some nobler end. Guidance has only recently intruded itself into the scheme of physics; but already there are guiding waves which determine the path of a particle of matter. And what the significance of those guiding waves may be, whether they have any connexion with the guiding and directing influence which we perceive here displaying itself in certain observed phenomena that we call life and mind, is at present an unanswered question.

To philosophise from a restricted point of view is interesting enough, but it is not conclusive. It does not fully account for the state of the world to-day, nor can it be depended on to formulate its course to-morrow.

In this beginning of a new century for the British Association we are turning our attention from the particles of matter to the spaces between them, where all the activity resides. We have thus, with Einstein, begun a new era, but have not gone far as yet, but when we attend to space properly we shall find that life and mind are not limited to the surface of lumps of matter. Intelligence can be found throughout space. Life and mind, in their essence, are more important and far more permanent than any material universe; they use it only for purposes of development and manifestation. I look forward to the future and see the Association, always seeking after truth, being led on to a psychical and physical immaterial reality of which in its present corporate capacity it has no idea.

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References to addresses, reports, and papers printed in extended form are given in italics.

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